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Articles by René Lemarchand, James C. Scott, Isaac Kramnick, Arnold Kanter, Gordon S. Black, Thomas W. Casstevens



Robert Axelrod

Where the Votes Come From: An Analysis of Electoral Coalitions, 1952-1968

Austin Ranney

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Interest Groups and Political Integration: British Entry Into Europe

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CONTENTS

- 11 Where the Votes Come From: An Analysis of Electoral Coalitions, 1952-1968
Robert Axelrod
- 21 Turnout and Representation in Presidential Primary Elections
Austin Ranney
- 38 Decision Making in Soviet Cities
B. Michael Frolic
- 53 Interest Groups and Political Integration: British Entry Into Europe
Robert J. Lieber
- 68 Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities
in Nation-Building
René Lemarchand
- 91 Patron-Client Politics and Political Change in Southeast Asia
James C. Scott
- 114 On Anarchism and the Real World: William Godwin and Radical England
Isaac Kramnick
- 129 Congress and the Defense Budget: 1960-1970
Arnold Kanter
- 144 A Theory of Political Ambition: Career Choices and the Role of Structural
Incentives
Gordon S. Black
- 160 The Committee Function: An Influence Equation
Thomas W. Cassievens
- 64 APSA Presidential Address: To Nurture a Discipline
Robert E. Lane
- 83 Communications
- 88 Editorial Comment
- 191 Book Reviews

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ARTICLES

- 11 **Where the Votes Come From: An Analysis of Electoral Coalitions, 1952-1968.** This paper describes how the contributions that different groups make to a party's total strength can be specified by breaking down each contribution into its three components—size, turnout, and loyalty. Through the use of survey data on reported vote rather than party identification, the voting coalitions of the Democrats and Republicans are analyzed. By means of examples of selected groups, the actual magnitude of these contributions and their components are presented for each of the last five presidential elections. Major attention is given to a group-by-group evaluation of the generally accepted view that the Democrats are a coalition of diverse overlapping minorities including the poor, Blacks, union members, Catholics and Jews, Southerners, city dwellers and perhaps the young; and the view that the Republicans have appeal for the corresponding non-minorities. The empirical results show which aspects of these views are valid and which are not. Some implications for the party system as well as the strategic considerations inherent in this approach are also discussed.

By ROBERT AXELROD, Assistant Professor of Political Science, University of California, Berkeley.

- 21 **Turnout and Representation in Presidential Primary Elections.** This paper provides a partial test of the widely-held assumption that preference primaries are the most representative element of the presidential nominating system. It notes that the average voting turnout in presidential primaries is only 39 per cent, compared with 69 per cent in the same states' ensuing general elections. The representative quality of these electorates is examined with data from sample surveys of the 1968 New Hampshire and Wisconsin primaries. A comparison of the primary participants with non-participants among each party's identifiers shows, first, that presidential primary electorates are demographically unrepresentative of their nonvoting fellow partisans in age, education, income, and social status. Second, primary participants have no more intense party identifications than do non-participants. Third, participants hold issue positions more strongly than nonparticipants do, and on some issues they even hold positions contrary to those of nonparticipants. Finally, both of the Democratic primaries overrepresented Johnson sentiment and underrepresented McCarthy and Kennedy sentiment. The author concludes that the preference primaries' claim to be the most representative element of the presidential nominating system may not be warranted, especially if and when the national conventions are reformed along lines such as those laid down by the McGovern-Fraser Commission. In any case, 1972 offers a unique opportunity to study the comparative representativeness of local, state, and national party conventions and the preference primaries.

By AUSTIN RANNEY, Professor of Political Science, The University of Wisconsin, Madison

- 38 **Decision Making in Soviet Cities.** How are decisions made in Soviet cities? Who are the municipal decision makers? What kinds of decisions do they make? Is there a Soviet urban political system? This article attempts to answer these questions by focusing on four aspects of decision making in Soviet cities: budget formulation, the planning process, housing construction and allocation, and the staffing of key municipal posts.

Urban autonomy has increased in the past decade, but Soviet municipalities are very much restricted in their decision making. Superior Party and governmental authorities continue to dominate the decision-making process and any decision made by municipal authorities can be vetoed by superior Party and governmental organs.

Soviet municipal decision making is now being influenced by three tendencies: municipal administrators are acquiring more influence in municipal government and administration; the educational qualifications and professional expertise of city Party members are rising; *ad hoc* citizen and group interest articulation may be developing. Comparisons between Soviet and North American urban decision-making models are useful and valid, although they require an improved methodology and much more Soviet data.

By B. MICHAEL FROLIC, Associate Professor of Political Science, York University.

- 53 **Interest Groups and Political Integration: British Entry into Europe.** This article analyzes the influence of interest groups in the formation of British policy toward participation in European unity. From the important theoretical literature bearing on the subject, it derives and tests two hypotheses relating group behavior to the political integration process. The hypothesis imputed to the group politics approach holds that if interest groups influence policy formation, then progress toward

integration is likely to be impeded. The hypothesis suggested by an extension of the functionalist approach implies that if interest groups influence policy formation, then progress toward integration is likely to be facilitated. The data indicate a confirmation of the first hypothesis and a rejection of the second. Groups were unfavorable toward European unity for both organizational and economic reasons. They restrained Britain's movement toward participation in integrated European ventures until the onset of effective politicization. Then the introduction of broadly conceived national interest considerations displaced cost-benefit calculations as the criteria for judgment and diminished the groups' influence. At least in the task of enlarging a geographic area of integration, if not in expanding that integration once a grouping already exists, a conscious political decision was found to be essential.

By ROBERT J. LIEBER, Assistant Professor of Political Science, University of California, Davis.

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- 68 **Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation-Building.** The concept of political clientelism is one of the few genuinely crosscultural concepts available to political scientists for the comparative study of transitional systems. As a descriptive concept, political clientelism helps us uncover patterns of relationships which deviate markedly from those ordinarily associated with class or ethnicity. As an analytic concept political clientelism provides crucial insights into the internal dynamics of social and political change. Moreover, if, as some contend, patterns of resource allocation are more meaningful indicators of political development than their conceptual opposites, political clientelism may well supply the critical "missing link" between micro- and macro-sociological or system-centered theories of political development.

By RENÉ LEMARCHAND, Professor of Political Science, University of Florida.

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- 91 **Patron-Client Politics and Political Change in Southeast Asia.** The analysis presented here is an effort to elaborate the patron-client model of association, developed largely by anthropologists, and to demonstrate its applicability to political action in Southeast Asia. Inasmuch as patron-client structures are not unique to Southeast Asia but are much in evidence, particularly in Latin America, in Africa, and in less developed portions of Europe, the analysis may possibly have more general value for understanding politics in preindustrial societies. After defining the nature of patron-client ties and distinguishing them from other social ties, the paper discriminates among patron-client ties to establish the most important dimensions of variation, examines both the survival and transformations in patron-client links in Southeast Asia since colonialism and the impact of major social changes such as the growth of markets, the expanded role of the state, and the creation of local regimes. Finally, the paper shows how patron-client bonds interact with electoral politics to create distributive pressures which, in turn, often lead to inflationary fiscal policies and vulnerability of regimes to losses of revenue.

By JAMES C. SCOTT, Associate Professor of Political Science, University of Wisconsin.

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- 114 **On Anarchism and the Real World: William Godwin and Radical England.** Much that is characteristic of contemporary anarchist thought can be found in the writings of the founder of that tradition, William Godwin. He is ambivalent on the value of technology and modernity, nostalgic at one moment and progressive another. He extols individual autonomy while preaching community solidarity. Above all he shares with modern anarchism an elitist disdain for ordinary men and women, which in the case of Godwin leads to an unresolved tension between theoretical radicalism and practical conservatism. His anarchist doctrine repudiates all forms of coercion, law, and government. It eschews cooperation of all kind as deleterious to individual development. At the same time it posits an ideal order characterized by a high degree of informal coercion practiced by zealous neighborly inspection. But Godwin was a less than eager friend of reform and agitation during the years of Pitt's repression of radical movements in England. This was clear even in the pages of *Political Justice*, but all the more obvious in his feud in 1795-96 with John Thelwall and the London Corresponding Society, the leading radical activists. This paper outlines Godwin's anarchism and points out the implications of his dispute with Thelwall. In addition it shows the extent to which Godwin has given anarchism certain of its enduring qualities.

By ISAAC KRAMNICK, Associate Professor of Political Science, Yale University.

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- 129 **Congress and the Defense Budget: 1960-1970.** It is commonly held that Congress has made only inconsequential changes in the President's requests for defense appropriations. Scholarly studies of

the budgetary process, notably the work of Fennò and Wildavsky, do not include defense spending in their analyses. For nondefense spending they find that Congress has primarily a *fiscal* rather than *programmatic* orientation toward appropriations.

House and Senate changes in the President's defense appropriations requests were analyzed for the fiscal years 1960 through 1970. It was found that small Congressional changes in total defense spending obscured more substantial activity in particular budget categories. The concentration of Congressional activity in two of these categories—Procurement and R&D—indicates that Congress has had a significant programmatic orientation toward defense spending. Changes in the level of Congressional activity across time confirm this hypothesis.

Efforts to measure the relative influence of the House and Senate proved inconclusive. The Senate dominated the conference committee, but the Senate's influence was concentrated in the least important budget categories. The House made most of the changes in the President's budget requests, but very few of such changes were appealed by the Defense Department to the Senate.

By ARNOLD KANTER, Assistant Professor of Political Science, The Ohio State University.

- 144 **A Theory of Political Ambition: Career Choices and the Role of Structural Incentives.** Officeholders periodically face the problem of choosing among a set of career alternatives, and these alternatives customarily include the choice of dropping out of political life, or seeking reelection, or of choosing to seek higher office. This paper assumes that officeholders behave according to a rational calculus in making such choices, and that the main elements involved in the choice process include the probabilities and values attached by the candidate to his alternatives, and the investments required to obtain these alternatives. Political ambition, or the desire to seek higher office, is shown to develop as a product of the investments that politicians make in their political careers, and the investments are shown to be associated with the structural characteristics of community size and electoral competitiveness. The subjects of the research are 435 city councilmen from 89 cities of the San Francisco Bay Region, and the data include information derived from interviews with the councilmen and aggregate election data collected on each city.

By GORDON S. BLACK, Assistant Professor of Political Science, University of Rochester.

- 160 **The Committee Function: An Influence Equation.** In legislative systems a bill is commonly considered and reported by an appropriate committee before it is considered on the floor of the house. Since motions on the floor frequently relate to such bills, it is often apposite to refer to a motion's relevant committee. This article presents a mathematical model of the relationship between the relevant committee's divisions on the floor and a motion's probability of passing. Let x be the proportion of the relevant committee voting yea on the floor, z be the proportion of the relevant committee voting nay on the floor, and y be the proportion of the relevant committee neither voting yea nor voting nay on the floor, then a motion's probability of passing is $(x^2 + y^2)/(x^2 + y^2 + z^2)$. The fit between theory and observation is quite good: six hundred eleven roll calls from the 90th and 91st Congresses have been analyzed; 0.628 of the motions actually passed; and 0.613 of the motions were expected to pass.

By THOMAS W. CASSTEVENS, Associate Professor of Political Science, Oakland University.

- 164 **APSA Presidential Address: To Nurture a Discipline.** Discipline based associations have gone through two stages: "learned society" and "professional association." Because of the inadequacies of the market for ideas and talent, it is time for a third stage or model, "science management." Certain characteristics of the discipline affect the utility of this model, including the level of training (relatively modest), the resources available to the discipline (lower than other social sciences), and disciplinary morale and cohesion (relatively high).

There has been an increase in doctoral programs in political science at the rate of about three new programs per year for the past twelve years. Many of these new programs are of doubtful quality, but they may be useful to the discipline in terms of teacher training (although less useful for research training), if their doctorates replace persons with the master's degree, and if they add to, rather than substitute for, a full complement of persons trained by the older quality programs. Regionalization of doctoral programs is opposed, and some characteristics of quality programs are stated: relatively large faculties and student enrollments, and high proportions of research oriented full professors. Discipline wide enrollment policies should be from "the top down" in terms of quality of the programs.

Creative political science can be identified and encouraged by attention to its constituent elements, conditions, and the selection of students identified by measures of creative potential. Creative research productivity is more related to motivation than to intelligence (beyond a certain level); a productive research career is directly and strongly related to the quality of a

person's graduate program and the quality of his employing institution, as well as to the eminence of his dissertation sponsor. Individual and collective research productivity has increased; the consequent information explosion requires more systematic information exchange and a policy of encouraging the most fruitful research agendas for the discipline.

By ROBERT E. LANE, Professor of Political Science, Yale University.

183 COMMUNICATIONS

From Gerald H. Kramer, Theodore S. Arrington, David Nexon, W. Scott Thompson, Douglas W. Rae and Michael Taylor, Kenneth A. Shepsle.

188 EDITORIAL COMMENT

BOOK REVIEWS

91 Political Theory, History of Political Thought and Methodology

- Black, *Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430-1450*. Paul E. Sigmund, p. 191
 Hobbes, *A Dialogue Between a Philosopher and a Student of the Common Laws of England* (edited by Joseph Cropsey). J. Roland Pennock, p. 192
 Jorin and Martz, *Latin American Political Thought and Ideology*. Edward J. Williams, p. 193
 Kruskal, ed., *Mathematical Sciences and Social Sciences*. Oliver Benson, p. 194
-

95 American Government and Politics

- Adams, *The Growing Church Lobby in Washington*. R. Wallace Brewster, p. 195
 Beck, ed., *Law and Justice: Essays in Honor of Robert S. Rankin*. John R. Schmidhauser, p. 196
 Bollens, Bayes and Utter, *American County Government: With an Annotated Bibliography*. Samuel K. Gove, p. 197
 Burton, *Democrats of Oregon: The Pattern of Minority Politics 1900-1956*. Lester G. Seligman, p. 198
 Domhoff, *The Higher Circles: The Governing Class in America*. Earl Latham, p. 199
 Feingold, *The Politics of Rescue: The Roosevelt Administration and the Holocaust, 1938-1945*. Morton J. Tenzer, p. 201
 Fitch and Walsh, eds., *Agenda for a City: Issues Confronting New York*. Andrew Hacker, p. 203
 Haveman and Margolis, eds., *Public Expenditures and Policy Analysis*. Walter G. Held, p. 204
 Hinckley, *The Seniority System in Congress*. George Goodwin, Jr., p. 205
 James, *The Years of MacArthur. Vol. 1: 1880-1941*. Dale Pontius, p. 206
 Kurland, ed., *Felix Frankfurter on the Supreme Court: Extrajudicial Essays on the Court and Constitution*. D. S. Hobbs, p. 207
 Lindbergh, *The Wartime Journals of Charles A. Lindbergh*. Alexander DeConde, p. 208
 Marcus, *Grand Old Party: Political Structure in the Gilded Age, 1880-1896*; and Yearley, *The Money Machines: The Breakdown and Reform of Governmental and Party Finance in the North, 1860-1920*. Hugh Douglas Price, p. 210
 Merelman, *Political Socialization and Educational Climates: A Study of Two School Districts*. Pauline Marie Vaillancourt, p. 211
 Navasky, *Kennedy Justice*. Nelson W. Polsby, p. 212
 Nimmo and Oden, *The Texas Political System*; and Anderson, Murray and Farley, *Texas Politics: An Introduction*. Joseph L. Bernd, p. 214
 Ries, *Executives in the American Political System*. Lloyd A. Rowe, p. 215
 Russett, *What Price Vigilance? The Burdens of National Defense*. Kenneth E. Boulding, p. 217
 Tyler, *The Political Imperative: The Corporate Character of Unions and The Labor Revolution*. Melvin A. Kahn, p. 218
 Wasby, *The Impact of the United States Supreme Court: Some Perspectives*. Twiley W. Barker, p. 220
 Wright, *Federal Grants-in-Aid: Perspectives and Alternatives*. Parris N. Glendening, p. 221
-

222 Comparative Government and Politics

- Allen and Johnson, eds., *African Perspectives: Papers in the History, Politics, and Economics of Africa Presented to Thomas Hodgkin*. Sheldon Gellar, p. 222
 Anderson, *The Nordic Council: A Study of Scandinavian Regionalism*. Joseph B. Board, p. 223
 Backer, *Priming the German Economy*. F. A. Breier, p. 225
 Chao, *Agricultural Production in Communist China 1949-1965*. Ramon H. Myers, p. 226

- Cohn, *Economic Development in the Soviet Union*. 'A. Nove, p. 227
- Crowder and Ikime, *West African Chiefs: Their Changing Status Under Colonial Rule and Independence*. Lloyd A. Fallers, p. 228
- de Robien, *The Diary of a Diplomat in Russia, 1917-1918*; Kenez, *Civil War in South Russia, 1918*; and Silverlight, *The Victors' Dilemma: Allied Intervention in the Russian Civil War, 1917-1920*. Robert F. Byrnes, p. 229
- Gamarnikow, *Economic Reforms in Eastern Europe*. Robert Owen Freedman, p. 231
- Gann and Duignan, eds., *Colonialism in Africa 1870-1960*. Volume Two: *The History and Politics of Colonialism 1914-1960*. Harvey Glickman, p. 232
- Gordon, *Self-Determination and History in the Third World*. Rupert Emerson, p. 233
- Green, *The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy*; and Duncan and Goodsell, eds., *The Quest for Change in Latin America: Sources for a Twentieth Century Analysis*. Alexander Edelman, p. 235
- Groennings, Kelley and Leiserson, eds., *The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents*. Steven J. Brams, p. 236
- Havens and Flinn, eds., *Internal Colonialism and Structural Change in Colombia*, Reid R. Reading, p. 238
- Hazard, *Communists and Their Law: A Search for the Common Core of the Legal Systems of the Marxian Socialist States*. Harold J. Berman, p. 240
- Hill, *God's Englishman: Oliver Cromwell and the English Revolution*; and Breen, *The Character of the Good Ruler: A Study of Puritan Political Ideas in New England, 1630-1730*. Michael Walzer, p. 241
- Hilliker, *The Politics of Reform in Peru: The Aprista and Other Mass Parties of Latin America*. Edward C. Epstein, p. 242
- Johnson, *The Cameroon Federation: Political Integration in a Fragmentary Society*. Claude E. Welch, Jr., p. 243
- Journalist M., *A Year is Eight Months*; and Sviták, *The Czechoslovak Experiment, 1968-1969*. Joseph Hajda, p. 244
- Kochan, ed., *The Jews in Soviet Russia Since 1917*. John A. Armstrong, p. 246
- Kornberg and Musolf, eds., *Legislatures in Developmental Perspective*. Hans H. Baerwald, p. 248
- Kumar, *Essays on Gandhian Politics; The Rowlatt Satyagraha of 1919*. Joan V. Bondurant, p. 249
- Leach and Mukherjee, eds., *Elites in South Asia*. Theodore P. Wright, Jr., p. 251
- Levinson and Brightman, eds., *Venceremos Brigade: Young Americans Sharing the Life and Work of Revolutionary Cuba*; and Reckord, *Does Fidel Eat More Than Your Father?: Conversations in Cuba*. Patricia W. Fagen, p. 252
- Loomis and Rytina, *Marxist Theory and Indian Communism*. John Patrick Haithcox, p. 254
- Malloy, *Bolivia: The Uncompleted Revolution*. Robert D. Tomasek, p. 255
- Mboya, *The Challenge of Nationhood: A Collection of Speeches and Writings*. Brack Brown, p. 257
- Moine, *Résultats des élections belges entre 1847 et 1914*. Val R. Lorwin, p. 258
- Myrdal and Kessle, *China: The Revolution Continued*; and Hsiung, *Ideology and Practice: Evolution of Chinese Communism*. Pi-chao Chen, p. 259
- Nagy-Talavera, *The Green Shirts and the Others: A History of Fascism in Hungary and Rumania*. Randolph L. Braham, p. 261
- Petrov, *Mongolia: A Profile*. Paul Hyer, p. 262
- Powell, *Social Fragmentation and Political Hostility: An Austrian Case Study*. Robert D. Putnam, p. 263
- Rhodes, *The Government of London: The Struggle for Reform*. Wallace S. Sayre, p. 264
- Rivkin, *Nation-Building in Africa: Problems and Prospects* (edited by John H. Morrow). I. William Zartman, p. 266
- Rotberg and Clague, *Haiti: The Politics of Squalor*. Jean-Claude Garcia-Zamor, p. 266
- Ruin, ed., *Scandinavian Political Studies, Volume 5/1970*. Dankwart A. Rustow, p. 267
- Sharp, *The Ministry of Housing and Local Government*. Fred Tickner, p. 269
- Spencer, ed., *Religion and Change in Contemporary Asia*. Donald E. Smith, p. 270
- Suh, *Documents of Korean Communism 1918-1948*. John Kie-Chiang Oh, p. 272
- Tuden and Plotnicov, eds., *Social Stratification in Africa*. P. C. Lloyd, p. 273
- Tyrmand, ed., *Kultura Essays*. S. L. Andreski, p. 274
- Ugalde, *Power and Conflict in a Mexican Community: A Study of Political Integration*. Carolyn Needleman and Martin L. Needleman, p. 274
- Walter, *Student Politics in Argentina: The University Reform and Its Effects, 1918-1964*. Peter G. Snow, p. 276

277 International Politics, Law and Organization

- Beck, Deutsch, Raup, Rose and Turnbull, *The Changing Structure of Europe: Economic, Social and Political Trends*. Stuart A. Scheingold, p. 277
- Butler, *The Soviet Union and the Law of the Sea*. Kazimierz Grzybowski, p. 278
- Corbett, *The Growth of World Law*; and Franck, *The Structure of Impartiality: Examining the Riddle of One Law in a Fragmented World*. Wesley L. Gould, p. 279
- George, Hall and Simons, *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam*. George Modelski, p. 280

Gupta, *The Fulcrum of Asia: Relations Among China, India, Pakistan, and the U.S.S.R.*; and Simmonds, *China's World: The Foreign Policy of a Developing State*. Loren W. Fessler, p. 282

Heath, *Old World, New Horizons: Britain, Europe, and the Atlantic Alliance*; and Calleo, *The Atlantic Fantasy: The U.S., NATO, and Europe*. Francis A. Beer, p. 283

Jervis, *The Logic of Images in International Relations*. Paul Kecskemeti, p. 285

Ogley, *The Theory and Practice of Neutrality in the Twentieth Century*. Urs Schwarz, p. 286

Šveics, *Small Nation Survival: Political Defense in Unequal Conflicts*. Carsten Holbraad, p. 287

Thorp, *The Reality of Foreign Aid*. Doris A. Graber, p. 288

Toscano, *Designs in Diplomacy: Pages from European Diplomatic History in the Twentieth Century* (edited by George A. Carbone). Norman Kogan, p. 289

Tung, *China and the Foreign Powers: The Impact of and Reaction to Unequal Treaties*. Samuel Shih-Tsai Chen, p. 290

Váli, *Bridge Across the Bosphorus: The Foreign Policy of Turkey*. Carl M. Dibble, p. 291

Van Ness, *Revolution and Chinese Foreign Policy: Peking's Support for Wars of National Liberation*. Shao-chuan Leng, p. 293

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Where the Votes Come From: An Analysis of Electoral Coalitions, 1952-1968*

ROBERT AXELROD

University of California, Berkeley

Where do the Democrats get their votes from? Where do the Republicans get *their* votes from? Because of the great interest of scholars, politicians, and the public in the changing shape of electoral politics in America, these questions have been asked over and over. V. O. Key has argued that one party or the other usually dominates American politics for decades by putting together a stable winning coalition and that since the Depression the dominant party has been the Democrats.¹ He has, however, also pointed to the existence of slow but steady secular trends in electoral coalitions.² In 1968 the rise of a third party challenge and the election of a Republican President led to numerous reassessments of the future of the party coalitions. Kevin Phillips's assertion of *The Emerging Republican Majority*³ is perhaps the best known of these recent prognostications, but a critical review by Nelson Polsby⁴ has already undermined much of Phillips's argument. The Republican Ripon Society has given a liberal interpretation of the *Lessons of Victory*,⁵ while the less partisan interpretation of Scammon and Wattenberg has emphasized the size of the middle groups in American politics.⁶ The long term cyclical pattern in American electoral politics has been reported by Sellers,⁷ and Burnham⁸

and Converse *et al.*⁹ have done excellent studies of some of the factors in recent presidential elections. Yet, a number of basic questions about the sources of partisan support remain.

Surprisingly enough, even professional politicians have only a rough and ready idea of how their own national coalitions are formed. For example, many do not know whether Blacks contribute more or less than union members to the vote totals of the Democratic party. As another example, we may ask whether the common wisdom is really correct that the Republicans do worse than the Democrats among the young and the poor. Questions such as these require measurement of groups and their voting behavior. Measuring where the parties have been and where they are now is the first step toward a sophisticated analysis of where they are going.

This paper has two purposes. The first is analytic—to specify the components of a group's contribution to a party's electoral coalition. The second is empirical—to measure the actual magnitudes of the contributions that have been made by selected groups in each of the last five presidential elections.

The Meaning of a Contribution to a Coalition

What does it mean to ask how large a contribution a group makes to the electoral total of a party? Clearly, a large group can contribute more votes to a party than can a small group. It is also true that a group with a high turnout can contribute more votes than a slightly larger group with a lower turnout. And yet it is also true that a relatively small group with a poor turnout can contribute votes to a party quite out of proportion to its size if the group loyally gives overwhelming support to that party. Thus measurement of the contribution of a group to a party's total vote must depend on three factors: the size of the group, its turnout, and its loyalty to the given party.

* I gratefully acknowledge the help of Nicholas Miller, my research assistant. I also wish to thank the Institute of Governmental Studies and the Committee on Research of the University of California, Berkeley, for their financial support of this study, and the Inter-University Consortium for Political Research for making the survey data available. Finally, I wish to thank William Cavala, Jack Citrin, Merrill Shanks, and Aaron Wildavsky who read drafts of this paper.

¹ V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3-18.

² V. O. Key, "Secular Realignment and the Party System," *Journal of Politics*, 21 (May, 1959), 198-210.

³ Kevin Phillips, *The Emerging Republican Majority* (New Rochelle, New York: Arlington House, 1969).

⁴ Nelson W. Polsby, "An Emerging Republican Majority? Review Essay," *The Public Interest*, No. 17 (Fall, 1969), 118-126.

⁵ Ripon Society, *The Lessons of Victory* (New York: Dial Press, 1969).

⁶ Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward-McCann, 1970).

⁷ Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, 29 (Spring, 1965), 16-38.

⁸ Walter Dean Burnham, "American Voting Behavior and the 1964 Election," *Midwest Journal of*

Political Science, 12 (February, 1968), 1-40. See also his *Critical Elections* (New York: Norton, 1970).

⁹ Phillip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 53 (December, 1969), 1083-1105.

Each of these three components of a group's contribution is important. Much confusion arises in political discussions about how the parties build their coalitions because no one of these components tells the whole story. Only when all three components are taken into account can the actual contribution of a group be fully determined.

The coalition literature from game theory is of little help here because of its assumption that groups are unified actors. Each group is assumed to be able to turn out all of its members and deliver them with complete loyalty to the coalition of its choice.¹⁰ While this assumption is suitable for certain legislative bodies,¹¹ it is a gross distortion when applied to a mass electorate whose turnout and loyalty are always less than complete.

Fortunately, the three components of size, turnout, and loyalty can be fitted together in a rather simple way to determine a group's contribution to a party's vote total. The formula is:

Contribution

$$= \frac{(\text{Size}) \times (\text{Turnout}) \times (\text{Loyalty})}{(\text{National Turnout}) \times (\text{National Loyalty})}$$

This formula says that if you multiply the size, turnout, and loyalty of a given group, and divide the result by the product of the national turnout and national loyalty, you get the magnitude of the contribution of that group to the electoral coalition of the party.¹² The *size* of a group is the proportion of all adults of voting age who are members of that group. The *turnout* of a group is the proportion that voted in a

given election.¹³ The *loyalty* of a group to a certain party is simply the proportion of the votes of that group which are cast for that party. The *national turnout* is the proportion of all adults who vote, and *national loyalty* is the proportion of all votes that go to the given party. Finally, the *contribution* of a group to the electoral coalition of a party equals the proportion of all the party's votes that come from members of that group.

The value of this formula is that it specifies how the three components can be combined to form the total contribution of a given group. It allows us to study contributions of a group over time, to determine how the contribution is attained in terms of the separate components, and to compare the contribution of one group to that of another.

Since any combination of groups may be chosen for analysis, the groups will typically overlap. A comparison can still be made between the overlapping groups in terms of their separate contributions, size, turnout and loyalty, but these factors cannot be added together directly. For example, in 1968 the poor (i.e., families with incomes less than \$3000 per year) contributed 12 per cent of the Democratic votes, while the Blacks contributed 19 per cent. The contribution of the group consisting of those who are poor or Black (or both) can be determined by adding the separate contributions and subtracting the contribution of the overlapping group, namely the poor Blacks. Since the poor Blacks contributed 5 per cent of the Democratic votes, the contribution of those who were poor or Black (or both) was 12% + 19% - 5% = 26%. A similar procedure applies to measuring the *size* of the group which is the union of two overlapping groups.

The procedure for handling overlaps readily generalizes to any group describable in terms of more than two component groups.¹⁴ This means that the contribution and size of any

¹⁰ See John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, Second ed. (Princeton: Princeton University Press, 1947), and R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: Wiley, 1957).

¹¹ See William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), and Sven Groennings, E. W. Kelley, and Michael Leiserson, *The Study of Political Coalitions* (New York: Holt, Rinehart and Winston, 1970).

¹² The proof involves nothing more complicated than substituting the definitions of terms for their names and cancelling whenever possible.

¹³ In calculating turnout, one counts all adults of voting age, whether or not they were eligible to vote.

¹⁴ John Kemeny, J. Laurie Snell, and Gerald Thompson, *Introduction to Finite Mathematics*, Second ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 116-122.

$$\begin{aligned} & \frac{(\text{size}) (\text{turnout}) (\text{loyalty})}{(\text{natl. turnout}) (\text{natl. loyalty})} = \frac{\left(\frac{\text{no. in group}}{\text{no. in nation}} \right) \left(\frac{\text{voters in group}}{\text{number in group}} \right) \left(\frac{\text{voters for party in group}}{\text{voters in group}} \right)}{\left(\frac{\text{voters in nation}}{\text{number in nation}} \right) \left(\frac{\text{voters for party}}{\text{voters in nation}} \right)} \\ & = \frac{\text{voters for party in group}}{\text{voters for party}} \\ & = \text{contribution.} \end{aligned}$$

group can be analyzed if the relevant information is available about the component groups and their intersections. While this type of information is difficult to get from aggregate data, it is readily available from survey data. Thus, the poor, the Blacks, the poor Blacks and those who are poor or Black can all be analyzed if desired. This paper will fully report a selected set of overlapping groups and note a few of the interesting properties of their intersections and unions.

It should be clear that the term "contribution" as it is being used in this study refers to the proportion of a party's total votes that is provided by a given group. It does not tell how the votes were gathered or even whether the group membership provides a reference point for its members. For example, it does not measure a group's contribution in terms of money, policy, or organizational strength. It simply counts what ultimately matters in elections, namely votes.¹⁵

The Coalition Members

Since the New Deal there has been a generally accepted answer to the question of where the Democrats get their votes. The answer is that the Democrats are a coalition of diverse overlapping minorities: the poor, Blacks, union members, Catholics and Jews, Southerners, and city dwellers. In the last few years it has been suggested that, at least for presidential elections, the Southerners may have left the Democratic coalition while the young people have entered it.

The Republicans are a different matter. It is possible to think of the Republicans also as a coalition of minorities such as the rich and the people of Anglo-Saxon descent. But the Republican party itself tries to avoid this sort of formulation in favor of the view that Republicans appeal to the nonminorities, while the Democrats appeal to the minorities. In these terms, the Republicans can be thought of as a coalition of the nonpoor. Whites, nonunion families, Protestants, Northerners, and those outside the central cities.

The Republican groups are larger in size than the corresponding Democratic groups, and they generally have a higher turnout, but size and turnout are only two of the components of a group's contribution to a party's coalition.

¹⁵ The same formula could be used to study contributions of resources other than votes, such as financial contributions and time of volunteers. In that case, the turnout would measure the resource provided by a given group to either party, and the loyalty would measure the proportion of the resource that went to a given party.

The Democrats' relative advantage is in the third component, namely partisan support, because the pro-Democratic minorities generally give a higher percentage of their votes to the Democrats than the corresponding nonminorities give to the Republicans.

To measure the magnitude of the three components and the total contribution of each group to its party's electoral coalition, a combination of survey data and election returns can be used. The election returns show the total national loyalty each party received, and they can be also used in conjunction with census data to estimate the national turnout. A survey based upon a carefully drawn national sample can be used to measure the components for each of the separate groups. Fortunately, the Survey Research Center of the University of Michigan has conducted such a national survey for every presidential election since 1952, so comparable data for the last five presidential elections are now available. Income, race, union membership, religion and place of residence, being relatively objective criteria, present few of the problems associated with measuring attitudes. There are still problems in determining how a person actually voted, even if the person is re-interviewed after the election as these respondents were. Some of them evidently reported that they had voted when in fact they did not, and a small percentage reported that they voted for the winner when they actually voted for the loser. The raw survey data have been adjusted with the use of national election data to compensate for these two types of error.¹⁶ The numbers will still contain some random sam-

¹⁶ The *national turnout* and *loyalty* figures are from the *Statistical Abstract of the United States*, 1970, pp. 354 and 368. The *group size* figures were taken directly from the Survey Research Center's sample. The *group loyalty* and *turnout* figures were adjusted so that taken together with the group sizes they conform with the national loyalty and turnout statistics. The adjustment procedure is an iterative process by which a contingency table with given marginal distributions can be transformed into a table with specified marginal distributions while preserving the nature and strength of the association, as measured by appropriate criteria. The procedure is that developed by Frederick Mosteller, "Association and Estimation in Contingency Tables," *Journal of the American Statistical Association*, 63 (March, 1968), pp. 1-28 (see especially pp. 6-8). For a similar application of this adjustment procedure see David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1969), pp. 283ff. The sizes of the national survey samples were 1,899 in 1952; 1,762 in 1956; 1,181 before weighing and 1,954 after weighing in 1960; 1,571 in 1964; and 1,557 in 1968. Pairwise deletions were used with missing data to preserve sample size, which means that a person was deleted from a count only when some information relevant to that particular count was missing.

Table 1. The Democratic Coalition, 1952–1968

Percentage Contribution =							(Size ×						Turnout ×						
Year	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	P
1952	28	7	38	41	20	21	36	10	27	26	28	16	46	23	66	76	35	68	47
1956	19	5	36	38	23	19	25	9	26	25	29	14	40	23	64	72	39	63	47
1960	16	7	31	47	27	19	23	10	25	25	34	13	46	31	60	74	50	74	48
1964	15	12	32	36	21	15	19	11	23	26	28	12	45	42	69	72	49	65	69
1968	12	19	28	40	24	14	16	11	24	26	31	10	44	51	61	68	53	63	44
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

P poor (income under \$3,000/yr.)
 B Black (and other nonwhite)
 U union member (or union member in family)
 C Catholic (and other non-Protestant)
 S South (including border states)
 CC central cities (of 12 largest metropolitan areas)
 NT National turnout
 NL National loyalty to Democrats

Year	
1952	+2
1956	+5
1960	-2
1964	+8
1968	+1
Column	27

pling error, but most of them are probably within three or four per cent of their true value.

The Democrats. Table 1 gives the data for the Democratic coalition in terms of the selected groups. Along the top of the table is written the formula for a group's contribution to a party, and within each block of the table are listed the different groups by year. Directly under the block for loyalty is a block which shows how a group's loyalty differs from the national loyalty.

1. *The Poor.* The poor are defined to be those with an annual family income less than \$3000.¹⁷ Column 1 of Table 1 shows that the contribution of the poor to the Democratic coalition has fallen dramatically: from 28 per cent of Democratic votes in 1952 to only 12 per cent in 1968. The main reason their contribution has fallen is that their size has shrunk over these years, from 36 per cent of the total adult population to 16 per cent (column 7). This decrease in the "poor" is partly due to the fact that \$3000 per year is a more restrictive definition for the 1960's than it is for the 1950's because of inflation. Much of the decrease, however, is due to gains in real income.¹⁸ Since their turnout has been steady but low at about 45 per cent, (Column 13) and their loyalty (Column 19) has been about 45 per cent Democratic (except in 1964), the decrease in the contribution of the poor to the Democrats is due almost solely to the dwindling size of this group.

The curious fact about the voting pattern of the poor is that, contrary to popular belief, they are not distinctly loyal to the Democrats. Column 19 shows that only in 1964 did they give the Democrats a majority of their votes, and Column 27 shows that to within a few percentage points, their vote generally divides the same as does the entire nation's vote. In brief, the poor are *not* part of the Democratic coalition. The working class may be loyal, but the poor are not.

2. *The Blacks.* The Blacks are another story altogether. Their contribution has grown sub-

stantially from 5-7 per cent in 1952-1960 to 12 per cent in 1964 and 19 per cent in 1968 (Column 2). This increase in their contribution to the total Democratic vote is not due to an increase in the number of the Blacks, since their size has been almost constant at about 10 per cent of the population throughout this period (Column 8). It is their turnout that has increased dramatically, nearly doubling between 1952 and 1968. The biggest jumps came in 1960 and 1964 as Blacks began to exercise their franchise in greater numbers, not only in the South but throughout the nation. The Blacks have always been very loyal to the Democrats (Column 20), especially in relation to voting patterns of other groups (Column 28). Curiously enough, this overwhelming loyalty of the Blacks was present in 1952, but not in Stevenson's second effort or in Kennedy's campaign. It returned to and exceeded its previous high level in the campaigns of Johnson and Humphrey. Only in 1968, however, did high loyalty and moderately good turnout combine to produce a Black contribution to the Democrats that was strikingly larger than their population percentage. Of course, the low national loyalty to the Democrats in 1968 also helped account for the high percentage of all Democratic votes that were contributed by Blacks in that year.

3. *The Unions.* Union members and their families provide yet a different story. They contributed more than a third of all Democratic votes in the 1950's, but in the 1960's their contribution fell slightly to a still very respectable figure of 28 per cent (Column 3). This large contribution arises because about a quarter of all adults are in union families (Column 9), they have a turnout close to the national average (Column 15 versus Column 25), and they consistently vote more Democratic than the nation as a whole (Column 29). The gradual decline of the union contribution to the Democratic party is due to a slight decline in their relative size (Column 9) and a large drop in their loyalty in 1968 (Column 21).

A comparison of the Black contribution and the union contribution is revealing, provided one bears in mind that people in both groups vote the way they do for many reasons other than their race and union membership. In 1960 the union voters contributed more than four times as many votes to the Democrats as the Blacks did, but by 1968 the unions contributed only one and a half times as many votes as the Blacks (Column 3 versus Column 2). So until recently the voters from union families made a much, much larger contribution than the Blacks, and they still make a significantly greater contribution.

¹⁷ This definition is not very satisfactory because inflation and rising median income have changed the social meaning of any fixed income level. It does have the virtue, however, of being easy to define and comprehend.

¹⁸ The magnitude of inflation in these years is indicated by the Consumer Price Index. Using 1957-59 = 100, the figures are 92.5 for 1952, 94.7 for 1956, 103.1 for 1960, 108.1 for 1964, and 121.2 for 1968. The figures were prepared by the Bureau of Labor Statistics and are reported by the Council of Economic Advisors in *Supplement to Economic Indicators Historical and Descriptive Background* (1964) and *Economic Indicators* (January, 1970).

4. *The Catholics.* The Catholics have formed a large and reliable segment of the Democratic coalition. They have always provided more than a third of the Democratic votes (Column 4), even though they are only a quarter of the population (Column 10). Part of the reason is their exceptionally high turnout (Column 16). The other part of the reason is their steadfast loyalty (Column 22) which has always been more than 10 per cent greater than that of the population as a whole (Column 30). Included in the Democratic coalition with the Catholics are the Jews, whose numbers are small, but whose turnout and loyalty have consistently been very high.

Of course a special year for the Catholics was 1960, when John Kennedy was the Democratic nominee. In that year the Catholics gave more than four-fifths of their vote to the Democrats (Column 22), thereby providing nearly half of all the votes the Democrats got in 1960 (Column 4).

5. *The Southerners.* The South, including the border states, offers something of a surprise. Voters from this region have provided roughly one-fourth of all Democratic votes (Column 5). The size of the group has held steady (Column 11), while the turnout has increased since 1956 (Column 17), partly because of the increased turnout of the Blacks in the South. The surprise is that overall, Southerners have not been exceptionally loyal to the Democrats since 1956 (Column 23). In the Eisenhower elections they split their vote down the middle, and since then have voted within a few percentage points of the national Democratic average (Column 31).

It appears from these figures that the South is moving away from the Democrats. The Southerners were about 10 per cent more pro-Democratic than the nation as a whole in the 1952 and 1956 elections, but only 2 per cent more in 1960; in 1964 and 1968 they voted 3 per cent and 4 per cent *less* Democratic than the country as a whole did (Column 31). This trend has occurred despite the counter-trend of Black Southerners, who are very loyal Democrats, to increase their turnout in those same years. The Democratic loss is not necessarily the Republican gain, for in 1968 a third of the White Southern votes went to Wallace (not shown in the table), accounting for slightly more than half of all Wallace's votes.

6. *The Central Cities.* Only about 10 per cent of the nation's population now lives in the central cities of the dozen largest metropolitan areas, a figure that has been falling slowly since 1952 (Column 12). Central city dwellers used

to have a slightly better than average turnout (Column 18), and since 1956 they have been 13 per cent to 15 per cent more Democratic than the nation as a whole (Column 32). These trends make the central cities an important but slowly decreasing part of the Democratic coalition, having provided about 20 per cent of the Democratic votes in the 1950's and about 15 per cent of the Democratic votes in the last two elections (Column 6).

A person may, of course, belong to more than one of these overlapping groups. For this reason the contributions and sizes of the groups listed on Table 1 can add up to more than 100 per cent. Some of the Democratic groups overlap more or less than would be expected from their sizes alone. Thus, for instance, the Blacks constitute a higher proportion of the population of the South than of the North, while on the other hand, the Blacks represent a lower proportion of Catholics than they do of Protestants. Typically, the overlap of two given Democratic groups has a higher loyalty than the average of the loyalties of the two given groups.¹⁹ For example, in 1968, Catholic union members had a loyalty of 65 per cent which is actually greater than the loyalty of either the Catholics (61 per cent) or the union members (51 per cent).

Some people belong to none of the six overlapping groups listed on Table 1. Actually about one-fifth to one-fourth of the adult population is neither poor, Black, unionized, Catholic, Southern, nor in central cities. This residual group tends to vote Republican, as would be expected, and in fact votes about twenty per cent more Republican than the nation as a whole.

One could go on almost indefinitely examining the partisan support provided by people with different attributes. But one additional group merits particular attention: that is the young. Those between 21 and 29 years of age are about 18 per cent of the voting age population, yet they provide only about 14 per cent of all Democratic votes. There are two reasons for this curious fact. First, the young have a turnout record about 15 per cent below the national

¹⁹ This empirical generalization is substantiated by the 1968 data for the five actual segments of the Democratic coalition. It does not follow automatically from the definitions, however. In fact, the overlap hypothesis that the intersection of two loyal groups is more loyal than the average of the two groups is closely related to the cross-pressure hypothesis that a person who is a member of one Democratic group and one Republican group is likely to be less loyal to both parties than either group membership alone would predict.

average. Second, the young are simply not loyal to the Democrats. In fact, in four of the last five presidential elections the young have given the Democrats the same proportion of their vote (within 3 per cent) as has the whole country. In 1968 the young with their weak party loyalties, were slightly pro-Wallace compared to the nation as a whole. So like the poor, the young have not been part of the loyal Democratic coalition.²⁰ Now that the 18 to 21 year olds have been enfranchised, the size of the under-30 group will increase. Their turnout is likely to remain below average, but it is uncertain whether their pattern of not being loyal to either major party will continue.

The Republicans. The Republican coalition can be thought of as consisting of the overlapping majorities that are the precise complements of the minorities that describe the Democratic coalition. This makes the Republican coalition a combination of the nonpoor, Whites, nonunion families, Protestants, Northerners, and those outside the central cities. Virtually everyone is in at least one of these overlapping groups, and most people are in four or five of them.

Most of the facts about how these groups contribute to the Republican party follow directly from what has already been said about how their complements contribute to the Democratic party. For example, the fact that the poor are no more loyal to the Democrats than is the nation as a whole implies that the nonpoor cannot be especially loyal to the Republicans. Because of these relationships we can discuss only a few of the more salient facts and let Table 2 tell the rest of the story of how the Republican coalition is put together.

1. *The Nonpoor.* Those with incomes greater than \$3000 have contributed an increasing proportion of all Republican votes since 1952 (Table 2, Column 1), mainly because more and more people have crossed this line over the years (Column 7). But as was just pointed out, the nonpoor do not deviate much from the national average in their loyalty to the Republicans (Column 27).

2. *The Whites.* Virtually all Republican votes come from Whites (Column 2). Even in 1960 when a quarter of the Black vote went to Nixon, 97 per cent of Nixon's votes came from Whites, while in 1968, 99 per cent of Nixon's votes came from Whites. Whites are about 90 per cent of the population (Column 8) and they vote 1 per cent to 3 per cent more Repub-

lican than the nation as a whole (Column 28). This is just the obverse of the statement that the Blacks, who are about 10 per cent of the population, vote heavily Democratic.

3. *The Nonunion Families.* Nonunion families have voted 5 per cent or 6 per cent more Republican than the nation as a whole from 1952 to 1964 (Column 29). This figure takes on real importance when combined with the fact that three-quarters of all adults are in families without a union member (Column 9).

4. *The Protestants.* The Protestants are also about three-quarters of the population (Column 10), and have usually voted about 5 per cent more Republican than the nation as a whole (Column 30). The special year of course, was 1960 when a Catholic candidate opposed a Protestant for President and 90 per cent of the Republican votes came from Protestants (Column 4).

5. *The Northerners.* The Northerners have an unusually high turnout (Column 17), relative to the turnout in the South. When it comes to loyalty, however, the Northerners have never been more than 3 per cent pro-Republican in these five presidential elections, and twice have not been pro-Republican at all (Column 31).

6. *Outside the Central Cities.* Those living outside the central cities of the twelve largest metropolitan areas have slowly increased from about 85 per cent of the total population in the 1950's to 90 per cent in 1968 (Column 12). Their turnout, however, is only about average, and their loyalty is only slightly more Republican than the nation as a whole (Column 32), partly because they are so close to equaling the whole nation.

The Party Coalitions

The most obvious fact about American party coalitions is that they are very loose. They are loose first in the sense that most group loyalties are not total. Except for Blacks, none of the twelve groups that have been examined in the Tables ever gave more than 80 per cent of their votes to one party. Second, the coalitions are loose in the sense that group loyalties are not constant from one election to the next. Finally, when a group's loyalty shifts it is as likely to shift in response to a national trend as it is for reasons specific to the group. Indeed, again with the exception of Blacks, each of the groups usually divided their votes no more than 15 per cent differently than did the nation as a whole.

The data from Table 1 and Table 2 do not point to an election in these years which suddenly shifted the nature of the coalitions and

²⁰ In 1968 the young contributed 13 per cent to the Democratic coalition by having a size of 18 per cent, a turnout of 47 per cent and a loyalty of 40 per cent.

Table 2. The Republican Coalition, 1952-1968

Percentage Contribution =							(Size						×	Turnout						×
Year	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	
1952	75	99	79	75	87	84	64	90	73	74	72	84	72	67	61	58	73	61	56	
1956	84	98	78	75	84	89	75	91	74	75	71	86	67	64	58	56	69	60	59	
1960	83	97	84	90	75	90	77	90	75	75	66	87	70	68	65	61	71	63	50	
1964	89	100	87	80	76	91	81	89	77	74	72	88	67	66	61	60	68	63	40	
1968	90	99	81	80	80	92	84	89	76	74	69	90	65	63	62	60	66	62	44	
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
																			Year	
NP	nonpoor (income over \$3,000/yr.)																		1952	
W	White																		+1	
NU	nonunion																		1956	
P	Protestant																		0	
N	Northern (excluding border states)																		1964	
NCC	not in central cities of 12 largest metropolitan areas																		+1	
NT	National turnout																		1968	
NL	National loyalty to Republicans																		0	
																			Column	
																			27	

then set the pattern for the elections to follow. In Key's terms, none of these elections is an obvious choice for a critical election, although there have been secular realignments. These gradual shifts have been in different factors for different groups: loyalty of the South, size of the poor and central cities dwellers, and both turnout and loyalty of the Blacks. There have also been dramatic short-term shifts, for instance by the Catholics.

The data on party identification provide another point of view about what has been happening to the party system. In each year the respondents were asked, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?" The results in Table 3 show that the party identification has remained almost stable since 1952. Neither major party has gained strength, and the Democrats still have a considerable edge over the Republicans. A new factor in 1968 was an increase in the number of Independents to 29 per cent of the national sample. This increase is not due solely to the appeal of George Wallace, because fully 28 per cent of those who did not vote for Wallace thought of themselves as Independents in 1968.

Table 3. Percentage of Party Identification, 1952-68

Party	Democrat	Independent	Republican
1952	47	22	27
1956	44	24	29
1960	46	23	27
1964	51	23	24
1968	44	29	25

(Note: Several per cent "apolitical" and "don't know" in each year are not shown).

The third-party coalition put together by Wallace can be measured just as the coalitions of the other two parties have been. In 1968 Wallace received 14 per cent of the national vote. The only one of the twelve groups that gave him a great deal more than this was the South (including the border states) which had a loyalty to Wallace of 28 per cent. The Catholics gave Wallace only 9 per cent of their vote, the central city dwellers 8 per cent, and the non-Whites a mere 1 per cent. The other categories did not have much of a net impact. The poor gave him 16 per cent of their vote, compared to 13 per cent from the nonpoor; the union families gave him 15 per cent, compared to 13 per cent from the nonunion families; and the young gave him 18 per cent, compared to the 13 per cent from those 30 and older.

All in all, there have been and still are significant differences in the electoral coalitions of the two major parties, despite Wallace's claim that there isn't a dime worth of difference in what they stand for. Race, union membership, religion and place of residence still matter. The Democrats usually get major contributions of votes from the Blacks, the union members, the Catholics and those in central cities. The Republicans usually have slightly greater appeal for the Whites, the nonunion families, the Protestants, and those outside the central cities. The poor and the young, however, divided their votes just about the same way as did the nation as a whole and were not really part of any party's electoral coalition.

Some Strategic Considerations

There is little a party can do to increase the size of a demographic group, but there is much it can do to try to increase its turnout and loyalty. A major consideration for a party in deciding whether to appeal to a given group is what might be called the group's "elasticity of response." The question is not whether a group will be receptive to an appeal, but whether a greater or lesser appeal will make much difference in the behavior of the members of the group. From a strategic point of view, the problem is to use scarce resources in the best way, presumably to maximize the chances of victory. If a group has a nearly constant response, independent of the intensiveness of the appeal to it, there is little sense in devoting many resources to that group. But if a group's response is sensitive to the magnitude of the efforts to appeal to it, then a party would do well to devote scarce resources to increase this appeal.

A good example of how a group can respond to a particular appeal is provided by the Catholics. In 1960, the Democrats nominated a Catholic for President, and the proportion of the Catholic vote that went to the Democrats went up almost 30 per cent from 53 per cent in the previous election to 82 per cent. In the same four year period the nation as a whole increased its loyalty to the Democrats by only 8 per cent. Thus the Catholics' response to the appeal of a Catholic candidate was highly elastic. Unfortunately for the Democrats, it was also elastic in the other direction as well. In the next two presidential elections, when Protestants were again nominated, the deviation in loyalty of the Catholics to the Democrats fell back almost to its previous levels (Table 1, column 3).

Another approach is for a party to direct some of its resources to increase the turnout of

a group, through such means as a registration drive. If loyalty is to be increased, voters must be converted from another party, but if turnout is to be increased, they need only be mobilized. The mobilization strategy has the disadvantage that a newly mobilized voter who votes for the party is only half as valuable as a voter who is newly converted from the other party of a two-party system. The mobilization strategy has the further disadvantage that the newly mobilized voter may not vote for the intended party. Mobilization is sometimes relatively inexpensive, however, and it can often be accomplished without antagonizing other groups in the coalitions. The ideal opportunity for a mobilization strategy by a party arises when a group has high loyalty combined with low turnout. Such has been the case with the Blacks, and the Democrats have acted accordingly. If there is new evidence that the young will depart from their previous pattern of behavior and start to be loyal to a given party, that party would be wise to use a mobilization strategy aimed at the young.

The expression of a group's contribution in terms of its size, turnout, and loyalty can be used to analyze hypothetical strategic choices involving any division of the population. The first step is to notice that the contribution of any group plus the contribution of its complementary group is always one. Using the definition of contribution, this gives

$$\frac{s_g t_g l_g}{t_n l_n} + \frac{s_c t_c l_c}{t_n l_n} = 1.00$$

where s , t and l are size, turnout and loyalty respectively and the subscripts g , c and n signify the group, its complement, and the nation. The national turnout can be expressed as a function of the size and turnout of the group and its complement,

$$t_n = s_g t_g + s_c t_c,$$

and the size of the complement can be expressed in terms of the size of the group itself,

$$s_c = 1 - s_g.$$

Substituting the last two equations into the first one, and solving for the national loyalty gives what might be called the strategic equation,

$$l_n = \frac{s_g t_g l_g + (1 - s_g) t_c l_c}{s_g t_g + (1 - s_g) t_c}.$$

The strategic equation has six variables. A typical application would assume that the values of several of them are known from previous measurements or extrapolations, and would ask what would be the effect of varying one or two

of the remaining variables. For example, if one knew the size, turnout, and loyalty of a group he could determine what levels of turnout and loyalty would have to be achieved from the people *outside* the group in order to get a given percentage of the national vote.

As a concrete example, consider as a single group the people that Scammon and Wattenberg²¹ emphasize, the "unyoung, unpoor and unblack." Using 1968 SRC data and the previous definitions, the size of this group is 62 per cent and its turnout is 70 per cent, while the turnout of all the others is 50 per cent. Substituting these values in the strategic equation and solving for l_c gives

$$l_c = .3281_n - .2281_g.$$

Thus if a party could count on 55 per cent of the vote of the "unyoung, unpoor and unblack," it would still need at least 39 per cent of the other voters to win a majority in a two party race. In a three party race in which it could count on 50 per cent of the "unyoung, unpoor and unblack," it would still need 34 per cent of the other voters to get 45 per cent of the national vote.

Other problems can also be analyzed with the strategic equation. If an appeal to a group increases its loyalty by a given amount, how much can the loyalty of the other voters decrease before the break-even point is reached? If the turnout of a given group is increased by a given amount, how much does this increase a party's percentage of the national vote? Finally, a generalized form of the strategic equation can be used to analyze similar questions involving the partitioning of the population into more than two nonoverlapping groups.²²

This paper has described how the contributions that different groups make to a party's total strength can be specified. It has also shown how these contributions can be broken down into their three components—size, turnout, and loyalty. Through the examples of selected groups, the actual magnitude of these contributions and their components for each of the last five presidential elections have been presented and discussed with the hope that measuring these previous coalitions can be helpful in the task of understanding what may come next. I leave that greater task to others, whether they be scholars, politicians, or voters.

²¹ Scammon and Wattenberg, *Real Majority*.

²² If the nation is partitioned into mutually exhaustive and nonoverlapping groups (such as income levels), the strategic equation is

$$l_n = \frac{\sum s_i t_i l_i}{\sum s_i t_i}.$$

Turnout and Representation in Presidential Primary Elections*

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After Carl Albert adjourned the 1968 Democratic National Convention *sine die*, a good many Democrats and Democrat-watchers left Chicago's tear-gassed streets and stink-bombed hotels convinced that something was terribly wrong with the traditional way the party had gone about choosing its presidential nominee. The Democrats' subsequent loss of the election gave them about the only benefit the losing party ever gets: a chance to make major changes in its structure and procedures without having to clear them with an incumbent President.

Surprisingly, the party did not let the chance slip away. Chicago's "winners" joined the "losers" to launch the most ambitious intraparty effort for presidential nominating reform since the Republicans adopted the bonus-vote apportionment principle more than a half-century ago.¹ Executing a mandate from the convention, the National Chairman, Senator Fred Harris of Oklahoma, established two commissions in early 1969: the Commission on Party Rules, chaired by Representative James O'Hara of Michigan; and the Commission on Party

Structure and Delegate Selection, chaired until early 1971 by Senator George McGovern of South Dakota and since then by Representative Donald Fraser of Minnesota. Both commissions urged a number of drastic changes in the party's accustomed ways, and the Democratic National Committee has incorporated most of them in the rules governing the 1972 national convention.

The content, rationale, implementation, and impact of these new rules are topics outside the central concerns of this paper. For present purposes we need note only that the McGovern-Fraser Commission's most fundamental charge against the established delegate-selecting procedures was that they make the convention *unrepresentative*. A review of the commission's hearings and deliberations confirms my participant-observer's impression that, while its majorities did not really face up to the enduring complexities and dilemmas in the theory and practice of representation,² they found the traditional conventions unrepresentative in two main respects. First, they are *demographically* unrepresentative. The commission declared that certain significant elements of the citizenry—blacks, women, and young people—are the ones specifically mentioned in the report—should be represented among the delegates in proportions that "bear a reasonable relationship to the group's presence in the population";³ and past conventions have fallen woefully short of this ideal. Second, the conventions are *preferentially* unrepresentative—that is, they do not accurately represent the issue and candidate preferences of the party's rank-and-file. Many 1968 dissidents argued that the victories of Eugene McCarthy and Robert Kennedy in the presi-

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¹ It is worth noting that the bonus-vote reform was adopted by the Republicans after the angry divisions and charges of rigging in their 1912 convention and their subsequent schism and loss of the presidency. The Democrats, being in power at the time, did not follow suit for several decades. See Paul T. David, Ralph M. Goldman, and Richard C. Bain, *The Politics of National Party Conventions* (Washington, D.C.: The Brookings Institution, 1960), pp. 165–168.

² The literature on representation is as vast as its intellectual puzzles and institutional mazes are difficult. I have found especially helpful the work of Hanna Fenichel Pitkin, especially her *The Concept of Representation* (Berkeley and Los Angeles: University of California Press, 1967). Also useful are many of the papers in J. Roland Pennock and John W. Chapman, eds., *Representation: Nomos X* (New York: Atherton Press, 1968).

³ Commission on Party Structure and Delegate Selection, *Mandate for Reform* (Washington, D.C.: Democratic National Committee, 1970), Guideline A-2, pp. 40, 26–28; see also the summary in *Congressional Quarterly*, November 28, 1969, pp. 2418–2419.

dential primaries showed that the great majority of ordinary Democrats wanted an immediate end to the Vietnam war and to the Johnson-Humphrey leadership that was perpetuating it. Yet the convention refused to adopt a meaningful antiwar plank, and gave its nomination to prowar Humphrey rather than to antiwar McCarthy or McGovern. The commission's new guidelines governing the state parties' selection of convention delegates are intended, some say, to prevent any future convention from being so out of tune with the social composition, issue stands, and candidate preferences of the party's grass roots.

A few party leaders, of course, feel that *no* delegate convention, however reformed, can ever be as representative, demographically or preferentially, as a national presidential primary. Thus in June, 1968 Senator Mike Mansfield introduced the most recent of a long series of proposals for establishing a nationwide primary election to precede or even replace the conventions.⁴ Like its predecessors, the Mansfield bill did not get out of committee, but the idea is far from dead. And whatever may be the prospects for a national primary, it is clear that state presidential primaries are likely to play an important role in the 1970s: at the present writing no fewer than seven states (Arkansas, Maryland, New Mexico, New York, North Carolina, Rhode Island, and Tennessee) have newly added presidential primaries for 1972. This makes a total of twenty-two states and the District of Columbia with presidential primaries, by far the largest number since 1924.⁵

Most political scientists who have written on the question feel that the present "hybrid" or "mix" of primaries and conventions is desirable and that neither element should be totally abandoned in favor of the others.⁶ But for present

purposes the significant fact is the tacit agreement among most of the convention reformers, national-primary advocates, and defenders of the convention-primary mix that the presidential primaries—especially those organized on the Oregon model⁷—are presently the most representative element in the mix, the principal standard by which the representativeness of the other procedures for selecting and mandating convention delegates should be judged.

This paper is intended to help test the empirical validity of this widely held assumption. First, however, let us set the inquiry in the context of what we already know about the differences in turnouts in primary and general elections for lesser offices and the impact of those differences on the representative quality of the electorates.

I. "Surge and Decline" in Primary and General Elections

In a seminal article published more than a decade ago, Angus Campbell noted that "one of the most dependable regularities of American politics is the vote decline in off-year congressional elections. The turnout in the off-year elections is invariably smaller than in the presidential elections which they follow, usually by a margin of over 25 per cent of the presidential vote."⁸ Moreover, Campbell declared, the off-year electorates are *not* small but representative versions of the presidential-year electorates. They differ significantly in several respects. Using data from the Survey Research Center's studies of voters and nonvoters in the 1956 and 1958 elections, he compared the "core voters" (those who voted in both elections) with the "peripheral voters" (those who voted in 1956 but not in 1958). He found that the core voters were demographically quite unrepresentative of the peripheral voters: they scored significantly higher on the socioeconomic dimensions of age,

⁴ The first proposal was made by Woodrow Wilson in his initial address to Congress in 1913. Others have since been made by Senators George Smathers (1952), Paul Douglas (1953), Estes Kefauver (1953), William Langer (1955), William Proxmire (1960), and Margaret Chase Smith (1961).

⁵ For a useful listing of the state presidential primaries and their outcomes from 1912 to 1964, see James W. Davis, *Presidential Primaries: Road to the White House* (New York: Thomas Y. Crowell Company, 1967), pp. 278–305.

⁶ See, for example, Nelson W. Polsby and Aaron B. Wildavsky, *Presidential Elections: Strategies of American Electoral Politics*, 2nd ed. (New York: Charles Scribner's Sons, 1968), pp. 238–239; Davis, *Presidential Primaries: Road to the White House*, pp. 269–271; Gerald Pomper, *Nominating the President* (New York: W. W. Norton & Company, 1966), pp. 230, 275–276; and Frank J. Sorauf, *Party Politics in America* (Boston: Little, Brown and Company, 1968), p. 273.

⁷ The most praised features of the Oregon presidential primary are the requirements that: (1) *all* serious presidential contenders are placed on their particular party's ballot unless they specifically state that they are not candidates, which is thought to make it difficult for any bona fide contender to duck a hard contest; and (2) the delegates are legally bound to support the preferential poll's winner for one or more convention ballots. At present the primaries in Maryland, New Mexico, North Carolina, Tennessee, and Wisconsin have similar, though not identical, provisions.

⁸ Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (Fall, 1960), 397–418. The article has been reprinted in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, Inc., 1966), pp. 40–62. The latter version is cited here, and the quotation in the text is from page 51.

education, income, and status of occupation; and even more impressive differences appeared on various dimensions of political interest and involvement.

Campbell's analysis deals explicitly only with turnouts in general elections for President and U.S. Representatives, but it offers a promising framework for understanding the consequences of the even greater differences in turnout between primary and general elections delineated by several scholars. V. O. Key, for example, found that in fifteen nonsouthern states from 1926 to 1952 the average voting turnout rate in gubernatorial primaries was 31 per cent, while the turnout in the ensuing general elections averaged well over 50 per cent.⁹ Arthur C. Wolfe collected turnout data for all states' primary and general elections for governor, U.S. Representative, and U.S. Senator for the period 1952–1960, and found primary-general differences comparable to Key's.¹⁰ And my own comparison of turnout rates for primary and general elections for governor and U.S. Senator for the period 1962–1968 shows the same direction and magnitude of differences in turnout.¹¹

It seems reasonable to expect, however, that *presidential* primaries will be different. After all, they play a significant, though not decisive, role in choosing the candidates for our highest and most visible office.¹² Certainly any hotly-

contested primary held when a party's nomination is not foreclosed—e.g., in Wisconsin and West Virginia in 1960, in California in 1964, and in New Hampshire, Wisconsin, Oregon, and California in 1968—receives much more attention from the state and national communications media than all but a handful of gubernatorial and senatorial primaries.

To test this expectation, I have assembled turnout data on the 72 presidential primaries held from 1948 through 1968 in which voters had the opportunity to vote directly (in presidential preference polls) or indirectly (in choosing among slates of delegates pledged to particular aspirants, as in California) for their preferences for the parties' national nominees. For all 72 cases the mean turnout in the primaries was only 27 per cent compared with a mean turnout in the states' ensuing general presidential elections of 62 per cent. This "surge" of 35 percentage points is notably larger than any we have considered up to this point.

The figures, however, are misleading, for in many of the 72 cases there was no contest, or no serious contest, in one or both parties' primaries, and in others the contestants did not include a major national contender in either or both parties. Accordingly, it seems appropriate to look closely only at the genuinely competitive presidential primaries. For selecting such primaries I have used the following criteria: (1) two or more candidates or candidate-pledged slates appear on each party's ballot; (2) at least one major national contender appears on each party's ballot; and (3) in neither party does the winner get more than 80 per cent of the votes. Eleven primaries meet all these criteria, and data on their turnouts are presented in Table 1.

All eleven competitive presidential primaries summarized in Table 1 were concentrated in two years, 1952 and 1968.¹³ The table makes clear that even the competitive primaries produced a primary-to-general election "surge" of 30 percentage points, which is somewhat larger than the comparable surges for gubernatorial and senatorial election pairs described above.

⁹ V. O. Key, Jr., *American State Politics: An Introduction* (New York: Alfred A. Knopf, 1956), pp. 134–138.

¹⁰ Wolfe found a mean turnout rate in all primary elections for governor of 32 per cent, as compared with a mean turnout rate in general gubernatorial elections of 53 per cent. The corresponding means for U.S. Senator were 30 per cent in the primaries and 54 per cent in the general elections; and for U.S. Representatives, 27 per cent in the primaries and 52 per cent in the general elections. See Arthur C. Wolfe, *The Direct Primary in American Politics* (unpublished Ph.D. thesis in political science, University of Michigan, 1966), Table 4.6, p. 102. For comparable data on turnout in primary elections and general elections for U.S. Representatives from 1938 to 1956, see Julian F. S. Foster, *Congressional Primaries* (unpublished Ph.D. thesis in political science, University of California, Los Angeles, 1963), Ch. IV.

¹¹ My data show that in all election-pairs in which both parties' primaries were contested and both parties presented candidates in the general election, the mean turnouts were: gubernatorial primaries, 34 per cent, gubernatorial general elections, 57 per cent; senatorial primaries, 32 per cent, senatorial general elections, 57 per cent. A somewhat revised version of these data is presented in "Parties in State Politics," in Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States: A Comparative Analysis*, 2nd ed. (Boston: Little, Brown and Company, 1971), Table 3, p. 98.

¹² Cf. the discussions of the presidential primaries' role in determining who gets the nominations in David, Goldman, and Bain, *The Politics of National Party Conventions*, Ch. 10; Polsby and Wildavsky, *Presi-*

dential Elections, pp. 83–86; Davis, *Presidential Primaries: Road to the White House*, Chs. 4, 10; and Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward-McCann, Inc., 1970), Chs. 7–10.

¹³ The criterion of not more than 80 per cent of the vote for the winner excluded most primaries: in 1948 either Dewey or Truman or both won over 80 per cent of the vote in one or both contests in every state, and the same was true for Eisenhower in 1956, Nixon in 1960, and Johnson in 1964.

Table 1. Turnout in Competitive Presidential Primaries and Ensuing General Elections, 1948-1968

State	Year	Per Cent Turn-out in Primary*	Per Cent Turn-out in Gen. Elect.*	Difference
California	1952	42	68	26
Nebraska	1952	39	73	34
New Hampshire	1952	37	78	41
Ohio	1952	25	70	45
Oregon	1952	45	67	22
Pennsylvania	1952	19	66	47
South Dakota	1952	41	73	32
Nebraska	1968	43	63	20
New Hampshire	1968	38	71	33
Oregon	1968	55	66	11
Wisconsin	1968	49	69	20
Mean turnout		39	69	30

* Turnout percentage is calculated, as in most studies, by dividing the total number of votes cast by the estimated population of voting age.

Sources: Primary election returns, 1948-1964, are taken from James W. Davis, *Presidential Primaries: Road to the White House* (New York: Thomas Y. Crowell Company, 1967), pp. 296-305. All general election returns and primary election returns for 1968 are taken from various volumes of Richard M. Scammon, ed., *America Votes* (Washington, D.C.: Governmental Affairs Institute and Congressional Quarterly). Estimates of populations of voting age for the states are taken from the *Statistical Abstract of the United States, 1969* (Washington, D.C.: Bureau of the Census, 1969), Table 534, p. 369.

In short, in turnout, at least, presidential primaries are species of the genus primary elections, not of the genus presidential elections.

But what are the consequences, if any, of their persistently low turnouts for the demographic and preferential *representativeness* of presidential primaries?¹⁴ Studies of electorates in other types of low-turnout elections give us some clues. We have already noted Angus Campbell's conclusion that the "core voters" in off-year congressional elections are socioeconomically of higher status and politically more

¹⁴ As the terms are used in this paper, "demographic representativeness" denotes the degree of likeness between the distributions of socioeconomic characteristics among the representatives (in this case, voters in the primaries and other low-turnout elections) and the corresponding distributions among the represented (in this case, the eligible but nonvoting citizens). "Preferential representativeness" denotes the degree of likeness between the distributions of preferences for candidates and attitudes on public issues among the representatives and the corresponding distributions among the represented.

interested and involved than the "peripheral voters" who join them in presidential-year elections. Campbell did not ask whether the two electorates' preferences on issues and candidates differ significantly, though he did suggest that standing party loyalties are relatively more powerful in the low-turnout elections.¹⁵ Our studies of voters and nonvoters in the Wisconsin gubernatorial primary and general elections of 1964 and 1966 concluded that the primary electorates were demographically unrepresentative but preferentially representative of both the general-election-only voters and the nonvoters in all four elections.¹⁶

So the question becomes: is the representative quality of presidential primaries simply an instance of that of primary elections in general; or are significant differences wrought in presidential primaries by the greater importance and visibility of the presidency and by some special characteristics of presidential primary laws? Let us see.

II. Voters and Nonvoters in Two 1968 Presidential Primaries

The Data. The data for the following analysis of voters and nonvoters in presidential primaries are taken from sample surveys of participants and nonparticipants in the primaries held in New Hampshire and Wisconsin in 1968. I would, of course, like to have had survey data for the Indiana, Nebraska, Oregon, California, and South Dakota primaries as well, but they were not available. Fortunately, however, not only were the New Hampshire and Wisconsin studies available, but they also had several other advantages: both were conducted by highly reputable survey research organizations;¹⁷ comparisons of their results with the

¹⁵ Campbell, "Surge and Decline . . .," pp. 53-57. See also Angus Campbell and Homer C. Cooper, *Group Differences in Attitudes and Votes* (Ann Arbor: Survey Research Center, University of Michigan, 1956), Ch. IV.

¹⁶ Austin Ranney and Leon D. Epstein, "The Two Electorates: Voters and Nonvoters in a Wisconsin Primary," *The Journal of Politics*, 28 (August, 1966), 598-616; and Austin Ranney, "The Representativeness of Primary Electorates," *Midwest Journal of Political Science*, 12 (May, 1968), 224-238.

¹⁷ The New Hampshire study was conducted by Audits and Surveys, Inc., of New York City. A statewide probability sample of registered Democratic and Republican voters was interviewed, yielding a total of 1143 usable interviews. The data are made available by the Inter-University Consortium for Political Research, and the Social Science Data and Program Library Service of the University of Wisconsin. The Wisconsin study was conducted by the Wisconsin Survey Research Laboratory, using a statewide clustered area probability sample of persons of voting age and yielding a total of 573 usable interviews. The data are made available by the WSRL.

election returns suggest that both samples were well drawn; and the primaries' outcomes—both predicted and recorded—were, correctly or not, widely regarded as a major influence on Lyndon Johnson's decision not to run for reelection.¹⁸

When considering the findings reported below, one should bear in mind two differences in the New Hampshire and Wisconsin studies. First, the New Hampshire data came from interviews taken *before* the March 12 election (in the period February 12–19), while the Wisconsin data came from interviews taken just *after* the April 2 election. Second, the New Hampshire sample consisted entirely of registered voters, while the Wisconsin sample, like those in the 1964 and 1966 gubernatorial primary studies, was drawn from the state's entire population of voting age, registered or not.

Mode of Analysis. The data were analyzed as follows. Since most (though not all) political leaders and commentators feel that presidential primaries should represent each party's rank-and-file, and not all citizens regardless of party identification or "membership," the two samples were first divided into Republicans and Democrats (those expressing some degree of preference for one party or the other) and Independents (those expressing no party preference of any degree).¹⁹ The New Hampshire sample yielded 656 Republicans, 380 Democrats, and 62 Independents; in Wisconsin the numbers were 197 Republicans, 281 Democrats, and 52 Independents. Next, each party-identification group in New Hampshire was divided, according to expressed intention to vote or not vote in the upcoming primary, into "intenders" and "nonintenders."²⁰ Comparable di-

visions were made in the Wisconsin sample, except that the party identifiers were divided into "voters" (those who reported having voted in the primary) and "nonvoters" (those who reported not having voted in the primary). This resulted in the following twelve basic analytic groups:

New Hampshire

Republican intenders (RI): 485
Republican nonintenders (RN): 171
Democratic intenders (DI): 251
Democratic nonintenders (DN): 129
Independent intenders (II): 33
Independent nonintenders (IN): 29

Wisconsin

Republican voters (RV): 134
Republican nonvoters (RN): 63
Democratic voters (DV): 158
Democratic nonvoters (DN): 123
Independent voters (IV): 28
Independent nonvoters (IN): 24

As in the 1966 study, in each state each party's participants (intenders or voters) were compared with its nonparticipants (nonintenders and nonvoters) on a number of socioeconomic, attitudinal, and behavioral measures.²¹ For each intraparty participant/nonparticipant comparison the null hypothesis was that the distribution in the sample does *not* support the

of New Hampshire's registered voters voted in the primary, it seemed reasonable to classify those answering "very certain" as intenders, and those giving the other answers as nonintenders. After removing the respondents whose party identifications were not ascertained, there remained 769 intenders and 329 nonintenders.

²¹ Some colleagues have suggested that the representativeness of presidential primary electorates would be better tested by comparing the party identifiers who vote in a state's primary with *all* who identify with that party in the state: for example, RI/RN + RN rather than RI/RN. They correctly point out that this approach would narrow the participant/nonparticipant differences reported in this study, and thus enhance the primaries' claim to be truly representative. However, I have chosen the straight participant/nonparticipant comparison because I believe it more accurately operationalizes the concept of representation held by most party leaders who regard the primaries as the nominating system's most representative element. From what they say and do, on the McGovern-Fraser Commission and elsewhere, it seems clear that most leaders assume a sharp distinction between the "representatives" (the convention delegates, the primary voters) and the "represented" (the party's grass-roots supporters). If, as I believe, this concept of representation permeates most current political dispute about the proper organization and role of primaries and conventions, then accepting it rather than substituting a concept which regards the representatives as part of the represented seems likely to make our empirical findings more relevant to those disputes.

¹⁸ Cf. Theodore H. White, *The Making of the President 1968* (New York: Atheneum Publishers, 1969), Ch. 4; and Lewis Chester, Godfrey Hodgson, and Bruce Page, *An American Melodrama: The Presidential Campaign in 1968* (New York: Dell Publishing Company, 1969), pp. 1–10.

¹⁹ Both studies used the Survey Research Center's familiar battery of questions for eliciting party identification.

²⁰ All New Hampshire respondents were asked before the primary, "As of now, how certain are you that you will vote in the primary this year?" The replies were distributed as follows:

Response	Number	Per Cent
Very certain	796	69.6
Somewhat certain	155	13.6
Not at all certain	107	9.4
Will not vote	54	4.7
Don't know	23	2.0
Not ascertained	8	0.7
Total	1143	100%

Since the election returns showed that 67.0 per cent

conclusion that participants in the population differ significantly from nonparticipants, and "significance," in the usual statistical sense, is assessed by the chi-square test, with the .05 level stipulated.

III. Demographic Representativeness

The two data sets analyzed here were produced by studies of different populations conducted by different research organizations at different times under different research designs. Consequently, on some dimensions we have identical or closely comparable measures, but on others we do not. It is therefore necessary to divide the findings under each heading into those produced for both states by comparable measures and those produced for each state by noncomparable measures. The findings with respect to demographic representatives are summarized schematically in Table 2.

The data summarized in Table 2 show that the party-identifying primary participants in both states differed significantly from their nonparticipating fellow partisans on a number of socioeconomic dimensions, while there were substantially fewer significant differences between participating and nonparticipating independents. We shall observe this same pattern

on most of our other measures of representativeness as well.

There were too few Blacks in either state's sample to produce any statistically significant differences in the black-white distributions of the six participant-nonparticipant pairs, and in Wisconsin there were no significant differences in the national origins of voters and nonvoters. However, in the light of the McGovern-Fraser Commission's finding that both young people (defined by the commission as those under thirty) and women were badly underrepresented in the 1968 Democratic National Convention,²² the figures in Table 3 on age and sex distributions are of some interest. A scan of Table 3 leaves no doubt that, while young people were only slightly underrepresented in the New Hampshire primaries, they were significantly underrepresented in both parties' primaries in Wisconsin. Women, on the other hand, were only slightly underrepresented in the New Hampshire primaries, while in Wisconsin they outnumbered men in both primaries. This may reflect the facts that in both states higher proportions of men than of women were registered voters, but in New Hampshire the sample con-

²² *Mandate for Reform*, pp. 27-29.

Table 2. Socioeconomic Characteristics of Presidential Primary Participants and Nonparticipants

Socioeconomic Characteristic	New Hampshire			Wisconsin		
	RI/RN	DI/DN	II/IN ^a	RV/RN	DV/DN	IV/IN ^a
<i>Comparable Measures</i>						
Age	+ ^b	0 ^c	0	+	+	0
Sex	0	0	0	0	0	0
Education	+	0	0	0	0	0
Occupation	0	0	0	+	0	0
Income	+	0	0	+	+	0
Religious preference	0	0	0	0	0	0
Frequency of church attendance	+	+	0	0	+	+
Size of place	0	0	0	0	0	0
Union membership	0	0	0	0	0	0
<i>Non-comparable Measures: N.H.</i>						
Self-assigned social class	0	0	0			
<i>Non-comparable Measures: Wis.</i>						
Membership in party clubs				+	0	0
Membership in church clubs				+	+	+
Membership in fraternal organizations				+	0	0
Membership in social clubs				+	0	0
Membership in PTAs				+	+	0
Length of residence in community				+	+	0
Home ownership				+	+	0
Original nationality on father's side				0	0	0

^a For definitions of the groups compared, see the text, p. 110

^b Participants score significantly higher than nonparticipants.

^c Difference between participants and nonparticipants not significant at the .05 level.

Table 3. Distributions of Presidential Primary Participants and Nonparticipants, by Age and Sex

New Hampshire							Wisconsin						
	RI	RN	DI	DN	II	IN		RV	RN	DV	DN	IV	IN
Age							Age						
21-24	2%	5%	2%	1%	3%	0%	21-25	3%	16%	4%	17%	4%	17%
25-29	5	5	2	3	6	7	26-30	9	8	9	10	4	8
30-34	6	6	5	8	15	0	31-35	5	5	9	13	7	8
35-44	18	12	22	16	27	17	36-45	31	13	24	15	25	21
45-54	19	14	21	16	9	31	46-55	21	17	22	15	18	8
55-64	17	22	17	19	15	21	56-65	14	16	14	9	21	25
65 and over	29	31	26	34	18	21	66 and over	17	25	18	19	21	13
n. a.	4	5	5	3	7	3	n. a.	0	0	0	2	0	0
	100%	100%	100%	100%	100%	100%		100%	100%	100%	100%	100%	100%
N	485	171	251	129	33	29		134	63	158	123	28	24
	$X^2=10.65^*$ df=6 $p=n.s.$		$X^2=7.18$ df=6 $p=n.s.$		$X^2=10.11$ df=6 $p=n.s.$			$X^2=17.41$ df=6 $p<.01$		$X^2=19.17$ df=6 $p<.01$		$X^2=4.47$ df=6 $p=n.s.$	
Sex							Sex						
Female	37%	43%	37%	42%	36%	24%	Female	52%	59%	57%	55%	39%	46%
Male	63	57	63	58	64	76	Male	48	41	43	45	61	54
	100%	100%	100%	100%	100%	100%		100%	100%	100%	100%	100%	100%
	$X^2=1.67$ df=1 $p=n.s.$		$X^2=.77$ df=1 $p=n.s.$		$X^2=.59$ df=1 $p=n.s.$			$X^2=.49$ df=1 $p=n.s.$		$X^2=.03$ df=1 $p=n.s.$		$X^2=.04$ df=1 $p=n.s.$	

* "Not ascertained" row eliminated before calculating chi squares.

sisted of registered voters, while the Wisconsin sample consisted of resident adults. Or it may reflect the greater proportion of women to men in Wisconsin (47 to 53 per cent) than in New Hampshire (43 to 57 per cent) in 1968. Or it may reflect a greater liberation from male chauvinist attitudes about political participation in Wisconsin than in New Hampshire. The data do not tell us, but they do suggest that the 1968 presidential primaries did a better job of representing women than of representing people under thirty, and a better job of representing both groups than that accomplished by the national conventions.

Otherwise, the data confirm that presidential primary electorates, like those in gubernatorial primaries, are demographically quite unrepresentative of the nonparticipating party identifiers: they are older, of higher status in income and occupation, and more active in a variety of civic, religious, and political organizations.

IV. Representativeness in Partisanship

Lester W. Milbrath declares that "at least nine studies in three or more countries have shown that *persons who strongly identify with or intensely prefer a given party are more likely to participate actively in the political process.*"²³

²³ Lester W. Milbrath, *Political Participation* (Chicago: Rand McNally & Company, 1965), p. 52, emphasis in the original.

The explanation usually given is that most citizens find the political world hard to fathom and are unwilling or unable to pay the high costs of acquiring full information. Hence the party labels attached to particular candidates together with the citizens' standing party identifications provide the most powerful cues for making choices. In high-stimulus presidential elections, intensity of party identification is more strongly related to the way a person would vote if he voted than to the likelihood of his voting; but in low-stimulus off-year congressional elections, strength of partisanship is also strongly related to voting turnout.²⁴

²⁴ Statements of the underlying causes for the partisanship-participation relationship are given by Don R. Bowen, *Political Behavior of the American Public* (Columbus, Ohio: Charles E. Merrill Publishing Company, 1968), p. 129; and in Leon D. Epstein's discussion of the general role of party labels in "structuring the vote," in *Political Parties in Western Democracies* (New York: Frederick A. Praeger, Publishers, 1967), Ch. IV. The Survey Research Center's findings on the relationship in presidential elections are set forth in Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (New York: Harper and Row, 1954), pp. 107-110, esp. Table 7.11; and in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), pp. 142-143. The SRC's findings on the corresponding relationship in off-year congressional elections are given in Campbell and Cooper, *Group Differences in Attitudes and Votes*, Ch. IV.

Table 4. Intensity of Party Identification and Primary Participation, by Party and State

Wisconsin							
Voted in primary?	Republicans			Independents	Democrats		
	Strong Rep.	Weak Rep.	Ind. Rep.		Strong Dem.	Weak Dem.	Ind. Dem.
Yes	75%	63%	70%	54%	60%	53%	59%
No	25	37	30	46	40	47	41
	100%	100%	100%	100%	100%	100%	100%
N	59	88	50	52	68	139	74
	X ² = 2.53 df = 2 p = n.s.				X ² = 1.23 df = 2 p = n.s.		
New Hampshire							
How certain to vote in primary?	Republicans			Independents	Democrats		
	Strong Rep.	Weak Rep.	Ind. Rep.		Strong Dem.	Weak Dem.	Ind. Dem.
Very certain	77%	72%	69%	53%	75%	60%	62%
Other*	23	28	31	47	25	40	38
	100%	100%	100%	100%	100%	100%	100%
N	304	275	77	62	142	156	82
	X ² = 2.20 df = 2 p = n.s.				X ² = 8.33 df = 2 p < .02		

* Somewhat certain, not at all certain, will not vote.

One may reasonably derive from these findings about general elections the hypothesis that strong party identifiers are more likely than weak identifiers to vote in low-stimulus primary elections. Yet our gubernatorial-primary studies did not confirm this hypothesis: we found that, while Strong Republicans are somewhat more likely than Weak Republicans to vote in such primaries, among Democrats there was even a weak *inverse* relationship between intensity of party identification and participation in the primary.²⁵ The data summarized in Table 4 suggest that, with one exception, intensity of partisanship played no stronger a role in impelling people to vote in the 1968 presidential primaries than it had in the 1964 and 1966 Wisconsin gubernatorial primaries. Of the party groupings shown in the table, only the New Hampshire Strong Democrats voted in the primary in a significantly higher proportion than their less intensely identified fellow partisans. Some

doubt arises about the significance of even this finding since the New Hampshire respondents were all registered voters, in itself an indication of a general interest and involvement in politics.²⁶ The doubt is increased by the finding that when a control is imposed for the respondents' expressed interest in the *particular* 1968 primary (as opposed to a general interest in all elections), the New Hampshire Democrats' relationship between intensity of partisanship and primary participation/nonparticipation weakens and fails to meet our test of significance at the .05 level. In sum, strong partisans of both parties are only slightly more likely than weaker partisans to vote in presidential primaries, and the relationship between intensity of

²⁵ For two somewhat different analyses of the interaction of institutional requirements, including registration laws, and levels of psychological motivation as they affect voting turnout, see *The American Voter*, Ch. 11; and Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June, 1967), 359-379.

²⁶ See the studies mentioned in footnote 16.

party identification and voting in such primaries is much weaker than that between partisanship and voting in presidential general elections.

V. Preferential Representativeness

The McGovern-Fraser Commission contends, correctly in my view, that demographic representativeness is an important standard by which to measure the viability of the conventions, the primaries, and the whole presidential nominating process. Quite aside from the quality, acceptability, and electoral appeal of the nominee chosen, if important segments of the party's identifiers and supporters or of the community at large feel that the nominating process and machinery do not want their presence or hear their voices, the legitimacy of the nomination and of the process itself is likely to be challenged and perhaps denied. If that happens, the consequences for the party—and, indeed, for the whole political system—could be catastrophic.

Even so, *preferential* representativeness seems to many a still more important standard. For even if the primaries were dominated by and the conventions composed largely of middle-class white males, if they consistently wrote platforms and chose candidates desired by the bulk of the party's supporters, the probabilities of major revolts against the nominating process or its products would be greatly reduced. So the question of the preferential representativeness of presidential primary electorates is nearer the heart of the matter than anything we have considered up to now.

Issue Orientation. One prime element of preferential representativeness is issue orientation: the degree of correspondence between the feelings of representatives and the feelings of those represented about what government should and should not do. The 1968 studies give us considerable data on the preferential representativeness of presidential primary electorates, although, unfortunately, none resulting from comparable measures used in both states. The New Hampshire study dealt mainly with questions about salient national political issues, asking a total of ten questions tapping views about the importance of issues, four questions concerning issues of national domestic policy, and five questions about national policy in Vietnam.

There were no statistically significant differences within any of New Hampshire's three participation pairs concerning the importance of the various issues, but some noteworthy differences in policy preferences did appear. The

less striking differences came over domestic issues, as is shown by the answers arrayed in Table 5. Table 5 reveals that, while on each domestic issue a majority of the Republican primary participants agreed with a majority of the Republican nonparticipants, the participants had higher proportions taking conservative stands on all four issues, and on two the proportions were sufficiently different to produce chi-square scores significant at the .05 level. Democratic participants and nonparticipants, on the other hand, held quite similar views on three of the four domestic issues; but on the fourth, a majority of the participants opposed national protection of equal job opportunities for Negroes, while a plurality of the nonparticipants supported such protection. On this important issue, then, Democratic primary participants were distinctly *unrepresentative*.

The four questions on the Vietnam war, however, produced the most significant participant/nonparticipant issue differences that have appeared in this study and in its 1964 and 1966 predecessors. The differences are shown in Table 6. One pattern evident in Table 6 is that Republican participants and nonparticipants agreed that present and past Democratic administrations had done things all wrong: Pluralities of both groups felt that we should not have entered the Vietnam fighting in the first place, and majorities of both disapproved of President Johnson's current handling of the situation. Pluralities of both groups also opposed any unconditional halt of the bombing of North Vietnam; but on the general question of what policy the nation should follow, the Republican participants were distinctly more hawkish than the nonparticipants: 45 per cent favored a stronger stand, and 42 per cent favored efforts to end the fighting; among the nonparticipants, on the other hand, only 30 per cent favored a stronger stand and 50 per cent favored efforts to end the fighting.

Over on the Democratic side, the participant/nonparticipant differences on the war issues were even more pronounced. A majority of the Democratic intenders approved our initial entry into the war, while a plurality of the nonintenders disapproved it; the participants approved their party chief's handling of the war by a margin of 56 per cent to 31 per cent, while the nonparticipants supported him by only 43 per cent to 39 per cent. On questions of future policy, the participants, like their Republican counterparts, were somewhat more hawkish: 53 per cent approved taking a stronger stand, compared with 42 per cent of the nonparticipants; and 86 per cent opposed

Table 5. Opinions on Domestic Issues of New Hampshire Presidential Primary Intenders (I) and Nonintenders (N), 1968

Question	Reps.		Dems.		Inds.	
	I	N	I	N	I	N
"Do you think the Government in Washington should help towns and cities provide education for grade and high school children, or do you think this should be handled by the states and local communities?"						
Govt. in Washington should help	14%	26%	38%	36%	27%	24%
Leave to states & localities	79	58	54	48	67	62
Don't know, not ascertained	7	16	8	16	6	14
	100%	100%	100%	100%	100%	100%
Number of cases	485	171	251	129	33	29
	$X^2 = 15.85^*$ df = 1 $p < .001$		$X^2 = .02$ df = 1 $p = n.s.$		$X^2 = .05$ df = 1 $p = n.s.$	
"Do you think the Government in Washington should see to it that every person has a job and a good standard of living, or should it let each person get ahead on his own?"						
Govt. in Washington should help	15%	25%	39%	35%	21%	31%
Let each get ahead on his own	78	61	55	57	73	48
Don't know, not ascertained	7	14	6	8	6	21
	100%	100%	100%	100%	100%	100%
	$X^2 = 10.35$ df = 1 $p < .01$		$X^2 = .44$ df = 1 $p = n.s.$		$X^2 = 1.03$ df = 1 $p = n.s.$	
"Do you think civil rights leaders are trying to push too fast, are going too slowly, or are they moving at about the right speed?"						
Too fast	64%	59%	61%	53%	67%	69%
Too slowly	4	7	6	10	0	7
About the right speed	26	28	28	29	30	14
Don't know, not ascertained	6	6	5	8	3	10
	100%	100%	100%	100%	100%	100%
	$X^2 = 2.19$ df = 2 $p = n.s.$		$X^2 = 2.21$ df = 2 $p = n.s.$		$X^2 = 4.09$ df = 2 $p = n.s.$	
"Do you think the Government in Washington should see to it that Negroes get fair treatment in jobs, or should these matters be left to the states and local communities?"						
Govt. in Wash. should guarantee fair job treatment for Negroes	32%	35%	43%	49%	30%	31%
Leave to states & localities	58	52	50	41	64	52
Don't know, not ascertained	10	13	7	10	6	17
	100%	100%	100%	100%	100%	100%
	$X^2 = .75$ df = 1 $p = n.s.$		$X^2 = 1.75$ df = 1 $p = n.s.$		$X^2 = .01$ df = 1 $p = n.s.$	

* "Don't know, not ascertained" rows eliminated before calculating chi squares.

Table 6. Opinions on Vietnam War Issues of New Hampshire Presidential Primary Intenders (I) and Nonintenders (N), 1968

Question	Reps.		Dems.		Inds.	
	I	N	I	N	I	N
"Do you think we did the right thing in getting into the fighting in Vietnam, or should we have stayed out?"						
Did the right thing	45%	37%	59%	44%	58%	38%
Should have stayed out	47	52	38	47	39	45
Don't know, not ascertained	8	11	3	9	3	17
	100%	100%	100%	100%	100%	100%
Number of cases	485	171	251	129	33	29
	$X^2 = 1.69^*$ df = 1 $p = n.s.$		$X^2 = 4.81$ df = 1 $p < .05$		$X^2 = .54$ df = 1 $p = n.s.$	
"Do you approve or disapprove of the way President Johnson is handling the situation in Vietnam?"						
Approve	20%	27%	56%	43%	27%	34%
Disapprove	61	50	31	39	45	52
Don't know, not ascertained	19	23	13	18	28	14
	100%	100%	100%	100%	100%	100%
	$X^2 = 5.74$ df = 1 $p < .02$		$X^2 = 4.49$ df = 1 $p < .05$		$X^2 = .01$ df = 1 $p = n.s.$	
"Which of the following do you think we should do now in Vietnam?"						
Pull out entirely	9%	14%	8%	9%	12%	10%
Keep our soldiers in Vietnam, but try to end the fighting	33	36	26	30	27	28
Take a stronger stand, even if it means invading North Vietnam	45	30	53	42	48	48
Don't know, not ascertained	13	20	13	19	13	14
	100%	100%	100%	100%	100%	100%
	$X^2 = 9.28$ df = 2 $p < .01$		$X^2 = 2.53$ df = 2 $p = n.s.$		$X^2 = .04$ df = 2 $p = n.s.$	
"Which of the following do you think we should do now with regard to the bombing of North Vietnam?"						
Halt the bombing immediately, even if it doesn't lead to negotiations	7%	6%	8%	9%	15%	0%
Halt the bombing immediately and see if it leads to negotiations	12	20	6	14	12	3
Halt the bombing now only if North Vietnam agrees to negotiate	42	35	40	33	45	38
Do not halt the bombing until North Vietnam surrenders	34	30	43	35	24	59
Don't know, not ascertained	5	9	3	9	4	0
	100%	100%	100%	100%	100%	100%
	$X^2 = 8.02$ df = 3 $p < .05$		$X^2 = 8.47$ df = 3 $p < .05$		$X^2 = 10.53$ df = 3 $p < .02$	

* "Don't know, not ascertained" rows eliminated before calculating chi squares.

any unilateral bombing halt, compared with 68 per cent of the nonparticipants.

Thus in both parties, but more among the Democrats than the Republicans, the New Hampshire primary participants were distinctly more hawkish than the nonparticipants, although on only two questions did a plurality of the intenders take a position different from that favored by a plurality of the nonintenders. We shall consider below some possible consequences of these differences for the primaries' outcomes.

The Wisconsin respondents, unfortunately, were asked only about state and local issues. As in New Hampshire, there was no significant difference within any of the three pairs of voters and nonvoters about the relative importance of the various issues facing the state. On four questions dealing with environmental issues, the RVs and RNs and the IVs and INs showed no significant differences, but on one of the four the DVs were significantly more willing than the DNs to "pay somewhat more in taxes if the money could be used to help clean up Wisconsin lakes and streams." On three questions about the state's welfare programs and their beneficiaries, the DVs and DNs and the IVs and INs showed no significant differences; but significantly more RVs than RNs disagreed with the proposition that "people who need assistance from a social welfare agency are really good people who have had a bad break in their lives."

Since the small wars in Madison were then second in public concern only to the large war in Vietnam, the survey also contained four questions about various aspects of the way the University of Wisconsin was handling its student affairs, and these evidently touched some Republican (though no Democratic or Independent) nerves. The two groups of Democrats and the two groups of Independents all agreed that the University had been too lenient in making and enforcing rules governing student activities, but the differences in the proportions holding these views did not meet the .05 test. On all four questions, however, the RVs were significantly more critical of University permissiveness than were the RNs. And on the question, "To what degree do you approve or disapprove of the gradual relaxing of regulations governing student hours, visiting privileges in residence halls, and apartment living at the U.W.?", 54 per cent of the RNs expressing an opinion approved relaxing the regulations, while 80 per cent of the RVs disapproved. Had these data been available to Madison students in September, 1970, perhaps many more would

have worked to get out the vote—especially in the Republican primaries, and for candidates identified with other causes as well as with peace. But they were not, and U.W. students, like the Adult Establishment, were less inactive in the 1970 November general election than in the September primaries.

Candidate Preferences. Many commentators and most political leaders hold that the prime, if not the only, function of a party's presidential nominating process is to produce the candidate most likely to win in November. Some, however, believe that its prime function is to produce the candidate most preferred by the party's ordinary adherents whatever may be their reasons or the wisdom of their choice. In either case, perhaps the most critical test of a convention's or a primary's preferential representativeness is whether the participants choose the candidate the party's identifiers most want. The record of the 1968 New Hampshire and Wisconsin primaries on this test is set forth in Tables 7 and 8.

Table 7 makes it quite clear that the New Hampshire Republican intenders' preferences not only closely resembled those of the Republican nonintenders, but also came very close to the choices actually registered in the primary balloting. This finding both increases our confidence in the sample and suggests that the Republican primary intenders well represented the candidate preferences of nonintending registered Republicans: more than three-quarters of both groups wanted Nixon, and Nixon they got.

The New Hampshire Democratic intenders, on the other hand, not only gave Lyndon Johnson a significantly larger proportion of their votes than did the Democratic nonintenders (and Eugene McCarthy a correspondingly smaller proportion), but they were also substantially more pro-Johnson than were the voters in the primary. These discrepancies may, of course, result from a biased sample or a poorly-conducted set of interviews. But they may support the opinion of some commentators that a considerable swing away from Johnson to McCarthy took place in the three weeks between the date of the last interviews (February 19) and primary day (March 12).²⁷ If that

²⁷ Chester, Hodgson, and Page state that private polls commissioned from Quayle Associates by the Johnson organization showed a substantial surge for McCarthy in the final three weeks of the campaign: *An American Melodrama*, pp. 110–112. White cites other private polling data showing a steady rise in McCarthy support from January on: *The Making of the President 1968*, p. 89.

Table 7. Candidate Preferences of Intenders and Nonintenders in the 1968 New Hampshire Presidential Primary

Candidate	Election Returns	Sample							
		All		Reps.		Dems.		Inds.	
		I	N	I	N	I	N	I	N
Johnson	18.4%	20.8%	19.9%	0.4%	0.6%	63.1%	47.7%	20.0%	16.0%
McCarthy	18.1	8.4	15.1	0.9	0.6	24.3	37.8	4.0	4.0
Nixon	52.2	54.0	45.4	77.6	76.1	4.5	4.5	60.0	36.0
Rockefeller	7.0	5.9	6.2	8.7	9.7	0.5	2.8	4.0	0
Romney	1.1	7.1	6.9	9.8	9.7	1.3	0	8.0	20.0
Other	3.2	3.8	6.5	2.6	3.3	6.3	7.2	4.0	24.0
	100%	100%	100%	100%	100%	100%	100%	100%	100%
N	159,402	707	291	460	155	222	111	25	25 ^a
				$X^2 = .30^a$		$X^2 = 7.88^b$			
				df = 3		df = 2			
				$p = n.s.$		$p < .02$			

^a Chi square calculated for Republican candidates only.

^b Chi square calculated for Democratic candidates only.

^c Too few cases for chi-square calculation.

was the case, we cannot say that the data for Democratic intenders and nonintenders in Table 7 prove that the New Hampshire Democratic primary electorate in 1968 was unrepresentative of the party rank-and-file. Perhaps the best judgment for our null hypothesis here is the old Scottish verdict of "not proved."

The Wisconsin data in Table 8 show a striking correspondence between the preferences reported by the voters in our sample and those cast in the actual election. The sample nonvoters were not directly asked their preferences, however, so we have to approach the representativeness question indirectly. Each respondent

was asked to rate each of the candidates on a scale from 0 (don't like him at all) to 100 (like him very much). We were thus able to construct a composite rating of each candidate for each of our analytic groups and compare them between groups. The results are set forth in Table 9.

Comparisons of the rank orders of the candidate ratings summarized in Table 9 reveal no significant differences between Republican voters and non-voters. Inspection of the individual composite-rating scores suggests that the RVs liked Nixon and McCarthy better than the RNs did, and liked Johnson and Kennedy less; but

Table 8. Candidate Preferences of Wisconsin Sample and Voters, 1968 Presidential Primary

Candidate	Election Returns	Sample			
		All	Republicans	Democrats	Independents
Johnson	20.7%	20.8%	2.3%	38.1%	13.6%
McCarthy	33.7	34.9	20.9	46.3	40.9
Kennedy	3.8	7.4	2.3	10.9	13.6
Nixon	31.9	31.2	65.1	2.0	27.3
Reagan	4.2	1.7	3.9	0	0
Other	4.2	3.0	4.7	2.0	0
No preference*	1.5	1.0	0.8	0.7	4.6
	100%	100%	100%	100%	100%
N	1,222,855	298	129	147	22

* A "no preference" option was offered on the election ballot as well as on the interview schedule.

Table 9. Ratings of Presidential Candidates by Wisconsin Primary Voters and Nonvoters

Candidate	REPUBLICANS					DEMOCRATS					INDEPENDENTS	
	Voters		Nonvoters			Voters		Nonvoters			Voters	
	Rank Order of Votes	Rating	Rank Order of Rating	Rank Order of Rating	Rank Order of Rating	Rank Order of Votes	Rating	Rank Order of Rating	Rank Order of Rating	Rank Order of Rating	Rank Order of Votes	Rating
McCarthy	2	46.3	2	45.9	2	1	57.6	1	55.6	2	1	58.9
Johnson	3*	25.2	4	37.2	4	2	52.8	2	44.9	3	3*	30.0
Kennedy	3*	30.4	3	44.9	3	3	52.1	3	61.1	1	3*	45.0
Nixon	1	66.3	1	62.3	1	4	29.0	4	27.1	4	2	51.8

* Tied rank.

both groups' rank orders in ratings were the same, and also the same as the rank order (except for the Johnson-Kennedy tie) that appeared in the RVs' reported votes. So there is little here to suggest that the RVs were significantly unrepresentative of RNs in their attitudes toward and preferences among the candidates. The same is true of the IVs and INs.

The Democrats, however, do not present so neat a pattern. Not surprisingly, the rank order of the DVs' candidate ratings is the same as for their reported votes; but the DNs' individual rating scores and the rank order of their ratings show much greater approval of Robert Kennedy and disapproval of Lyndon Johnson than shown by the DVs. In this sense, then, the DVs distinctly underrepresented the Kennedy sentiment among all Wisconsin Democrats and (strange as it may seem in the light of the election outcome) overrepresented the Johnson strength. We cannot say with confidence to what extent this distortion reflected the respondents' post-election knowledge that Johnson had withdrawn from the national race (as he did on March 31st, two days before the primary) and Kennedy had entered it (on March 16th, seventeen days before the primary). Yet it certainly raises the question of what damage is done to the representativeness of presidential primaries by laws that make it possible—indeed, likely—that only some serious national contenders will be on the ballots and others' names will have to be written in if they are to receive any votes at all.²⁸

²⁸ In 1968 the New Hampshire Republican primary ballot carried the names of Nixon, Romney (who withdrew from the national contest on February 28, thirteen days before the primary, but whose name remained on the ballot), Harold Stassen, Willis Stone, Herbert F. Hoover, David Watumull, William Evans, Elmer Coy, and Don Dumont. There were also Republican write-in votes for Rockefeller, McCarthy, and Johnson. The Democratic primary ballot carried the names of McCarthy, John Crommelin, Richard Lee, and Jacob Gordon; and write-in votes were re-

VI. Wisconsin's Cross

In 1968 Wisconsin was the only primary state using the open primary law provided that every qualified voter entitled to vote in the primary (but not in both) without purchasing his choice. The La Follette provision of this provision into Wisconsin laws in the early 1900s, and it effectively challenged or lost its support.²⁹

Most political analysts and scholars prefer closed primaries to the open primary. After all, they argue, the labels constitute the main signal to many voters through the maze of long ballots; hence choosing a party shall and shall not bear its official stamp, the most important decision made in that seriously contests elections. In this case, it seems only logical and the right to participate in mass nominations to bona fide parties, however defined. Closed primaries to achieve this goal, but open primaries, are designed to favor the "over"—voting in one party's primaries of an opposing party or of

Several scholars have speculated on the extent, motivation, and impact

recorded for Johnson and Nixon. In the new Oregon-style law, the state publican ballot Nixon, Reagan, State Rockefeller (who withdrew but votes). McCarthy and Johnson were Democratic ballot, and there were votes for Kennedy, Humphrey, and George Polsby and Wildavsky for an illustration of the strategy of write-in campaigns. *Elections*, pp. 84-85.

²⁹ In our study of the 1966 Wisconsin primary we asked our respondents whether they were changing the open primary, 82 per cent saying it as it is, and only 9 per cent saying it into a closed primary.

open-primary elections. Some have pointed to instances in which the total vote in a particular party's primary exceeded its vote in the general election, and then drawn the not unreasonable inference that the discrepancy resulted from extensive crossovers.³⁰ One motivation often ascribed for crossing over is the desire of the adherents of one party—especially the second party in a modified one-party system—to help the best (or least bad) of the other party's aspirants win its nomination.³¹ Another, quite different motivation sometimes ascribed is said to underlie the practice of "raiding"—adherents of one party participating in the other party's primary "to assure the nomination of a weak candidate who can be defeated in the general election."³²

Survey data clearly have many advantages over aggregate data for studying both the extent of and motivations for crossing over. The Wisconsin data in Table 8, for example, show that more than one-fifth of the sample's Republican voters (20.9 per cent) reported crossing over to vote for McCarthy. Adding the 2.3 per cent who voted for Johnson and the 2.3 per cent who voted for Kennedy produces the finding that a full quarter of the Republican voters in the sample voted in the Democratic primary. (Only 2.0 per cent of the Democrats reported voting in the Republican primary, all of them for Nixon.) These voters, moreover, were an important component of McCarthy's

³⁰ Cf. Sorauf, *Party Politics in America*, pp. 205–206. For a more detailed account of crossing over in Wisconsin primaries, see Leon D. Epstein, *Politics in Wisconsin* (Madison: University of Wisconsin Press, 1958), pp. 94–95; and Howard R. Penniman, *The American Political Process* (Princeton, N.J.: D. Van Nostrand Company, Inc., 1962), pp. 85–86.

³¹ Sorauf, *Party Politics in America*, p. 206.

³² V. O. Key, Jr., *Politics, Parties & Pressure Groups*, 5th ed. (New York: Thomas Y. Crowell Company, 1964), p. 391. See also Kay Lawson, *Political Parties and Democracy in the United States* (New York: Charles Scribner's Sons, 1968), p. 134.

Table 10. Party Composition of the Three Leading Candidates' Support in the 1968 Wisconsin Presidential Primary

Party Identification of Voters	Voted for		
	Nixon	Johnson	McCarthy
Republican	90%	5%	26%
Democratic	3	90	65
Independent	7	5	9
	100%	100%	100%
Number of cases	93	62	104

Table 11. Reasons Given for Crossing Party Lines, Wisconsin Presidential Primary, 1968

"Why did you cast your primary vote for a candidate for President [not of the party with which you identify]? Does this mean you are moving closer to [the other party], or what?"	Number Responding
No, but felt he was best qualified; his views best coincided with mine; didn't like the other candidates	20
I just consider myself closer to [the other party]	6
No, crossed over to show disapproval of the Vietnam war	2
Yes, not further specified	3
Not ascertained	7
Total	38

overall margin over Johnson and Nixon, as is shown by the data in Table 10. Table 10 makes it clear that, while McCarthy would have defeated Johnson even if only Democrats had voted in the Democratic primary, his Republican-crossover support provided much of his impressive margin: among Democrats alone, McCarthy led Johnson with 48.6 per cent of the sample to 40.0 per cent; but with the Republicans added, he led with 54.9 per cent to Johnson's 34.1 per cent.

The total of 38 crossovers in our Wisconsin sample³³ does not constitute an N large enough for any very elaborate analysis. Each respondent who reported crossing over, however, was asked why he or she had done so, and their answers are tabulated in Table 11.

So far as it goes, the distribution of answers in Table 11 strongly suggests that here, at least, we are dealing with a case in which most of the crossovers temporarily deserted their party to register their special approval of a candidate, or a policy associated with a candidate, available only in the other party. McCarthy's Republican supporters clearly did not vote for him because they thought he would be the easiest Democrat for Nixon to beat in November; they did so because they liked him and his opposition to the Vietnam war. This case does not, of course, prove that raiding the opposition's primary to nominate a weak candidate never takes place; but it does nothing to contradict the view that only a handful of ordinary American voters have the high politi-

³³ They included twenty-seven Republicans for McCarthy, three for Johnson, three for Kennedy, and one for Wallace; and three Democrats for Nixon and one for Rockefeller.

cal concern and technical sophistication required to use their primary votes in such a Machiavellian manner.³⁴

VII. Conclusion

What does the foregoing analysis tell us about the widespread assumption, noted at the outset, that the presidential primaries are the most representative element of our current nominating system and furnish the standard by which the representativeness of its other elements should be judged? At least three of the findings seem most relevant.

First, it is clear that presidential primary electorates, like those in primaries for lesser offices, are demographically quite *unrepresentative* of their nonvoting fellow partisans. They are unrepresentative not only in the same respects in which voters differ from nonvoters in general elections, but also in much the same way that national convention delegates—at least prior to 1972—have been unrepresentative both of their parties' rank-and-file identifiers and of the general population. Consequently, if our prime object is to maximize demographic representation in the presidential nominating system, strict enforcement of rules for selecting convention delegates like those developed by the McGovern-Fraser Commission is clearly a much better bet than expanding the number of state primaries or adopting a nationwide primary.

Second, the existing presidential primaries' claim to a high degree of preferential representativeness is better, though hardly without flaws. Of eight national issue questions asked in New Hampshire, six evoked substantial differences between Republican participants and nonparticipants. In four of the six instances, the differences consisted of the greater intensity of the participants' preferences; in the other two instances, majorities of the participants favored policies substantively at odds with those favored by majorities of the nonparticipants. New Hampshire Democrats had major differences on only three of the issue questions, and all three were differences of intensity, not substance. Eleven questions on state issues were

asked in Wisconsin; five produced significant differences between Republican voters and nonvoters, one of which was a disagreement on substance and the others differences in intensity. Hence presidential primary electorates are also unrepresentative in issue orientation, at least to the degree that differences in the intensity with which opinions are held are politically as significant as differences in the direction of those opinions.³⁵

Finally, given the fact that in each state some national contenders were on the ballot and others were not, there is no reason to suppose that if all of each party's identifiers had voted in the primaries the outcomes would have been significantly different. But what the outcomes would have been in our samples as well as in the actual elections if *all* the contenders had been on the ballot and if no write-ins were permitted or necessary, we cannot say. Certainly the sharp increase from 1968 to 1972 in the number of states holding presidential preference primaries has not made the rules governing these matters more uniform or rendered interstate comparisons or primary outcomes more meaningful.

This paper, of course, shows only that presidential primaries fall well short of any ideal of perfect representativeness. It touches only indirectly on the larger question of whether or not the primaries are *more* representative than the other institutions of the nominating system. We can speak to that question best by setting alongside the data presented here comparisons of the demographic composition, issue stands, and candidate preferences of local, state, and national convention delegates with those of the nonparticipating party identifiers at each level. For such comparisons we presently have usable data for national convention delegates and party identifiers since 1952, but little or none for state and local convention delegates and their constituents.

The parties' nominating processes in 1972 provide unique opportunities for collecting such data. The McGovern-Fraser Commission's efforts to make all levels of the Democrats' process more representative should yield, at a

³⁴ It is worth recalling, in this regard, that our study of the 1966 Wisconsin gubernatorial primary turned up only ten crossovers. Of these, four were Republicans who left their own uncontested primary to vote in the hotly-contested Democratic primary, but six were Democrats who made the opposite move! See Ranney, "The Representativeness of Primary Electorates," pp. 227–228. Both the 1966 and 1968 Wisconsin findings support Sorauf's view that "as for organized 'raiding,' there is precious little evidence to suggest it is more than a worrisome myth": *Party Politics in America*, p. 206.

³⁵ Leading discussions of the problem posed for theories of democracy and representation by differing intensities with which opinions are held by different members of the community include: Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), Ch. 4; Henry B. Mayo, *An Introduction to Democratic Theory* (New York: Oxford University Press, 1960), pp. 178–179, 206–212; and Willmoore Kendall and George W. Carey, "The 'Intensity' Problem and Democratic Theory," *American Political Science Review*, 62 (March, 1968), 5–24.

minimum, far more systematic evidence about the composition and preferences of national, state, and local conventions than ever before available. And the Republicans' decision to make only minimal changes in their procedures should at least provide something approximating control groups for assessing the impact of the Democrats' reform efforts upon the representativeness of their conventions.

If political scientists take advantage of these

opportunities, we should be much better placed than now not only to answer the scientific question raised at the beginning of this paper but also to furnish much of the information decision makers will need to determine whether a national primary, or more state primaries, or reformed party conventions, or some mixture of the three is the best way to give Americans the truly representative presidential nominating process we say we want.

Decision Making in Soviet Cities

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The study of Soviet decision making has traditionally been a difficult task. Soviet authorities do not publicly ask the question, "Who makes the decisions here?" No published Soviet studies of decision making exist; nothing written attempts to describe and analyze a "process" of decision making; no Soviet political science texts provide empirical data covering the entire range of governmental activity on a particular territorial-administrative level. Soviet authorities publish few details about how the Party makes policy and then ensures policy implementation through its control over the governmental-administrative structure known as the system of Soviets. Nor do they provide any real information on how decisions are made by the various governmental-administrative organs and levels which comprise the system of Soviets.

As a result, detailed Western studies of the Soviet decision-making process are scarce. Good luck, in the form of captured Soviet archives, enabled Merle Fainsod to produce *Smolensk Under Soviet Rule*—a classic study of decision making in the province of Smolensk during the 1930s.¹ More recently, Philip Stewart and Jerry Hough produced important studies of Soviet provincial and local politics by relying upon painstaking scholarship and fortuitous research experiences in the USSR.² My own study of Soviet municipal politics is based upon a similar combination of good fortune and sustained research over a number of years.³ I chose to examine the urban level of Soviet politics because Soviet authorities, apparently concerned over the effects of rapid Soviet urbanization, have begun to publish a considerable amount of new information on Soviet urban life and politics, and have made it easier for Western noncommunist scholars to obtain useful interviews with Soviet

municipal officials.⁴ These research opportunities have made it possible to examine selected aspects of decision making on the urban level, to draw some conclusions about the future development of municipal politics, and to make some broader generalizations about the national Soviet political system.

My choice of four aspects of decision making—budget formulation, the planning process, housing construction and allocation, the staffing of top urban posts—is based upon the greater availability of Soviet data for these types of decisions, as compared to such issue areas as local industry or education. In addition, budget, planning, housing and staffing are probably the four most important municipal decisions which are made in the USSR. Without a plan nothing can be produced or distributed by anyone in any Soviet organization. Budget and plan are closely related—one cannot be conceived nor exist without the other. New and better housing is now a high priority item, and the provision of housing is becoming a major municipal function. A carefully structured system of staffing (*nomenklatura*) facilitates Party control over the entire municipal decision-making process, by ensuring that top municipal posts are staffed by the Party, with men and women who are Party members.

From my examination of the decision-making process in each of these four areas, I conclude that urban autonomy has increased slightly in the 1960s, although Soviet municipalities still remain very much restricted by superior Party and governmental authorities operating under the centralizing principles of Party dominance and dual subordination. The Party remains the directing core of all Soviet organizations, and it determines the regime's priorities. The nature and quality of this Party domi-

¹ Merle Fainsod, *Smolensk Under Soviet Rule* (Cambridge: Harvard University Press, 1958).

² Philip D. Stewart, *Political Power in the Soviet Union: A Study of Decision Making in Stalingrad* (Indianapolis: Bobbs-Merrill, 1968); Jerry F. Hough, *The Soviet Prefects* (Cambridge: Harvard University Press, 1969).

³ B. Michael Frolic, "Soviet Urban Politics," unpublished doctoral dissertation, Cornell University, 1970. See also B. Michael Frolic, "The Soviet Study of Soviet Cities," *Journal of Politics*, 32 (August, 1970), 675-695, and "Municipal Administrations, Departments, Commissions and Organizations," *Soviet Studies*, 22 (January, 1971), 376-393.

⁴ For example, in 1968, B. N. Gabrichidze, a Soviet political scientist, produced the first full-length study of Soviet municipal government. A year later, a major work on urban sociological research was published. See B. N. Gabrichidze, *Gorodskiy Sovet deputatov trudyashchikhsya* [City Soviets] (Moscow, Yuridicheskaya literatura, 1968); and Institut konkretnykh sotsialnykh issledovaniy AN SSSR, *Sotsiologicheskiye issledovaniya goroda* [Urban Sociological Research] (Moscow, 1969). During two trips to the USSR, in 1965 and 1969, I interviewed 70 officials in ten Soviet cities (Baku, Bratsk, Donetsk, Irkutsk, Leningrad, Minsk, Moscow, Tallinn, Tashkent, and Thorez).

nance may be changing, as the educational qualifications and professional expertise of urban Party officials rises, but these changes have not altered the basic fact of Party dominance over decision making. Dual subordination—whereby higher level bodies establish policy guidelines for their subordinate organs at the lower levels and can suspend decisions made by these subordinate organs—remains fully in operation. Dual subordination encourages national uniformity in decision making and it discourages the growth of local parochialism (*mestnichestvo*). Unfortunately, the result has been a highly centralized system which practically has stifled local initiative. Now, as municipal administrators and other local specialists acquire more expertise and influence in making administrative and technical decisions within the municipality, one might expect local initiative to develop in the future.

Budget Formulation

The municipal finance department (*gorfo*) is responsible for the budgeting process within Soviet cities. This process is complex and takes almost a full year to complete. *Gorfo* is formally accountable to two state organs: the executive committee of the city Soviet (the cabinet of city government), and the Republic Ministry of Finance.⁵ In a city of 500,000 inhabitants, *gorfo* normally will employ about 125 persons, most of them having specialized training in finance or in economics. *Gorfo's* top staff are usually Party members, although few of its leading cadres have held full-time Party posts. They generally devote their entire careers to the state finance apparatus.⁶

At the start of each year, municipal functional and branch administrations, departments, organizations and commissions⁷ submit their budget proposals to *gorfo*. These proposals must fall within frames of reference already established in prior, informal discussions with *gorfo* and with the municipal executive committee, using republic and national Party and state control figures as basic guidelines. For example, a municipal trade and public catering administration which may want a large increase

in operating revenues and in capital expenditures to build and staff more restaurants, must first consider the Party's announced national commitment to the service sector. (Will there be a significant increase? If yes, are restaurants a high priority item?) Then the priorities of the republic have to be met. (Perhaps other cities have more urgent needs for new restaurants.) Finally, there are other municipal needs to consider. Given the choice of either a new department store or some new restaurants, the executive committee may opt for the department store. After taking such factors into account, the trade and public catering administration will submit its formal budget proposal to *gorfo*.

Gorfo organizes the various budget proposals into one consolidated municipal draft budget, and this draft budget is submitted in May to the executive committee for discussion. At this stage, *gorfo* has already consulted with its vertical superior, the republic Ministry of Finance, in order to ascertain just what the republic is prepared to allocate to the municipality (more than 90 per cent of municipal revenues come from republic sources), and to determine how much room there might be for some bargaining between the municipality and the republic later on. *Gorfo* and the municipal planning commission (*gorplan*) confer together, to ensure that the draft budget and draft plan are coordinated. The executive committee normally works on the draft budget for two or three months. The chairman of the executive committee (the mayor) takes charge of the budget in the executive committee. The committee meets with the heads of *gorfo*, *gorplan*, other city department heads, the chairmen of city district executive committees, and Party officials, to discuss the draft and to make changes.⁸ The executive committee then approves the draft and sends it upwards to the republic Ministry of Finance, either directly or through *gorfo*. "At this point in municipal budget formulation," observed a Tallinn *gorfo* official, "significant changes in the original draft budget may have been made by the executive committee, with the implicit approval of *gorfo* and the Ministry."

The draft budget arrives in the Ministry of Finance by August. The Ministry examines the draft and consults with the republic Council of Ministers and with other republic committees and ministries. Budget cuts are invariably made. The Ministry meets with top *gorfo* offi-

⁵ I am using as examples the cities of Tallinn (Estonia) and Donetsk (the Ukraine), two cities whose immediate governmental superior is the republic level. Smaller, less important cities normally come under the jurisdiction of provinces or districts, the lower links in the Soviet state hierarchy.

⁶ See Frolic, "Municipal Administrations, Departments, Commissions and Organizations," for a fuller description of *gorfo's* structure and its other municipal duties.

⁷ Referred to, hereafter, as "municipal administrations."

⁸ City districts are governmental-administrative subdivisions in large Soviet cities. In structure they are smaller replicas of their city parent. Moscow has 25 city districts, each with a population of 200,000-250,000 inhabitants.

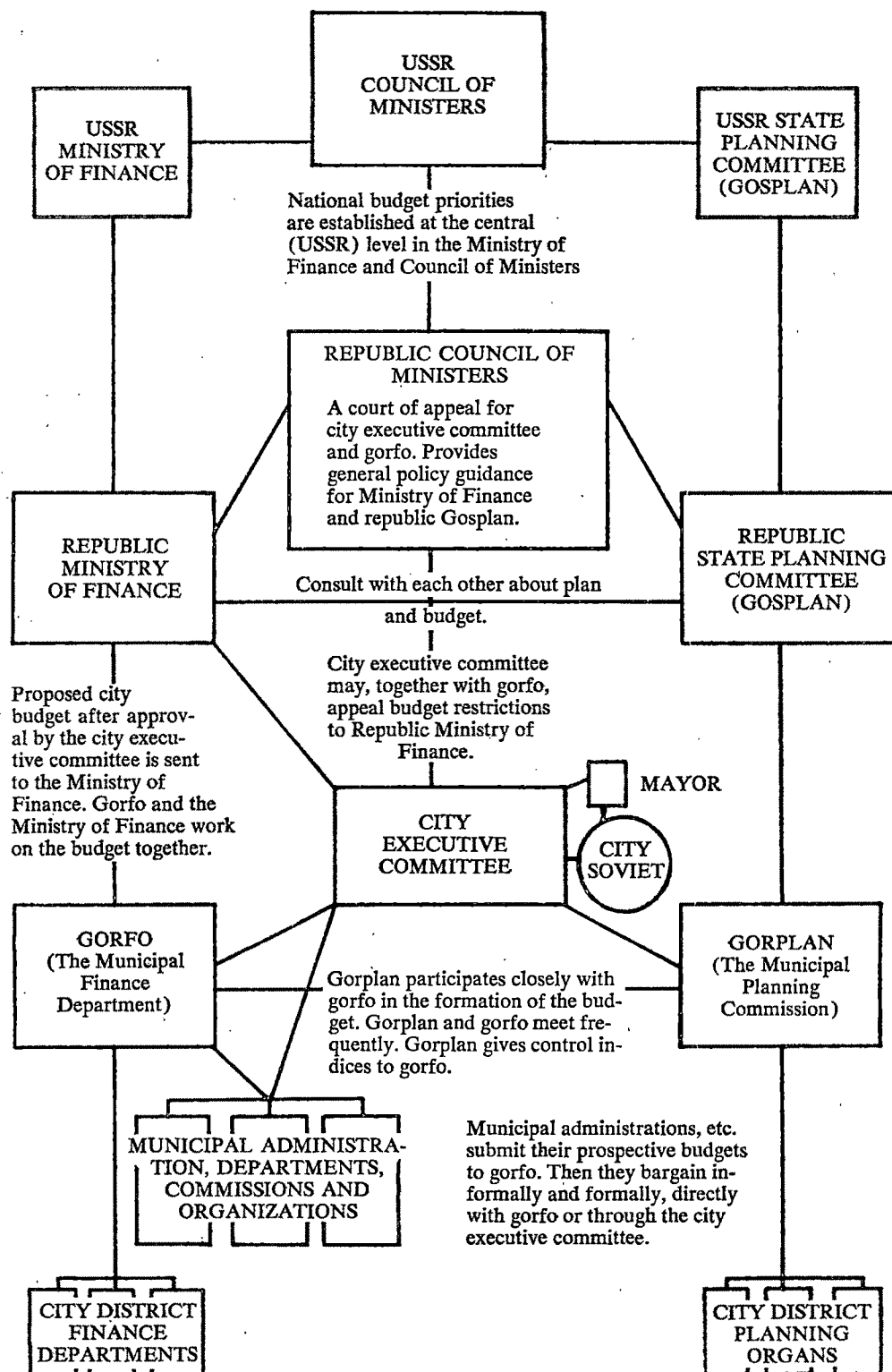


Fig. 1. Vertical-Horizontal State Relationships During Municipal Budget Formulation

cials to discuss these revisions. A final draft budget is produced and then submitted to the municipal executive committee in early fall. Municipal officials usually accept the final draft budget without reservations, although it is legally possible to appeal to the republic for additional funds. In the late fall, after all adjustments have been made, gorfo draws up its final version of the budget, in collaboration with gorplan and the municipal executive committee. The final budget is printed up and circulated to all municipal administrations, to city district executive committees and to other leading municipal officials. Changes made at this stage are minor. The final budget is formally submitted by the executive committee to the city Soviet, for ratification by the deputies at their December session. (Copies of the budget are distributed to the city Soviet's standing committee on budget and finance several weeks in advance of the session. The committee has the power to recommend changes but rarely does so.) At the session, the budget is approved unanimously, usually without much discussion. The budget now becomes a legal document, binding upon all municipal organs. It is then officially published, and the new budgetary cycle for the following year begins.

The above description of budget formulation shows that the republic level exercises strong control over municipal finance. The control figures that gorfo uses come from the republic: the municipality, having almost no revenues of its own, is totally dependent upon revenues allocated to it from above; the republic can make arbitrary budget cuts, from which the municipality has little recourse; municipalities secure few real gains in negotiations with republic financial authorities. It is true that gorfo speaks for the municipality during the process of budget formulation; but gorfo is also the municipal arm of the republic Ministry of Finance, so its municipal loyalties are questionable, despite its formal subordination to the municipal executive committee.

On the municipal level, gorfo and the executive committee share responsibility for budget formulation. Gorfo provides information about control indices and the technical details of budgeting. Within this framework, the municipal executive committee coordinates the claims and demands of the various municipal administrations. How closely do gorfo and the executive committee cooperate? According to some municipal officials, there is minimal cooperation between the two organs, even though the head of gorfo usually sits on the executive committee. One gorfo official noted,

"No one likes us very much. The municipal ad-

ministrations know that we are going to limit their expenses, and other members of the executive committee claim that we are not committed enough to the municipality. Then, too, few other municipal officials have the technical expertise which is needed to cope with detailed budget and financial matters."

How well does the executive committee get along with the municipal administrations during the budgetary process? Since the heads of major municipal administrations also sit on the executive committee, and since there rarely are large uncommitted funds to argue over, relations between the executive committee and the administrations are fairly routine. The mayor, the heads of the key administrations and one or two other leading municipal officials usually resolve any conflicts while the budget is in executive committee.

The ordinary deputy to the city Soviet hardly participates in the budgetary process. He is called upon only at the end, to ratify a document which is considered an established fact by municipal officials. The average deputy, like aldermen in most North American cities, cannot debate budgetary provisions, many of which are too complex for him to understand. He has precious little time to read the text of the budget before its adoption at the December session. A few deputies are members of the city Soviet's standing committee on budget and finance, but this committee has only recommendatory powers. "Just as in your country," noted one Leningrad official, "the average city deputy knows very little about the budget. He has neither the time nor the expertise to understand the document in its entirety. But he will be able to understand the budget's basic provisions."

What role does the Party perform during budgetary formulation? The Party (whose members comprise about five per cent of the Soviet population) establishes basic priorities and guidelines for officials in the Ministry of Finance, and then, apparently, leaves the budgetary process in state hands except for emergencies (a sudden revision of economic priorities or a dispute among state agencies or levels). Several officials pointed out that the Ministry of Finance is relatively free from Party control and politics, noting that the Ministry developed a tradition of expertise and independence under the leadership of A. G. Zverev.⁹ Top financial officials are, however, Party members; each of the twelve financial officials whom I interviewed were Party mem-

⁹ Arseny G. Zverev was USSR Minister of Finance from 1938 to his retirement in 1960, except for a brief ten-month period in 1948, when Alexei Kosygin held the post. Zverev's 22-year tenure as head of the USSR Ministry of Finance was an extraordinary achievement.

bers, although none of them had worked in the Party apparatus for the last ten years. The Party also exerts its controls on the municipal level in a number of ways. First, the top officials in the city Party organization (the first secretary and other Party secretaries) are members of the municipal executive committee; they represent the Party during budget discussions. Second, every key member of the municipal executive committee is a Party member. The heads and top staff of most municipal administrations are also Party members.¹⁰ Third, the city Party organization has its own financial and budget specialist who can give advice and information to municipal officials. Fourth, the city Party helps to train municipal deputies, to explain the basic features of the budget to them, and to show them how to communicate this information to their constituents. Finally, the Party functions as a supercoordinator, a municipal arbiter, and provides a constant check upon the work of state organs. These Party controls may not be as obvious during the budgetary process as they are during the formulation and implementation of other policies within the municipality, but they are available to the Party should it choose to exercise them.

The Planning Process

The budget is actually one part of the annual city plan, which effectively controls the activities of all individuals, enterprises and organizations under municipal jurisdiction. Everything, everybody, has his plan—an officially determined set of productivity expectations. The city plan is a mosaic of many smaller plans, stuck together in the hope of providing maximum productivity and efficiency. The process of making and approving a city's annual plan follows the same basic pattern as does budget formulation. Gorplan, the municipal arm of the republic State Planning Committee, is the municipal equivalent of gorfo. Republic state and Party organs exercise strong control over the work of gorplan. For example, in the city of Baku, "the annual city plan is based upon control figures worked out by various Azerbaidzhan republic ministries, together with Azerbaidzhan Gosplan (the Azerbaidzhan republic State Planning Committee), and affirmed by the Central Committee of the Azerbaidzhan Communist Party and the Council of Ministers of

the Azerbaidzhan republic." The Baku city plan is worked out, "based on these republic directives and the needs of the Baku city Soviet."¹¹ Thus Baku, a major Soviet city, has little control over its own annual plan, and the Baku city Soviet must work within a framework which is established by republic Party and state organs and which is then communicated to the municipality through gorplan.

Once it has received its instructions from above, gorplan consults with the executive committee and then sends the control figures for the new plan to all municipal administrations. The latter submit draft plans to gorplan in late spring. Gorplan then produces a consolidated municipal draft plan, confers with the executive committee, making some additional changes, and then sends the municipal draft to Gosplan in June. Republic authorities make extensive changes, usually revising productivity targets upward, reducing operating expenses, and restructuring municipal priorities to conform to last minute Party instructions. The draft plan now returns to the municipality, and the executive committee and the municipal administrations examine the revised draft, hoping to restore some of the fat which republic authorities have trimmed off. The municipality lobbies with its republic superiors for additional concessions, such as a greater allotment for housing, and a reduction in municipal productivity targets. In November, copies of the final plan are circulated to top municipal officials prior to the December session of the city Soviet, at which the plan and budget are formally ratified, with little discussion and no reservations.

While planning and budget formulation are closely intertwined and both processes follow similar patterns, the Party is comparatively more active during the planning process. There is more bargaining and conflict among municipal and republic organs, and municipal organs now have a moderate amount of autonomy in certain areas of the planning process. One Leningrad official observed, "The budget is a technical document, and the Party, for the most part, leaves the budget to the financial experts. The plan is another matter. It represents the Party's allocation of priorities during the coming year, and is more of a political document, requiring greater Party control." The head of gorplan, for example, is often a former full-time

¹⁰ When Mikhail Vasilevich Posokhin became head of Moscow's Administration of Architecture and Town Planning in 1961, he simultaneously joined the Party. All Moscow administration heads are Party members. In small cities, non-Party members head the less important municipal departments.

¹¹ "Zadachi i rabota bakinskoy gorodskoy planovoy komissii" [Tasks and Work of the Baku City Planning Commission], Baku, 1965, p. 1. This seventeen-page document was specially prepared for me by the Baku planning commission.

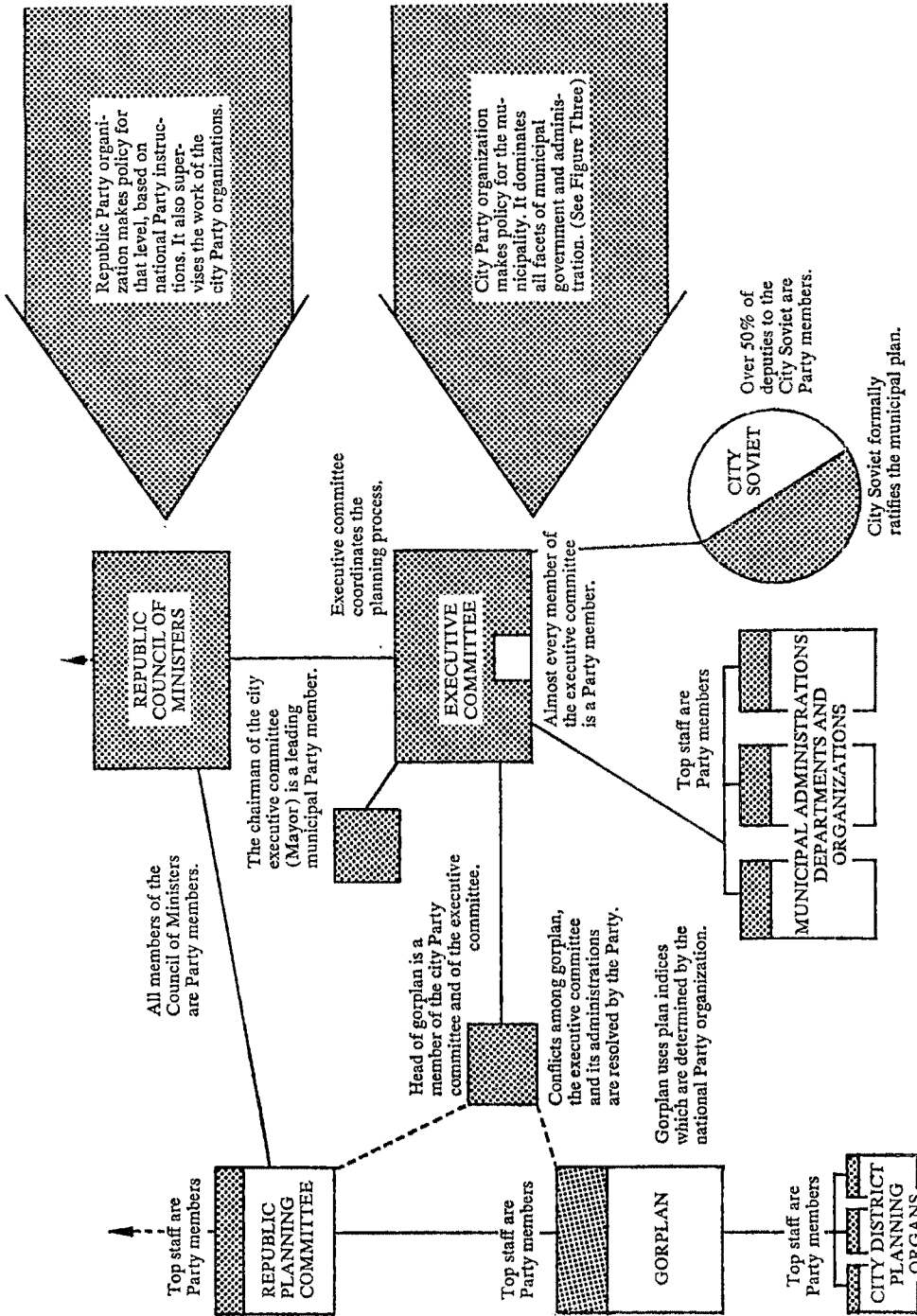


Fig. 2. Party Dominance over the Governmental-Administrative Organs Engaged in the Municipal Planning Process
(Continued in Russian Party membership or Party Candidate membership)

Party official. Planning specialists sitting on the municipal executive committee usually have had extensive Party experience.¹² Party organs directly intervene during the planning process to provide information, stimulation, and coordination. The Party has its own planning specialists—experts in trade, construction, light and heavy industry, transportation, and the municipal economy. These Party specialists frequently step in during the municipal planning process, giving the Party viewpoint on a particular matter, and offering guidelines and other advice to municipal officials.¹³ The Party also plays an important role in the solution of conflicts which arise during the planning process. "The municipal executive committee is not always in a position to resolve these conflicts, and sometimes the Party bureau steps in to settle disputes," said a Tashkent city official. "For example, both the city and republic Party organizations had to intervene to resolve a recent jurisdictional dispute between two Tashkent administrations. The executive committee had been unable to settle the conflict, and the dispute had been hampering the planning process."

What is meant by "municipal autonomy" in the planning process? It is hard to conceive of "autonomy" when basic planning decisions are made by nonelected officials from a higher territorial administrative level. In the Soviet context, however, it is apparent that municipal organs have acquired a moderate amount of autonomy (perhaps "initiative" is more appropriate) in certain areas of the planning process, probably due to a number of historical and developmental factors. The death of Stalin, the decentralizing administrative reforms in 1957, rapid urbanization, consumer-oriented policies, an increase in the technical and educational qualifications of lower level officials—all these factors in varying degrees, have contributed to some democratization of the municipal decision-making process. How is this shown during the highly centralized municipal planning process? First, "the city Party organization has become more independent in its relationships with higher Party organizations, and is less inclined simply to carry out orders from above."¹⁴ Second, municipal officials have acquired some

competence in planning, and higher authorities are willing to entrust them with more responsibilities. Third, the scale of municipal government has greatly increased in the past decade, and higher authorities are forced to delegate an increasing number of tasks to municipal administrators. The head of one municipal administration noted that he had served for more than twenty years in the administration, "but only in the last few years, when the volume of our work increased radically, did our administration begin to perform meaningful tasks within the planning process. Now our preliminary figures are not treated so rudely by our superiors, they consult us more often, and they have stopped imposing arbitrary production quotas upon us." To a Western reader, such examples do not add up to democratization, municipal autonomy, or greater initiative, although in the Soviet setting these changes are fairly significant.

Housing Construction and Allocation Staffing

The average Soviet citizen is allocated 97 square feet of housing space. In large cities this figure may drop to 60–75 square feet of housing. The prospect of new and better housing, therefore, is a major preoccupation of most urban dwellers, and probably constitutes the most important municipal decision affecting their daily lives. Supply and maintenance of housing is becoming a major (though by no means exclusive) concern of Soviet municipalities. Cities have gained considerable responsibilities in this area in the past decade. Larger cities have begun to take over the allocation and administration of housing from factories and ministries which had in the past monopolized these functions and had not been subject to municipal jurisdiction. Municipal housing construction organizations have been established in the three largest Soviet cities, Moscow, Leningrad, and Kiev, and housing design has become more decentralized and responsive to the needs of particular urban environments. Even so, municipalities today have only partial control over urban housing. Higher level state and economic organizations are reluctant to relinquish control over this scarce commodity which is used as an inducement for attracting and keeping workers. Thus even in 1965, the city of Moscow still administered only two-thirds of the housing located on its territory; the rest was in the hands of some 300 organizations and enterprises not under the Moscow government's jurisdiction. Four years later, in 1969, a Moscow housing official said that the city was administering 75 per cent of the housing located on its territory,

¹² This observation is based upon an analysis of the career patterns of members of Soviet municipal executive committees, 1957–1971, and from personal interviews.

¹³ One municipal official observed that "Certain city Party officials are apparently born with a telephone permanently attached to their body. They are always calling, always checking, always complaining."

¹⁴ From an interview with the first secretary of a city Party organization.

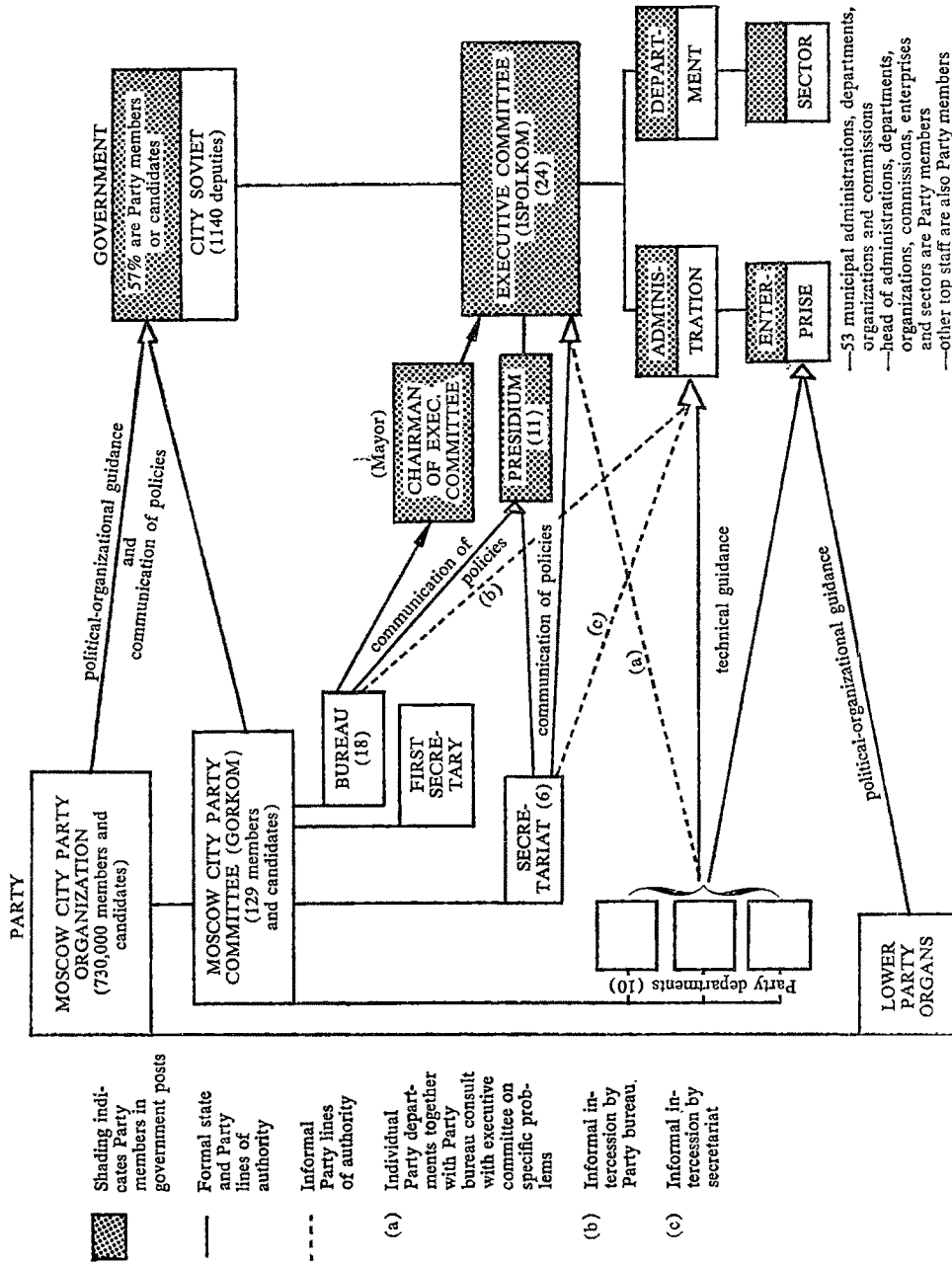


Fig. 3. Party Domination of Municipal Government in Moscow, 1966

"in keeping with a continuing national trend toward giving Soviet municipalities full control over housing."

Housing policy is formulated at the national level by the Party and communicated to republic and municipal Party and state officials in a fashion resembling budget formulation and planning.¹⁵ Annual housing construction figures, allocation priorities and funds for housing maintenance and repair are included in the city's plan. A large number of municipal administrations are involved in various aspects of housing construction, design, allocation and administration, and many of the key members of the city Party bureau and the executive committee have had extensive experience in the field of housing.¹⁶ Figure 4 shows how the various municipal administrations and the executive committee divide responsibilities for housing in Moscow. The Main Administration of Construction (Glavmosstroy) builds 85 per cent of Moscow's housing, has 250,000 employees and an annual payroll of \$300 million. Glavmosstroy is responsible to the municipal executive committee and to Gosstroy, the State Construction Committee. The head of Glavmosstroy always is a member of the executive committee.¹⁷ Glavmosstroy meets with the executive committee early in the year to discuss details of Glavmosstroy's forthcoming yearly plan and to coordinate housing construction with other phases of the housing "process." For example, the Main Administration of Town Planning and Architecture (Glavapu) designs the apartments and buildings which Glavmosstroy builds. These two administrations meet together to coordinate housing construction and design. Because the Main Administration of Construction Materials provides building

supplies to Glavmosstroy, the executive committee tries to ensure that this administration's production schedule fits in with Glavmosstroy's construction schedule. Glavmosstroy cannot get paid by the state until another administration, the Main Administration of Capital Construction, has inspected the completed building and given its approval. At this point disputes often arise, and the executive committee may have to intervene. Once the building is legally completed, the Administration for the Allocation of Housing takes charge of the distribution of this new housing and the Main Administration of Housing assumes the responsibility for the operation of and repairs to Moscow's housing.¹⁸

Municipalities are faced with endless problems and crises during this housing cycle. Construction targets are almost never filled because of delays in getting materials, low worker productivity, construction mistakes and overly ambitious construction targets. There is always a chronic shortage of housing. The municipal waiting lists for new housing space are so long that citizens may have to wait several years to get new or better housing. It is almost as difficult to get housing repaired. The Main Administration of Housing just cannot meet an ever-increasing demand for urgent repairs to new and old housing, despite the creation of hundreds of decentralized housing repair units. Desperate tenants resort to black market repairs, paying a "bonus" out of their own pockets to "repair entrepreneurs," who make illegal repairs on state time, using state materials. The Soviet press continually condemns these practices, berates municipal officials for failing to improve housing and exhorts construction workers to improve the speed and quality of their work. Conscious of the impact which housing has on the average urban dweller, the Party encourages public criticism of housing inadequacies, thereby hoping to improve the performance of individuals and officials involved

¹⁵ Current Soviet housing policy is based upon the July, 1957, resolution of the Central Committee of the Communist Party, "On the Development of Housing Construction in the USSR." This resolution established a number of important housing principles, such as "massive industrialization of housing construction" and "greater control over housing by municipal executive committees."

¹⁶ The last three mayors of Moscow had long careers in housing construction and administration. N. I. Bobrovnikov, who was mayor from 1956-1961, spent most of his career as a Moscow housing expert; N. A. Dygai (1961-63) was USSR Minister of Construction prior to his appointment as mayor; V. F. Promyslov, the current Moscow mayor, was Minister of Construction for the Russian Republic.

¹⁷ V. F. Promyslov, the first head of Glavmosstroy, later became mayor of Moscow; A. A. Etmekdzhiyan, a deputy head of Glavmosstroy, became a top USSR State Construction Committee (Gosstroy) official; N. Pashchenko, Promyslov's successor as head of Glavmosstroy, now holds a high Russian Republic state post.

¹⁸ State housing is allocated on a space per capita basis, with 97 square feet representing the "norm." Certain individuals and organizations have priority over others in the allocation of space. Writers and other professionals frequently can obtain an extra room; military organizations get preference over civilian organizations. These and other priorities are usually regulated by statute. See "Ob utverzhdenii polozheniya, 'O poryadke raspredeleniya zhiloy ploshchadi v Moskve'" [On Affirming the Statute, "On How Housing Space Will Be Distributed in Moscow"], Decision of the Moscow city executive committee, May 4, 1965, *Byulleten' ispolnitel'nogo komiteta moskovskogo gorodskogo Soveta deputatov trudyashchikhsya* (Bulletin of the Executive Committee of the Moscow City Soviet), 10, (1965), 22-27. This municipal statute is a duplicate of the republic housing statute.

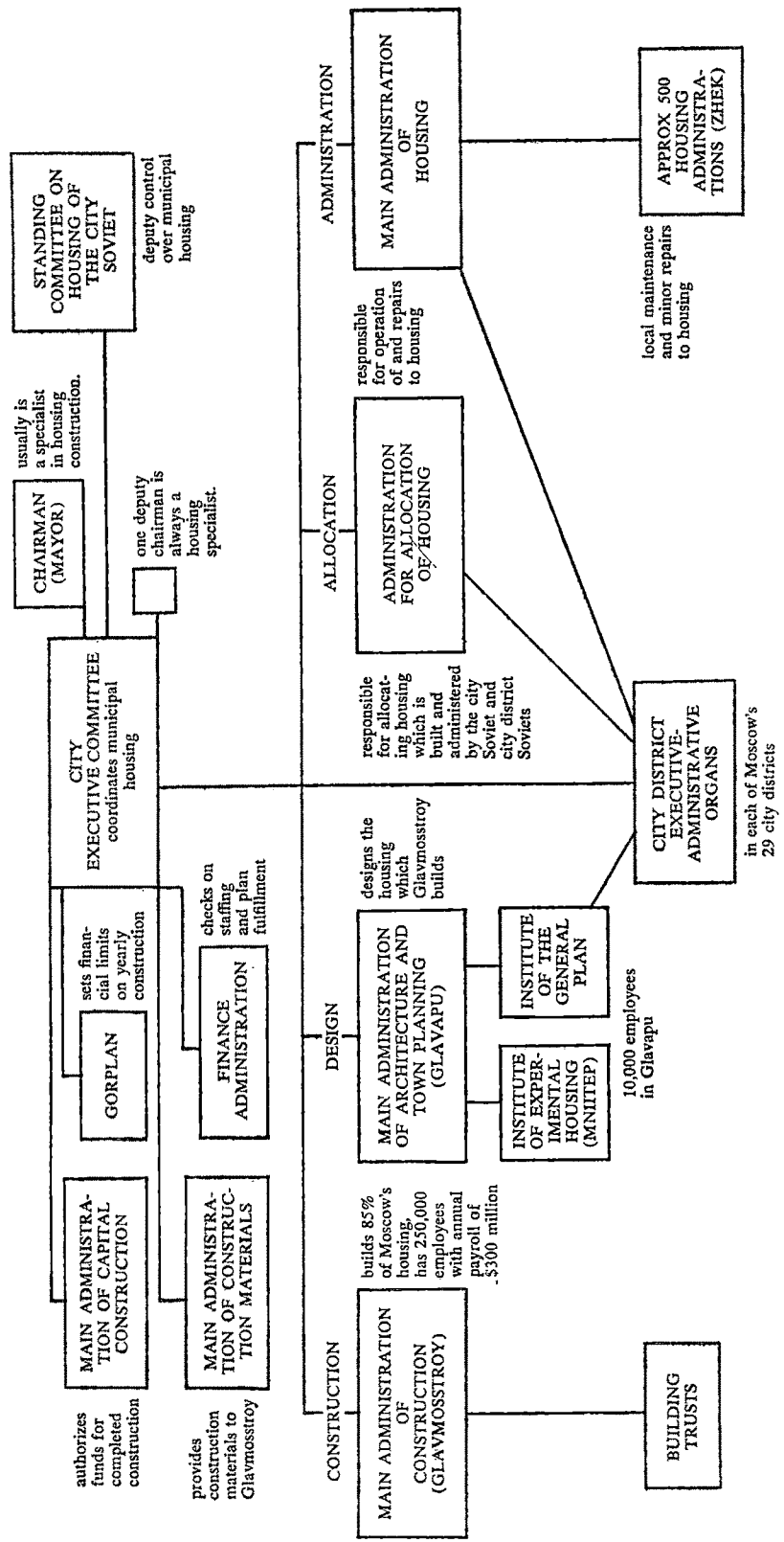


Fig. 4. Housing Construction, Design, Allocation and Administration in Moscow, 1969

in the housing cycle and to provide an outlet for citizen discontent with poor housing. During municipal election campaigns, prospective candidates are most frequently questioned about housing matters. I attended several municipal "campaign speeches" in Moscow and in Leningrad, and housing always was the main topic of discussion.

The city Party organization devotes a great deal of its energies to municipal housing problems, particularly in cities which have their own housing construction organizations. One city Party secretary always is a housing specialist, and two or three Party departments (usually in construction, industry, and the municipal economy) concentrate on housing problems. According to one city Party secretary, "More than half the decisions made by the Party bureau (the most important city Party organ) have to do with housing. The Party bureau establishes municipal housing policy, in cooperation with the municipal executive committee." Since the key members of the executive committee (the Party secretaries, the mayor) sit on the Party bureau, it is clearly the Party bureau which makes any decisions affecting housing policy in the municipality.¹⁹ Besides its responsibility for policy formulation, the city Party organization provides political, organizational and technical guidance to municipal housing administrations. By means of campaigns in the media, and through its control over the trade unions and youth organizations, the Party constantly tries to improve the pace and quality of municipal housing construction. The city Party committee organizes all Party members within a given city organization into Party groups which are to meet regu-

larly to receive and to discuss Party instructions. Municipal housing officials may find themselves asking Party housing specialists for technical assistance, advice about policy implementation, and help in coordinating their work with other municipal agencies. At the lower levels, the Party explains housing policies to citizens and hears their complaints, either through specially trained Party activists in local housing administrations, at work, or in public organizations; or through municipal deputies, who are trained and supervised by the city Party organization. From top to bottom, therefore, from mayor to ordinary citizen, the Party is involved in the municipal housing cycle, making housing policy and then presiding over its implementation.

Staffing

In the USSR, the Communist Party traditionally is responsible for "the selection and appointment of leading personnel, their education in the spirit of communist ideas, honesty and truthfulness, and a high sense of responsibility to the Party and to the people for the work entrusted to them." The Party ensures "the appointment of the best leaders in the most responsible areas of the state apparatus."²⁰ On the municipal level, all top posts, Party or non-Party, are staffed by the Communist Party with Party members. If a municipal post is important, it is usually held by a Party member, who, of course, has been appointed by the Party.²¹ Within each municipality there are three levels of staffing jurisdiction (*nomenklatura*). Top non-Party posts are staffed by the city Party organization on the recommendation of higher Party and state authorities. Middle range non-Party posts normally are staffed solely by the city Party organization. Lower level non-Party posts are filled by municipal non-Party bodies.

Top municipal non-Party posts—such as the mayor, deputy chairmen of the executive committee, heads of municipal administrations, and the head of the city trade unions—are staffed by the city Party organization on the recommendation of higher Party and state bodies. What does this mean in actual practice? Does the city Party organization have any initiative here, or does it merely serve as a rubber stamp for staffing decisions made on a higher level? These questions are difficult to answer, because Soviet authorities do not publish many details

¹⁹ For example, two weeks after the Party Central Committee passed its July, 1957, resolution "On the Development of Housing Construction in the USSR," the Moscow city Party bureau met to implement this resolution in Moscow. The bureau "affirmed the July resolution" and promised "to emphasize these policies in Moscow." It "approved the present work of the city Soviet's Main Administration of Construction" and instructed its members "to show municipal organizations how to carry out the July resolution." See *Nekotorye voprosy organizatsionno-partiinoy raboty v sovremennykh usloviyakh* [Several Questions of Organizational-Party Work in the Present Time] (Moscow: Higher Party School and Academy of Social Sciences of the Central Committee of the CPSU, 1961); and A. A. Levsky, "Deyatelnost moskovskoy organizatsii KPSS po vypolneniyu reshenii XX syezda partii v oblasti uluchsheniya zhilishchnykh uslovii trudyashchikhsya" [The Work of the Moscow Party Organization in Fulfilling the Decisions of the 20th Party Congress Concerning the Improvement of Housing for Workers], dissertation for candidate degree in history, Moscow, AON pri TsK KPSS, kafedra of Party history, 1961.

²⁰ *The Statutes of the Communist Party of the Soviet Union*, 1961.

²¹ This section does not discuss the staffing of top city Party posts (Party secretaries, members of the Party bureau, Party department heads). These posts are normally staffed by higher Party organizations.

about staffing procedures, particularly when Party organizations are directly involved. It appears that city Party staffing initiative varies in intensity and scope, depending upon the type of post, the size of the city and the personalities of those involved. According to the first Party secretary of a small city of 150,000 inhabitants, "most top non-Party posts are generally staffed from above. The city Party organization may recommend names, but the actual staffing is done from above." Party officials in large cities said that the city Party organization has more initiative than that in staffing top non-Party posts, particularly in the appointment of deputy chairmen of the executive committee. In large cities, the city Party organization probably participates more closely in the appointment of heads of municipal administrations. In Moscow, "the head of the Main Administration is nominated by the republic State Construction Committee after consultation with the city Party organization,"²² while several Leningrad administration heads referred to greater city Party and state control over the appointment of heads of Leningrad administrations. Yet these are relatively minor changes, and, on balance, they do not indicate major gains in staffing autonomy. The city Party organization still continues to be dominated by higher authorities in the staffing of top municipal non-Party posts.

Middle range non-Party posts are staffed exclusively by the city Party organization. What kinds of posts are these, and do they present distinctive staffing patterns? The range of posts is wide, comprising factory directors, heads of research institutes, key media personnel, the chairmen and deputy chairmen of city district executive committees and leading cadres in the municipal executive committee and its administrations. Following are examples of middle range staffing in Leningrad and Riga:

In the past two years the Leningrad Party committee has placed a large number of Party workers in Soviet trade union work. . . . Two former city Party department heads are now chairmen of city district Soviets. The former first secretary of the Lenin city district, Yu. M. Solomonov, is now deputy chairman of the Leningrad city Soviet.

The Riga city Party committee transferred 42 people in its *nomenklatura* to other posts. Comrade Bobrov, secretary of the Kirov city district Party committee, was demoted and made director of a tobacco factory; . . . the head of the industrial-transport department of a city district Party committee was demoted to the post of deputy director for cadres in a factory; the first secretary of the Moscow city district Komsomol (Party

youth) organization was transferred to an economic post.²³

Of 660 posts staffed by the Riga Party organization in 1966, 253 (38.3 per cent) were "workers in Party and Soviet organs"; 107 (16.2 per cent) were "economic officials" (factory directors and top staff); 300 (45.4 per cent) were divided among "ideological organizations," "educational organizations," "administrative and trade-financial organs," and "workers in construction and the municipal economy."²⁴ The most distinctive staffing pattern on this level, according to municipal officials, is a noticeable increase in the quality of these appointments and a greater willingness on the part of city Party officials to consult with relevant non-Party organs before staffing posts which require specialized skills.

The Party staffs the top and middle range municipal posts, but non-Party agencies fill a large number of lower-level posts. A Western study estimates that 80-85 per cent of Soviet non-Party posts "are completely within the purview of the employing agency, subject to compliance with existing classification schedules, tables of organization and service regulations."²⁵ Barghoorn notes that the Ministry of Finance "determines the wage fund of ministries, state committees and enterprises" and thus is a key participant in the staffing process, since it must approve increases or decreases in the wage funds of state organs. The State Staffing Commission (*Gosudarstvennaya shtabnaya komissiya*) is instrumental in setting up salary schedules, job descriptions and tables of organization; the state Committee on Labor and Wages "works out uniform salary scales" and "legislation on salaries and wages."²⁶ Municipal execu-

²² See N. N. Popov, "Podbor, rasstanovka i vospitaniye kadrov" [The Selection, Placement and Training of Cadres], *Voprosy partiinoy stroitelstva* [Questions of Party Construction], (Leningrad: Lenizdat, 1962), p. 339; and Yu. Ruben, "Rabota s kadrami—produmannuyu sistemu" [Working with Cadres—Well Thought Out System], *Kommunist*, 1 (1966), 36-37.

²³ Yu. Ruben, "Rabota s kadrami—produmannuyu sistemu," p. 36. Riga's *nomenklatura* is comparatively smaller than some other municipal staffing lists (in 1958, for example, Moscow had a *nomenklatura* of 17,000). The Riga posts are probably those posts staffed by the central city Party organization and exclude a large number of lesser posts staffed by city district Party organizations.

²⁴ *Staffing Procedures and Problems in the Soviet Union* (Washington, D.C.: US Government Printing Office, 1963), p. 17.

²⁵ Frederick C. Barghoorn, *Politics in the USSR* (Boston: Little, Brown and Company, 1966), pp. 290-291. See also Merle Fainsod, *How Russia is Ruled* (Cambridge: Harvard University Press, revised edition, 1963), pp. 414-417.

²⁶ From an interview with a Glavmosstroy official.

tive committees have their own *nomenklatura*, and can appoint "cadres of the city Soviet."²⁷ The municipal organizational-instruction department, which trains and organizes municipal deputies, is called upon by the executive committee during the staffing of its *nomenklatura*, to assess the performance and capabilities of prospective appointees. According to several municipal officials, the executive committee's *nomenklatura* is growing in size and in importance, in keeping with the expanding governmental-administrative responsibilities of Soviet municipalities. Municipal branch administrations now have larger staffs to be appointed, and their vertical superiors are becoming less inclined to interfere in municipal staffing procedure.

Is there a trend toward reduction in Party dominance over the staffing of municipal non-Party posts? With Soviet municipalities now performing expanded, more complex governmental-administrative tasks, and with a continually growing urban population, the absolute number of non-Party posts within municipalities has risen and the Party just cannot handle the staffing of all these posts. The criteria for staffing are also shifting in favor of merit principles. Political reliability may be becoming less of an issue, with technical-rational expertise becoming the pertinent factor. "Getting the job done" now demands highly specialized knowledge of construction and engineering techniques, as well as traditional political-organizational skills. Can the Party become specialized enough to be able to use technical-rational criteria effectively in staffing, or will it have to delegate its authority to those non-Party bodies which have this competence? Several points speak out against a decline in the Party's dominance over the staffing of municipal non-Party posts. First, the Party is itself changing and is acquiring more specialists who are rationally-technically oriented. Thus it may not have to rely upon non-Party bodies to supply this sort of expertise. Second, the Party fully controls the instruments for punishing and firing officials whom it deems to be incompetent. Even if the Party cannot appoint everybody, it keeps a

close check on everybody's performance. Third, the Party will always appoint the top municipal officials—the political leaders—and thus is in a position always to influence other appointments. Finally, even if non-Party bodies appoint someone to a municipal post, why should this automatically produce a decline in the Party's role in the municipality? The prospective appointee almost certainly is a Party member, proposed by the leading officials (who are, of course, Party members) in a non-Party body. Actually the real issue confronting municipal governments may not even be Party dominance. For many municipal officials the real issue is too much higher level interference, from *both* state and Party bodies. Even the city Party organization is not permitted to staff its own top posts. But it is unlikely that these vertical controls will be lifted in the near future.

Soviet Municipal Government, Present and Future

The foregoing analysis of the Soviet municipal decision-making process in budget formulation, planning, housing and staffing has emphasized three basic features. First, the Communist Party stands squarely in the center of this process, making all the basic policies, appointing key municipal officials to implement these policies, and serving as the "boss" of the municipality. Second, higher level authorities decisively and consistently interfere in the municipal decision-making process. The budget and plan are handed down from above. The municipality is, in effect, told what to produce and how its resources are to be distributed. The city's top leaders, state and Party, are imposed from above and can be removed at the pleasure of higher authorities. Compared with Western municipalities, Soviet cities are so dominated by higher level authorities that one wonders if it is even appropriate to speak of a Soviet municipal decision-making process, since decisions allocating municipal priorities have already been made elsewhere, in higher state and Party bodies. Yet, ironically, the third feature of Soviet municipal decision making is a slight trend toward greater initiative and autonomy for Soviet municipalities in their self-government. This is evident in housing, in which some municipalities have expanded their rights, and in planning and staffing, where cities are making some gains. Admittedly these gains are small, and they occur mainly in the larger Soviet cities, but when one remembers that the Stalinist days of hyper-centralization still existed less than a generation ago, then these gains in mu-

²⁷ L. Karapetyan and V. Razin, *Sovety obshchennarodnovo gosudarstva* [The Soviets of the State of All the People] (Moscow: Politicheskaya literatura, 1964), pp. 103–104. Fainsod observed that in Smolensk a large number of appointments were made by the Soviet, which had its own *nomenklatura* for "junior appointments" and "technical specialists." "Leading workers," on the other hand, were appointed by the Party (*Smolensk Under Soviet Rule*, p. 100).

municipal autonomy, small as they may seem to Western readers, are significant.²⁸

In the future, decision making in Soviet cities will be influenced by three already noticeable tendencies. First, the nature and quality of Party control is changing. The educational qualifications and professional expertise of urban Party members are rapidly rising and will continue to do so. An analysis of the career patterns and educational backgrounds of 59 municipal Party officials holding an urban post between 1958–1966 shows that 91.5 per cent had a higher specialized education and only 5 per cent had a Party political education.²⁹ These findings seem to confirm that municipal Party officials can be simultaneously red and expert and can possess what Fischer calls “dual leadership skills,” which combine political and technical training.³⁰ Thus the Party, by becoming more specialized and rational-technically oriented, would be able to communicate more effectively with municipal administrators and to control them. Second, municipal administrators are becoming more professionalized. Despite a widespread campaign between 1959–1964 to deprofessionalize administration at the lower levels, the number and quality of full-time municipal administrators actually increased substantially, the average tenure in office rose by 20 per cent in this period.³¹ Crossover between Party and non-Party posts has decreased, and officials are becoming now more committed to long term careers which require extra specialization and early career orientations. These patterns of increased specialization, reduced crossover from Party posts and longer tenure in ur-

ban administrative posts³² are more pronounced in large cities—the most advanced Soviet sector and the model for future development. Finally, more attention is now being paid to the views and complaints of citizens in Soviet cities. *Ad hoc* citizen groups have managed, on occasion, to stop the razing of an historic church by impatient road builders; to publicize the problems of industrial pollution in a number of cities; to get transportation facilities expanded and improved in new urban residential areas. But the Party does not like to see the growth of such groups even if they are only *ad hoc* groups. Instead, citizens are still encouraged to present their grievances in institutionalized meetings with local deputies, by writing letters to *Pravda* and *Izvestia*, and through ritual citizen participation in thousands of Party-controlled public organizations, from apartment house committees to volunteer tree-planting brigades.

The study of Soviet municipal decision making can produce a fresh perspective on the way in which the Soviet political system functions. Since we rarely get any details about how decisions are made at the national level in the USSR, we must generalize from what occurs at the local level when we are fortunate enough to gain this information. Even partial information is welcomed, and it is tempting to make massive generalizations about the nature of Soviet politics when fresh data about aspects of Soviet life and politics are uncovered. I have tried to avoid such generalizations, but the reader is free to draw his own conclusions about how municipal decision making correlates with Soviet national decision making. Certainly it is likely that the same tendencies will occur in the USSR at both the local and the national levels especially in view of the hierarchical and totalist nature of the Soviet political system. The tendencies which I have noticed developing on the municipal level have been seen emerging on the provincial, republic and national levels by other scholars. Nevertheless, one ought to be cautious in making broader generalizations, at least until we have more data for a longer time period.

²⁸ To clarify the expanding rights and responsibilities of Soviet municipalities, the Central Committee of the Communist Party recently passed a major resolution which, “strengthened the material and financial base of city governments” by increasing the sources of municipal revenues. The resolution called for a major increase in municipal autonomy, “so that cities can deal effectively with current problems.” So far, however, it appears that the resolution has not led to any significant restructuring of municipal rights and duties. See, “O merakh po dalneyshemu uluchsheniyu raboty rayonnykh i gorodskikh Sovetov deputatov trudyashchikhsya” [On Measures for the Further Improvement of the Work of the District and City Soviets], *Pravda*, March 14, 1971 (Translated in *Current Digest of the Soviet Press*, XXIII, April, 13, 1971, 1–5).

²⁹ See Frolic, “Some Soviet Elite Comparisons,” *Canadian Slavonic Papers*, 12 (December, 1970).

³⁰ George Fischer, *The Soviet System and Modern Society* (New York: Atherton, 1968), p. 14.

³¹ From a sample of 44 heads of municipal administrations, departments, commissions, and organizations.

³² In Moscow and Leningrad between 1956–1966, the average tenure in office of heads of municipal administrations was approximately six years—twice as long as the average tenure of city Party committee members in both cities during that period. The comparison is not ideal, since city Party committee members perform other full-time jobs as well as holding city Party committee membership, but despite this reservation, municipal administrators clearly serve for much longer terms than most city Party officials.

Can we identify and compare distinctive Soviet and North American urban decision-making models? I think we can, eventually, although the comparisons will take time to develop, and again I want to introduce a note of caution. Because we lack data, it would be best to focus upon particular aspects of decision making, rather than upon the entire process. Municipal issue areas, such as housing and transportation, can be researched using Soviet materials, and we can produce urban elite comparisons from Soviet biographical data. Western scholars may find it necessary, however, to redefine "politics" and "decision making" in order to pursue these comparisons. Our notion of "conflict" poses problems for Soviet scholars

and officials. One Soviet political scientist remarked to me recently, "You Westerners are obsessed with conflict and consequently you seem to have trouble in understanding our system. You insist that conflict exists, is good, and has to be resolved publicly. In our system, however, because of our class viewpoint and the existence of the Communist Party, our bases for disagreement are not the same, and we resolve our disagreements in a different manner." One can only hope that as more Soviet data about decision making are made available to Western scholars, and as we then improve our comparative methodology and conceptualization, we may be able to affirm or refute the validity of his remarks about Soviet decision making.

Interest Groups and Political Integration: British Entry into Europe

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I. Introduction

This paper¹ analyzes the influence of interest groups in the formation of British policy toward successive European unity developments in the years from 1956 to 1967. Considerable attention has been given elsewhere to the subject of Britain and Europe, particularly through detailed accounts of the various negotiations and the development of official views,² but this study seeks to analyze the effect of the major interest groups upon the development of British policy toward Europe in order to test specific hypotheses regarding the relationship between interest groups and the process of political integration.³

¹The study is based on data drawn from pressure group materials and official publications, and from sixty elite interviews with civil servants, former cabinet ministers, party leaders, and officials of the major pressure groups. The major part of the interviewing was conducted in London during the spring of 1967. Subsequent work took place in Oxford and London during 1969-70. For support, I wish to express my appreciation to Harvard University for a Knox Traveling Fellowship, to the Social Science Research Council for a postdoctoral Research Training Fellowship, and to the University of California for a Faculty Research Grant.

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²See, for example, Miriam Camps, *Britain and the European Community, 1955-63* (Princeton: Princeton University Press, 1964), and her subsequent work, *European Unification in the Sixties: From the Veto to the Crisis* (New York: McGraw-Hill, 1966); Nora Beloff, *The General Says No: Britain's Exclusion From Europe* (Baltimore: Penguin, 1963); Uwe Kitzinger, *The Second Try: Labour and the EEC* (Oxford: Pergamon Press, 1968); Pierre Uri, *From Commonwealth to Common Market* (Harmondsworth, Middlesex: Penguin, 1968).

³This effort also differs from previous British pressure group case studies in that it examines neither an important piece of legislation nor the administration of an existing program, but rather the formulation of a policy, and one which is concerned with foreign rather than domestic matters. Cf. James B. Christoph, *Capital Punishment and British Politics: The British Movement to Abolish the Death Penalty, 1945-57* (Chicago: University of Chicago Press, 1962); H. H.

Two important bodies of writing and theorizing can be brought to bear in analyzing the relationship between group behavior and political integration. One of these is the group politics approach, which is set out in a coherent body of interest group theorizing based on British and American politics.⁴ The hypothesis that may be inferred from this approach will here be termed the *group politics hypothesis*. The second major approach is that of functionalism; it figures predominantly in much of the recent work on political integration. In this paper the functionalist approach is extended beyond the area to which it has hitherto been applied, and this extension gives rise to an alternative *functionalist hypothesis*. The following sections will set out the context of the two hypotheses, test them against the empirical evidence derived from the British experience, and then offer some conclusions regarding the theoretical implications of these results.

II. Collectivist Politics in Britain and the Group Politics Hypothesis

This study is placed in the context of the British political process, which Samuel Beer has aptly described as one of "collectivist politics."⁵ It differentiates two channels through which society and government interact. On the

Wilson, *Pressure Group: The Campaign for Commercial Television in England* (New Brunswick, New Jersey: Rutgers University Press, 1961); and Harry Eckstein, *Pressure Group Politics: The Case of the British Medical Association* (Stanford: Stanford University Press, 1960).

⁴See, for example, Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Knopf, 1965); S. E. Finer, *Anonymous Empire*, 2nd ed., rev. (London: Pall Mall Press, 1966); and Eckstein, *Pressure Group Politics*. For the United States, see also Raymond A. Bauer, Ithiel de Sola Pool, and Lewis A. Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (New York: Atherton, 1963); and Lester W. Milbrath, "Interest Groups and Foreign Policy," in *Domestic Sources of Foreign Policy*, ed. James N. Rosenau (New York: Free Press, 1967), pp. 231-261.

⁵Beer, *British Politics in the Collectivist Age*, (see especially Chapter 12). Collectivist or corporatist politics are not confined to Britain: Joseph La Palombara describes in Italy, "a vast network of quasi-corporative relationships between certain interest groups and the administrative agencies." See La Palombara, "The Utility and Limitations of Interest Group Theory in Non-American Field Situations," in *Comparative Politics*, ed. Harry Eckstein and David E. Apter (Glencoe: Free Press, 1963), p. 427.

one hand there is the system of *party government*. Development of the welfare state and the realities of winning power have required that the political parties (each influenced by its distinctive conception of the common good) bid for the votes of consumer groups (blocs of voters), who thereby exercise their influence through the electoral process. The alternative channel linking society and government is that of *functional representation*.⁶ Here, as development of the managed economy has involved the government in extending its control over the economy, the realities of governing in a free society have compelled the government to bargain for the active cooperation of the major interests involved.⁷ Pressure groups embody these interests, and at least one prominent political scientist in Britain finds that they have become "a far more important channel of communication than parties for the transmission of ideas from the mass of the citizenry to their rulers."⁸ Similarly, Harry Eckstein identifies a fading of major class and ideological contours and sees the emergence of a fundamental consensus on general policy matters which relegates conflict to points of detail. These trends lessen the intensity and importance of party competition and enhance the role of pressure groups in the political process.⁹

Several specific bases of group influence exist. To begin with, pressure group *advice* is essential in order for government to obtain basic information and technical knowledge without which economic regulation would be impossible. Next *acquiescence* is a necessity if governmental programs are to operate successfully. Finally, the *approval* of the groups concerned is required if particular governmental policies affecting them are to possess legitimacy.¹⁰ This last consideration reflects a basic collectivist ethos which is shared by the Labour and Conservative Parties. In Labour's case it is expressed in terms of "Socialist Democracy," and for the Conservatives it is embodied in traditions of "Tory Democracy." The collectivist theory of representation, which sets both parties apart from the nineteenth-century

political individualism of the Liberals, is an intrinsic part of twentieth-century British political culture.

In the present context the term "pressure group" is understood to follow the relatively straightforward conception of S. E. Finer. It designates those organizations which are "occupied at any point of time in trying to influence the policy of public bodies in their own chosen direction; though (unlike political parties) never themselves prepared to undertake the direct government of the country."¹¹ More specifically, this paper will concentrate on *sectional* pressure groups, those types of organizations whose political task it is to reflect the interests of the economic or occupational sections they represent. Unlike promotional pressure groups, which are organized around attitudes and which generally seek to persuade people without regard to their sectional affiliation, the sectional groups do not direct their attention to the parties, Parliament, or the electorate, which make up the party government sphere. Instead, as a result of governmental structure, activities, and attitudes, they normally concentrate their efforts upon the administrative departments of government.¹² Thus they typically operate through the functional representation process. Their bargaining power ultimately rests upon their performance of crucial productive functions in the society, and to the extent that government commits itself to intervene in the economy, it must obtain their cooperation. This need can be minimized in a totalitarian system, but in a country with a system of representative government there is little choice but to secure a large measure of voluntary cooperation from the bodies being regulated.¹³ The actual mechanisms for group operation via functional representation are both formal and informal. They include numerous advisory committees on which sectional groups are represented, as well as close personal contact between representa-

⁶ As Beer defines it, the notion of functional representation is one which "finds the community divided into various strata, regards each of these strata as having a corporate unity, and holds that they ought to be represented in government" (p. 71).

⁷ Beer, p. 321.

⁸ R. T. McKenzie, "Parties, Pressure Groups and the British Political Process," *Political Quarterly*, 29 (January-March, 1958), 10.

⁹ Eckstein, pp. 18-19.

¹⁰ Beer, pp. 321-331.

¹¹ However Finer actually prefers the term "lobby" to the term "pressure group." (See Finer, p. 3). In this paper, the terms "pressure group" and "interest group" will be employed interchangeably. There is however considerable—and occasionally tedious—treatment of the definitions elsewhere. See Gabriel Almond and G. Bingham Powell Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966), p. 75; Allen Potter, *Organized Groups in British National Politics* (London: Faber and Faber, 1961); Peter Self and Herbert J. Storing, *The State and the Farmer* (Berkeley: University of California Press, 1963); and Wilson, Christoph, and Eckstein.

¹² Eckstein, pp. 16-17.

¹³ Beer, p. 321.

tives of the interests and their opposite numbers in the Civil Service.¹⁴

While sectional pressure groups have a thoroughly economic basis, they do not function exclusively on the basis of a mechanistic economic self-interest for the following reasons: first, they seek to enhance their own legitimacy by identifying themselves with some conception of broader national interest which their actions are designed to advance. Thus the Confederation of British Industries (CBI, and its precursor, the Federation of British Industries, or FBI),¹⁵ National Farmers Union (NFU), and Trades Union Congress (TUC)—which are the three most important groups and the ones treated in this paper—all articulate their demands above the level of crude advantage for businessmen, farmers, and trade unionists, respectively, even though their particular conceptions of free enterprise, agricultural prosperity, or working-class advancement still fall short of a more or less universalistic public interest. A second restraint upon these bodies is that their role depends on competence in specialized areas. The sectional pressure groups thus focus more upon matters of a technical nature than on overall policy. A third limitation is that the groups devote much of their attention to organizational self-maintenance—providing information and services and seeking to hold together a huge and heterogeneous membership with divergent interests. As a result of coping with internal stresses of this character, a group will often find it difficult to formulate a business, agricultural or labor view at the national level. Fourth, and finally, leadership constitutes a critical variable in the determination of group interest. A change in individuals can produce an entirely different organizational policy. In the present case, a nonactivist FBI President, a staunchly anti-European NFU President, and a pro-Common Market TUC leader were personally responsible for organizational viewpoints which differed sharply from those adopted under leaders who immediately preceded or followed them in office.¹⁶

¹⁴ For a detailed treatment, see Political and Economic Planning, *Advisory Committees in British Government* (London: Allen and Unwin, 1960).

¹⁵ In 1965, the Federation of British Industries merged with two smaller organizations, the National Union of Manufacturers and the British Employers Confederation to form the Confederation of British Industries.

¹⁶ These limitations on British pressure group activity are similar to those found by Bauer, Pool and Dexter in their analysis of American groups. Thus in the United States, the role of a trade association leader may be more that of "arbitrator" among forces within his organization than of "statesman"; he also finds

Despite such restraints, these groups do occupy positions of great power within the British political system. How, then, can the amount of their power—particularly in specific policy matters—be assessed? In dealing with the Italian experience, Joseph La Palombara judged it impossible to measure the influence of groups over administrative decisions.¹⁷ It is possible, however, to attempt such a judgment by utilizing the characterization of the pressure group role put forward by S. E. Finer. He finds that although organized capital and labor in Britain do not dictate public policy, their position in the economy makes it essential that their cooperation be won. Essentially, the relationship is such that "they do not direct but they may veto."¹⁸ In Finer's terms, Britain has reached the position wished for by John C. Calhoun well over a century ago. Calhoun had dreaded the prospect of the "numerical majority" gaining control of government by means of its majority status and then using its authority to oppress minority sections. To forestall this situation he sought to have government regard interests as well as numbers, allowing each a concurrent voice in the making of laws or a veto in their execution. Finer estimates that the major sectional pressure groups have attained exactly this "concurrent majority" status in British domestic politics.¹⁹ This notion of group veto power provides, in the present study, a means of determining whether and under what conditions the groups' position of power carried over into the formulation of Britain's policies toward European unity in the 1956–7 period.

As will be shown below (Part IV), the evidence from successive periods, 1956–60 (negotiations for a Free Trade Area and then a

himself in a position of mediating between his organization and the outside world and is often caught in a web of conflicting forces (p. 331). In addition groups are reluctant to take stands on issues unless there is near unanimity within the organization (p. 337). However, British groups do not suffer from the serious lack of money, skills, and information which Bauer, Pool and Dexter find characteristic of their American counterparts (p. 349). The parallels are further limited because the British groups operate within a corporatist political culture so that their legitimacy is enhanced. They also enjoy far greater "density" (percentage of potential membership actually belonging) and "amalgamation" (extent to which the organized have been brought into one body) than do the American pressure groups. See Beer, p. 332.

¹⁷ La Palombara, p. 425. For an alternative conception, based on Dahl's criteria for the measurement of power as the capacity to shift the power of outcomes see Lieber, Chapter 10.

¹⁸ Finer, p. 27.

¹⁹ Finer, p. 133.

European Free Trade Association), 1961-63 (the Macmillan Government's attempt at Common Market entry), and 1966-67 (the Wilson government's renewal of the Common Market application), indicates a progressive decline in the power of the sectional pressure groups over Britain's European policy making. This decline is caused by effective *politicization* of the European issue, that is, by the perception and treatment of European unity as a matter of major national importance rather than as a relatively specialized or economic question. The result of politicization is to transfer the subject from consideration by the processes of functional representation, in which sectional pressure groups are dominant, to the channel of party government, in which parties play the leading role. Politicization does not require that the issue be subject to partisan dispute, though it may be, for issues can be highly politicized even when there is bi- or tri-partisan accord.

It should be made clear that the term politicization is applied in this paper only when the overall *treatment* of policy merits the label. In one sense, to be sure, all the interactions analyzed here are political in their content, as exemplified by the kind of argument which Almond and Powell offer in their definition of the political system:

When we speak of the political system we include all of the interactions which affect the use or threat of use of legitimate physical coercion. The political system includes not only governmental institutions such as legislatures, courts and administrative agencies, but *all structures in their political aspects*. Among these are traditional structures such as kinship ties and caste groupings; and anomic phenomena such as assassinations, riots and demonstrations; as well as formal organizations like parties, interest groups, and the media of communication.²⁰

As a result, the usage adopted in this study can be regarded as only a specialized case within Almond and Powell's all-encompassing framework.²¹ It should also be understood that politicization applies here to the status of those

issues which enter into the public domain. Thus, as alternative designations for politicization and nonpoliticization, respectively, we can make use of the terms "publicization" and "privatization," as suggested by Beer.²²

To be more systematic, it seems useful to identify three necessary indicators of politicization. The *first* of these is the handling of an issue by a primarily political ministry (such as the Foreign Office) rather than by an economic one (such as the Board of Trade). The two types of ministry can be differentiated on the basis of what provision they make for group access. While the Board of Trade accords groups an intimate corporate role, so that extensive consultation occurs between civil servants and their opposite numbers in the sectional groups, the Foreign Office makes no such provision. Although it may receive deputations from groups such as the TUC, these are heard as bodies of substantial importance within the country rather than in their capacity as sectional representatives. Routinized consultation and bargaining are central to the functional representation process; the shift of attention away from the economic ministries tends to prevent their operation.²³

The *second* indicator is the existence of involvement by the broader public. At a minimum it implies that an issue passes from the exclusive scrutiny of an elite or specialized audience to the notice of the attentive public.²⁴ This wider attention is reflected in the activity of promotional pressure groups and attention to the issue in the communications media (including the mass press) and in by-elections and opinion polls. The significance of

²² This is also akin to the distinction drawn by E. E. Schattschneider between "socialization" and "privatization" of conflict. Expanding the scope of a previously privatized conflict brings the public, or "audience" into involvement, thus changing the coalition possibilities and decisively affecting the outcome. *The Semi-Sovereign People: A Realist's View of Democracy in America* (New York: Holt, 1960), pp. 2-8.

²³ One listing of 700 central advisory committees presented to the House of Commons in 1949 included only two attached to the Foreign Office and both of these were highly specialized. (The two committees were in the Foreign Office's German Section: The Book Selection Committee and the Scientific Committee). See Allen Potter, pp. 223-225.

²⁴ "Elite" and "attentive public" are here used in the manner of Gabriel Almond's basic approach. The attentive public is "informed and interested in foreign policy problems, and . . . constitutes the audience for the foreign policy discussions among the elites." The "policy and opinion elites" are "the articulate policy-bearing stratum of the population which gives structure to the public, and which provides the effective means of access to the various groupings." *The American People and Foreign Policy*, rev. ed. (New York: Praeger, 1960), p. 138.

²⁰ Almond and Powell, p. 18. Italics in original.

²¹ Politicization is also used in different contexts, but with related meanings, by Karl Deutsch *et al.*, *Political Community and the North Atlantic Area* (Princeton: Princeton University Press, 1957), pp. 46-47; and by Ernst B. Haas and Philippe C. Schmitter, "Economics and Differential Patterns of Political Integration: Projections About Unity in Latin America," in *International Political Communities: An Anthology* (Garden City, N.Y.: Anchor, 1966), pp. 261-262.

According to the P.E.P. study, a nonpolitical subject "is merely one about which politicians do not feel strongly for the time being" (p. 106).

such involvement is that it conflicts with the necessarily private process of bargaining between government and sectional pressure groups which makes up functional representation.

The *third*, and most important of the indicators, is the participation of the political parties. Since public opinion sets few limits on foreign policy²⁵ the crucial element in politicization, and in the limitation of group influence, becomes the political parties. Party involvement interferes with the exclusively technical consideration of an issue in a closed relationship between administrative department and sectional pressure group. It also provides the opportunity—though certainly not the assurance—that judgments may be rendered more often on the basis of an interpretation of broad national interest than on some kind of cost-benefit calculations important mainly to sectional interests. This impact of party involvement stems not only from the fact that parties are organized around some distinctive conception of the common good—no matter how vague or vestigial—but also because those interests which the parties do heed in their search for support and in bidding for votes have to be aggregated with numerous other interests. Inherently this aggregation implies dilution. For politicization to become *effective* in the limitation of group influence, one other dimension must be present: the issue involved must be treated as falling within the realm of diffuse or general-purpose politics rather than of functional or special-purpose politics. The distinction resembles that made by Stanley Hoffman between “high politics” and “low politics.”²⁶

If the above factors are indicators of politicization, what are its causes? The onset of politicization appears to be determined by a combination of external events and conscious choices. Thus a pressing international situation may thrust a subject into the forefront of national attention, or at least make such treatment possible. What events do is to create a propensity toward politicization. They constitute the nec-

essary, if not always the sufficient, condition. Internally, a conscious choice by political or governmental figures is almost always essential before politicization can occur. For example, despite the importance of the EEC, neither Conservative nor Labour leaders consciously chose to treat European unity as a salient political matter until 1961.

The importance of the above treatment of the circumstances in which pressure groups rather than parties can be expected to influence policy is that pressure groups are different from parties in ways that directly affect issues such as those involving European unity. Basically, pressure groups influence policy formation whenever the criteria for judgment are based more upon the foreseeable balance of economic gain and loss than upon broader conceptions of national interest or political benefit. As a result, policy initiatives which threaten identifiable costs to significant interests, even though these may represent a minority of the sector involved, are likely to be opposed. Comprehensive groups, such as the CBI and NFU, will tailor their positions so as not to outrage important sectors of their membership; and in a setting which remains nonpoliticized, political parties will play little role in the policy process. The result, therefore, is likely to be that profound new ventures such as those involving European unity cannot be made so long as pressure groups provide the main constituency for policy makers.

While the group politics theorists do not deal with the phenomena of supranational integration per se, it does seem possible to extend their analysis in order to do so. Based on the above arguments, such an extension would logically imply that when an integration issue is nonpoliticized (i.e., treated as a technical matter), groups will tend to influence policy formation, because cost-benefit calculations become the criteria for judgment, and movement toward integration is likely to be hindered. Put more economically, this extension of the group politics position gives rise to an hypothesis: *If interest groups influence policy formation, then progress toward integration is likely to be impeded.*

By implication, progress toward any major new initiative in European unity (that is, obtaining a British decision to join an integrated European organization) will require that an issue become effectively politicized.

III. The Functionalist Hypothesis

Functionalism shares basic assumptions with group politics in the sense that it implies there

²⁵ British voters tend to be bi-partisan on foreign policy, and concentrate their attention on domestic matters. Their tendency to vote on the basis of domestic issues is analyzed in Max Beloff, *New Dimensions in Foreign Policy: A Study in British Administrative Experience, 1947–1959* (London: Allen and Unwin, 1961), p. 15; Jean Blondel, *Voters, Parties and Leaders* (Harmondsworth Middlesex: Penguin, 1966), pp. 75–79, 81–83, 87; and Kenneth Younger, “Public Opinion and British Foreign Policy,” *International Affairs*, 40 (January, 1964), 22–23.

²⁶ See, e.g., “European Process at Atlantic Cross-purposes,” *Journal of Common Market Studies*, 3 (February, 1965), 92.

will be group influence if an issue (specifically one involving integration) is nonpoliticized. But, unlike the group politics analysis, it finds nonpoliticization to be advantageous for integration.

The basic arguments of functionalism are most commonly associated with David Mitrany and Ernst Haas.²⁷ The essence of Mitrany's position is that the surest route to world peace is to organize a multitude of international organizations around functional tasks such as health, transport, and postal services.²⁸ As governments eventually cede more and more of their tasks to these worldwide organizations, a diminution of national antagonisms will result. Functional institutions will gradually bring about a sense of community; ultimately, peace, world government, and political integration will follow. Mitrany's thought is usually contrasted with that of the federalists, but he also stands apart from present functionalist theory in certain respects, particularly in his stress upon functional organizations which are universal rather than regional in their membership. Nonetheless, that aspect of the Mitrany thesis which is most relevant to the present discussion and which is shared by contemporary functionalists, is that the surest route to political integration is to depoliticize the subject by concentrating on technical or functional tasks.

For Ernst Haas, who has developed important modifications of Mitrany's approach, the essence of functionalism is that step-by-step economic decisions are superior to crucial political choices and that the operation of ever more controversial policies, starting from a shared interest in economic welfare, will, with a certain automaticity, bring about the establishment of a new central, supranational authority regardless of the wishes of individual actors. Haas's functionalism assumes that economic self-interest is more important than political commitment and that unintended consequences and incremental decision making are more effective in bringing about integration

than are purposive behavior or the construction of grand designs. Basing his analysis upon the Western European experience, Haas identifies a natural economic progression which has led the six members of the European Economic Community (EEC) from their initial common community treatment of coal and steel (in the European Coal and Steel Community) to refrigerator tariffs, and then to chickens, to monetary policy, and ultimately to a mutual interest in the operation of the business cycle.

Initially, Haas argued that in modern democratic industrial society, particularly that of Western Europe, there was no longer a distinctly political function, separate from economics or welfare or education, that existed in the realm of foreign policy, defense, and constitution making. He also viewed the advent of supranationality within the EEC as symbolizing the "victory of economics over politics," thus signaling the demise of the ethnocentric nationalism which preferred guns to butter, passion to reason, and excited demands to statistical bargaining.²⁹ During the past decade, Haas has modified his dismissal of the political function. He acknowledges that *The Uniting of Europe* erred in assuming the permanent superiority of step-by-step economic decisions over crucial political choices, and takes the position that while integrative decisions based on high politics are more durable, in the absence of a statesman with the vision to weld disparate publics together "we have no alternative if we wish to integrate a region, but to resort to gradualism, to indirection, to functionalism."³⁰ In sum, he maintains that subject to amendments, the expansive logic of functionalism remains valid.³¹

Yet Haas does characterize the relationship between economic and political union as a "continuum."³² And his functionalism continues to imply that a kind of benign invisible hand will operate to bring about supranational integration in a nonideological, depoliticized economic setting. Since government concentrates increasingly on technical questions of an economic nature, political life more and more revolves around interest groups seeking narrow

²⁷ See especially, Mitrany's *A Working Peace System: An Argument for Functional Development of International Organization* (London: Oxford University Press, for the Royal Institute of International Affairs, 1943); also Haas's *The Uniting of Europe: Political, Social and Economic Forces, 1950-1957* (Stanford: Stanford University Press, 1958), and his *Beyond the Nation-State: Functionalism and International Organization* (Stanford: Stanford University Press, 1964).

²⁸ For a useful summary of Mitrany's functionalism and its differences from later interpretations, see Andrew Wilson Green, "Review Article: Mitrany Reread with the Help of Haas and Sewell," *Journal of Common Market Studies*, 8 (September, 1969), 50-69.

²⁹ "Technocracy, Pluralism and the New Europe," in *A New Europe*, ed. Stephen Graubard (Boston: Houghton Mifflin, 1964), p. 71.

³⁰ "The Uniting of Europe and the Uniting of Latin America," *Journal of Common Market Studies*, 5 (June, 1967), 327-328.

³¹ Haas, "The Uniting of Europe . . .," p. 321.

³² Haas and Schmitter, "Economics and Differential Patterns of Political Integration . . ." p. 261. For Haas's revisions to his original theorizing, see "The Uniting of Europe . . ." and his 1968 Preface to the reissued edition of *The Uniting of Europe*.

advantages, and public policies are regarded as the outcome of these maneuverings along with other economic and political forces.³³ Haas stresses the growth of interest groups across frontiers, and a chief conclusion of his main work is that group pressure will shift from a national to a community level and have the effect of stimulating integration as groups organize across national boundaries to influence policy.³⁴ His pluralistic thesis holds that in advanced industrial democracies, a larger political community can be developed if crucial expectations and behavior patterns of key groups can be refocused on a new set of central symbols and institutions; and over time, political loyalties, following economic interests, will gradually become attached to new supranational institutions.

It is true that functionalism has been more concerned about integration within existing communities, where processes such as spillover do in fact seem to take place, than about the geographic expansion of integration or the construction of new communities.³⁵ Leon Lindberg and Stuart Scheingold have characterized the former type of integration, to which Haas's expansive logic of functionalism has been applied, as "forward-linkage growth." This process involves primarily incremental change and the projection of well-established trends. They label the latter type or process as "systems transformation." This category includes not only the adherence of new members to an established community, but also other large changes which mean the introduction of new constituencies such as those involving a notable increase of integrated activities within an existing community.³⁶ In fact, the functionalists generally have not sought to extend their argument to deal with the adherence of a new member such as Britain to an established supranational entity. But in seeking to analyze the British case, we nonetheless may find it worth the effort to extend the functionalist approach in order to determine whether it could also account for this kind of effort at systems transformation. Such an extension would logically imply that when-

ever an integration issue is nonpoliticized, groups will tend to influence policy formation, because cost-benefit calculations become the criteria for judgment, and movement toward integration is likely to be facilitated. Put more economically, this extension of the functionalist position gives rise to the following hypothesis: *If interest groups influence policy formation then progress toward integration is likely to be facilitated.* The difference between the hypotheses here imputed to the group politics and functionalist approaches is that while both agree upon a significant role for interest groups in the absence of politicization, the first hypothesis asserts that this role will hinder integration, while the second predicts the opposite—that it will facilitate integration. The difference of orientation is evident in the notion of group veto power identified by the group politics analysis, as compared to the stress on technocratic politics and the attribution of an integrative group role by the functionalists.

With these two hypotheses in mind, it is now appropriate to see how each one holds up against the British experience. The period concerned begins with Britain's effort to come to terms with the newly established European Economic Community by means of the ultimately abortive Free Trade Area during the years from 1956 to 1958. It then continues with the establishment of the European Free Trade Association during 1959–60, the effort of the Macmillan Government to join the Common Market beginning in 1961 and vetoed by President de Gaulle in January 1963, and—following a hiatus of more than three years—the similarly fruitless renewal of Britain's application by the Wilson Government in 1966–67.

IV. Pressure Groups and British Policy: 1956–67

A. The Free Trade Area and the European Free Trade Association. During the years from 1956 to 1960, the period of policymaking for the Free Trade Area and the European Free Trade Association, the European issue remained almost completely nonpoliticized. Neither external events nor the choices of party or governmental leaders compelled a political treatment. The three indicators reflected this lack of politicization. First, at the administrative level, policy formulation and international negotiations were conducted by the Board of Trade with some participation by the Treasury. The Foreign Office did not take part. Second, generalized debate and involvement by the broader public were largely absent. The mass press gave little coverage to the FTA and EFTA, particularly

³³ See Michael J. Brenner, *Technocratic Politics and the Functionalist Theory of European Integration* (Ithaca, New York: Cornell University Center for International Studies, 1969), p. 5.

³⁴ According to Haas, perhaps his "chief finding is that group pressure will spill over into the federal sphere and thereby add to the integrative impulse." *The Uniting of Europe*, 1968 edition, p. xxxiii.

³⁵ Brenner discusses this on p. 8.

³⁶ *Europe's Would-Be Polity: Patterns of Change in the European Community* (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 244.

in comparison with its later handling of the Common Market question, and there was no mobilization by promotional pressure groups. Third, the parties remained largely uninvolved, particularly because they shared similar official attitudes and because the subject of European unity was widely perceived as economic in content.³⁷

Interest groups enjoyed concurrent majority power during this period; they succeeded in maintaining a political position similar to the one they customarily occupied in domestic politics. That is, they exerted influence because the executive found it necessary or desirable to bargain for their cooperation. Though the groups paid more attention to technical aspects than to overall policy, this was consistent with their usual pattern of operation on domestic matters.

Throughout this period, all three of the major groups were hostile to the idea of outright Common Market membership. For example, the TUC was completely opposed, fearing that the Common Market would constitute a first step toward complete integration and the development of a central authority which could overrule national governments on important economic matters. TUC leaders felt such a change would be undesirable since they viewed most governments of the Six as having less favorable ideas on economic and social policy than the British government, albeit a Conservative one. Even more important for the TUC—as for the other sectional interest groups—was the structural change involved. A transfer of authoritative decision-making power to a supranational body would drastically weaken the effective influence of the TUC. In a new European Community, the sectional veto which British trade unions often hoped to exercise upon their own government's policies would no longer be so significant. That is, even though operation of a concurrent majority might persist at the national level, its existence would be far less important because the determination of economic policies would have been removed to a supranational authority upon which the British government—and the British public—would have only partial influence.

During the years from 1956 to 1958 Britain unsuccessfully sought to negotiate establishment of a wide Free Trade Area linking the six members of the newly formed Common Market with the other Western European members

of the OEEC. What is noteworthy about this period is the extent to which the pressure groups not only resisted any thought of membership in a supranational EEC, but also took a very cautious position toward even the very limited European arrangements in the proposed Free Trade Area. Thus, in the case of the Federation of British Industries, while the larger business firms and the FBI leadership were favorable to European free trade, many of the smaller firms expressed concern about meeting increased competition; given the nature of the FBI as an organization, this apprehension meant that its policy had to remain a cautious one. As for the National Farmers' Union, it categorically refused to consider any European agreement which would include agriculture. By its success in delaying until October 1957 a governmental move to meet demands by the Common Market countries for some provision for increased agricultural trade, it played a significant role in hindering European agreement on the FTA. Even the TUC's policy toward the FTA concentrated almost entirely on safeguards for full employment instead of on the opportunities for economic growth offered by the proposed trading arrangements.

There is every reason to judge, as Miriam Camps does, that the government felt its main task lay in defeating internal opposition to free trade and in winning over business and labor, rather than concentrating on gaining agreement with the Six.³⁸ The government's decision to exclude agriculture from the initial FTA proposals prompted a very explicit acknowledgment of the groups' importance from a Labour MP:

The only really effective argument which I have heard for excluding foodstuffs from the scheme is that if we are going to have trouble with pressure groups and with the FBI, why should we have trouble at the same time with the NFU? From a tactical point of view, there is much to be said for taking on these pressure groups one at a time.³⁹

Most of the European countries wanted Britain in Europe for political reasons, but France was unwilling to pay Britain's price and later vetoed the FTA in November 1958. The question, then, is why the UK required such a price, which was ultimately to preclude the successful establishment of a Europe-wide FTA. The answer is to be found in the requirements of British pressure-group politics. The groups were highly active participants in the formulation of British policy, by virtue of their concurrent majority powers, yet they played a role that di-

³⁷ E.g., Edward Heath told the 1960 Conservative Conference that the government had sought to create the FTA and EFTA "for economic reasons." National Union of Conservative and Unionist Associations, *79th Annual Conservative Conference* (October 12-15, 1960), p. 61.

³⁸ *Britain and the European Community*, pp. 104-105.

³⁹ Fred Mulley in Hansard, *Parliamentary Debates* (Commons), 561 (November 26, 1956), c. 80.

rectly opposed European integration and emphasized sizeable economic safeguards, even upon the quite limited FTA arrangements.

In 1959–60 (the period during which the European Free Trade Association, or EFTA, was formed), the sectional pressure groups again played an intimate role in the shaping of policy, one which went well beyond a mere veto power. In fact the Federation of British Industries, through meetings with its Scandinavian counterparts during the winter of 1958–59, played a substantial part in moving the British government toward the establishment of the EFTA.⁴⁰ The FBI, in close cooperation with the NFU overtly worked to secure the establishment of EFTA at a time when government probings were still without commitment. FBI activities gave prominence to the EFTA option, and at the very least this advocacy made it possible for the government to proceed with EFTA in the knowledge that such a course of action would benefit from firm FBI support. In the face of industrial disapproval, the EFTA choice would have been unlikely.

As in the case of the Free Trade Area, the EFTA issue remained almost entirely nonpoliticized. Because of the economic and commercial treatment of the European issue throughout 1959, the FBI's involvement and importance came naturally. To describe the government as having taken the more narrowly economic view is no idle generalization. In their failure to adopt a wider outlook, British leaders paid little attention to the political dynamics behind the Common Market; they seldom displayed awareness of the Common Market countries' sense of building Europe and of laying to rest the antagonisms which had caused two World Wars.

While the interest groups played a significant role in a depoliticized setting, their participation was not such as to contribute to integration, particularly in terms of Ernst Haas's definition, widely employed by the functionalists, which sees integration as:

the process whereby political actors in several distinct national settings are persuaded to shift their loyalties, expectations, and political activities toward a new and larger center, whose institutions possess or demand jurisdiction over the pre-existing national states.⁴¹

⁴⁰ The original seven members of EFTA were Sweden, Norway, Denmark, Switzerland, Austria, Portugal and Britain. The arrangements resembled those which Britain had sought for the abortive FTA, namely freer intragroup trade, a minimum of institutions and of integration, and no harmonization of external tariffs.

⁴¹ "International Integration: The European and the Universal Process," in *International Political Communities*, p. 94.

But the merely intergovernmental arrangements of the EFTA did not begin to provide a setting in which institutions or arrangements existed toward which previously national loyalties might be transferred. By continuing to oppose British participation in genuine integration of the EEC variety, interest-group activity impeded integration. Indeed, the groups contributed to the formulation of a policy and to the establishment of an organization which actually hampered British efforts to come to terms with the Six. The existence of EFTA complicated Britain's task in seeking Common Market membership, both technically and—as it contributed to the Six's suspicion of British motives—psychologically.

It thus becomes clear in looking at the 1956–60 period that a nonpoliticized treatment of European policy would not suffice to move Britain into an integrated European arrangement. As long as the setting remained nonpoliticized, interest groups would play a significant role in policy making, at least to the extent of preserving veto power, but this role would be in opposition to rather than in the direction of genuine integration. The contrast with 1960–61 is dramatic, for no British movement would be possible until the European issue became politicized.

B. Common Market I: 1961–63. In the case of Britain's first Common Market application, during the period 1961–63, the key decision was taken by Prime Minister Macmillan when he decided to abandon Britain's aloofness from European integration and to seek membership in the European Economic Community. The overwhelming preponderance of views by elite respondents interviewed for the present study was that political motivations (both short- and long-run) predominated. To a slightly lesser extent, these respondents also concurred in the observation that once the decision was made, Macmillan presented the European choice to the British public as though Common Market membership were primarily an economic matter. The result was that there were really two distinctly different phases to the British approach, the first being that of the initial decision to seek entry, the second consisting of the period of actual negotiation. Treatment of the European issue, the role of pressure groups, and the nature of the policy process differed sharply between these two phases.

As for the initial decision, its aims corresponded with the conclusions of an early 1960 report by the Economic Steering Committee under the chairmanship of Sir Frank Lee

(then newly appointed as Joint Permanent Secretary to the Treasury). This interdepartmental group of senior civil servants had been assembled to study possible association with the Six, but concluded that Britain should seek outright membership in the Common Market, for primarily political reasons.⁴² The Committee regarded the economic arguments as at best balanced; for them, entry was a political, not an economic question. They expected Britain to exert leadership within a united Europe, thus regaining a certain measure of world influence and even improving her transatlantic ties.

In short-range political terms, European integration offered a novel issue on which to set the Conservatives apart from the Labour Party, following a period in which the two major parties had drifted toward the political center as factors of ideology and class diminished. Finally, Macmillan himself, seeking a lasting contribution with which to cap his political career, had long been favorable toward European unity.⁴³ Macmillan had become Prime Minister in early 1957, and having disposed of the urgent tasks of liquidating the Suez venture, assuming control of the Conservatives, and rallying his dispirited Party for an overwhelming victory in the 1959 election, he felt that circumstances in 1960-61 at last seemed appropriate for seeking Common Market entry.

The available evidence indicates that the Prime Minister made his decision in late December 1960. It is quite clear—especially in comparing the chronology of public statements and private documents of the sectional interest groups—that Macmillan's decision antedated group advocacy of Common Market membership. This finding offers no comfort for the functionalists. The distinctly political function is evident; its existence is a prime requisite for the critical transformation of British policy from opposition to European integration to participation in it. The key point here is that the July, 1961 announcement that Britain would seek EEC entry reflected a political judgment, made by political authorities before pressure groups had strongly articulated their own interests. In particular it meant that considerations of broad national interest, construed in terms of Britain's overall international position,

took precedence over cost-benefit calculations about the effects of Common Market membership on individual economic interests. It also reflected the existence of policy powers held by the Prime Minister and Cabinet which could be exercised relatively free of the constraints that interest groups typically imposed on lesser domestic issues. But because Macmillan would not or could not make the case to the British public in grandly political or Churchillian terms (as opposed to less ambitious commercial ones), the subsequent formulation of Britain's negotiating position offered the opportunity for a powerful pressure-group role.

Politicization of the issue of European unity was evident throughout the period of the first Common Market application, and the three indicators reflect this fact: first, the negotiations were no longer handled by ministers with primarily economic departmental responsibilities, but by Edward Heath, then number two man at the Foreign Office. Second, the broader public was drawn into the debate. Promotional pressure-group activity became widespread; mass circulation papers such as the *Daily Mirror* and *Daily Express* launched strident campaigns for and against entry; and polls and by-elections reflected the Common Market's position as a highly visible subject of debate. Third, the political parties became deeply involved and even made European unity a matter of partisan contention. Yet while there was full politicization of the European issue, in the sense of publicization, during the actual negotiation period, politicization did not become *effective* because Macmillan chose to treat the Common Market as an economic issue. He operated, in the words of Richard Neustadt, "by disguising his strategic choice as a commercial deal."⁴⁴ The Prime Minister's decision to deal with the issue as one of functional politics rather than of general purpose politics, facilitated the persistence of functional representation and hence assured a significant pressure-group role in shaping the practical terms of entry to be sought by Britain.⁴⁵

The increasing opposition to Common Market entry by the Labour Party also enhanced the significance of sectional demands because,

⁴² Camps, *Britain and the European Community*, pp. 280-281.

⁴³ Macmillan later wrote in his memoirs, "About Europe, regrets still haunt me," and he recalled that he had written Churchill in protest when the newly elected Conservative government of 1951 failed to move Britain toward the European Coal and Steel Community. *Sunday Times* (London), July 31, 1966.

⁴⁴ "Whitehouse and Whitehall," Paper delivered at the 1965 Annual Meeting of the American Political Science Association, Washington, D.C., September 8-11, p. 9.

⁴⁵ The negotiations also became mired in detail, a prime area of group operation, because the Government sought to negotiate *à sept* (rather than regard the Six as a single group with a coherent position), because it felt obliged to negotiate on behalf of the Commonwealth, and because the openness of the negotiations frequently involved Edward Heath in an embarrassing dialogue with the groups.

lacking bipartisan reinforcement for a policy which he had described in economic terms, Macmillan needed to assure himself of the fullest possible support within his governing Conservative Party. In this case, because the Tory constituency was composed very considerably of the agricultural, industrial, and commercial portions of the society, sectional groups such as the NFU and FBI actually were able to exert influence via the Prime Minister's own party.

The relationship between domestic and international forces is especially visible in the 1961-63 period, with Macmillan becoming increasingly trapped between domestic political needs and the necessities of the Brussels negotiations. An easier domestic situation would have allowed the Government to accept the EEC's Rome Treaty more unreservedly. This acceptance in turn would have facilitated mutual concessions by the Six and a speedier course for the negotiations. As Kenneth Waltz has observed, however, Macmillan failed publicly to confront the European issue as one which challenged long-existing national notions.⁴⁶ Instead, in order to gain domestic approval for Common Market entry, he pledged to obtain safeguards which were virtually incompatible with full British membership in the EEC. The British reservations were not dictated by public opinion; they were established on the basis of consultations with the major interests.⁴⁷ The consequences of this approach were to hinder the negotiations at Brussels, and then to undercut the Government's domestic position because the necessary retreat from the initial conditions was interpreted at home as a series of surrenders. These difficulties facilitated the subsequent de Gaulle veto.

Once again, interest groups exerted an influence which cannot be said to have promoted integration. The case of the FBI is the most interesting. During 1956-60, British business had perceived the trend toward European unity more as a threat than an opportunity, though near the end of 1959 a number of larger business firms became more favorable toward Europe because of their concern with stagnating British Commonwealth trade as contrasted to growing opportunities within the EEC. Smaller firms remained more hostile, whether because of vulnerability in the home market or less imaginative leadership, but they sometimes expressed their fears in political rather than eco-

nomic terms. By the spring of 1961, the heads of giant firms such as Imperial Chemical Industries, General Electric Corporation, and British Motor Corporation had begun to advocate outright entry into the Common Market rather than some more limited form of association. The FBI, caught between conflicting views, issued a major report in July 1961, two weeks before the Government's announcement. This report stressed the need for Commonwealth and EFTA safeguards, termed many of the EEC's agricultural arrangements unacceptable, expressed a preference for intergovernmental cooperation rather than common European institutions, and also took note of numerous "negotiable" lesser problems.⁴⁸ While the FBI had taken one important step in its willingness in principle to accept a common external tariff, even this declaration was negatively phrased.⁴⁹ The whole tenor of the FBI report was legalistic and parochial; the terms it stipulated remained quite irreconcilable with Common Market membership—especially because of the position on agriculture and common institutions. At the same time however, a survey of 130 large firms found more pro-EEC sentiment.⁵⁰ The leaders of some of Britain's largest industries were clearly moving toward European unity faster than the FBI was, though even these industrialists moved no more rapidly than Prime Minister Macmillan.

During the actual period of negotiation for Common Market entry, the role of the FBI vis-à-vis the government and its own membership shifted somewhat. The FBI maintained a close relationship of sustained consultation with the government, expressing concern over vulnerable sections of industry and also offering technical advice. Because outright hostility on the part of business would have prevented the Common Market application altogether, the British government needed to devote considerable care and attention to entertaining the representations and occasional protests of the FBI (and of the TUC as well) in order to insure that they continued to provide support, or at least acquiescence, for entry. With the passage of time, and following the lead of the larger industries, the FBI became increasingly pro-EEC and directed substantial effort at educating and persuading its own membership as to the benefits of entry.

It was the National Farmers' Union, having

⁴⁶ *Foreign Policy and Democratic Politics: The American and British Experience* (Boston: Little Brown, 1967), p. 266.

⁴⁷ This point is emphasized by Lord Windlesham in *Communication and Political Power* (London: Jonathan Cape, 1966), p. 158.

⁴⁸ Federation of British Industries, *British Industry and Europe* (London, July 1961).

⁴⁹ "In principle we do not oppose the suggestion for a common or harmonized tariff put forward by HMG." Federation of British Industries, p. 3.

⁵⁰ *Sunday Times* (London), July 16, 1961.

the most to lose, which exerted the greatest influence upon the British position. The farmers may not have been able to veto entry itself, but they did succeed in shaping the conditions under which Britain sought that entry. The NFU first obtained a governmental pledge, in a January, 1961 White Paper, that the government would be concerned to see whether closer European unity could be achieved "without sacrificing the vital interests of UK farmers and horticulturalists."⁵¹ Next, in the official July, 1961 announcement, the Prime Minister incorporated safeguards for British agriculture as one of three essential British conditions in negotiations with the Six.⁵² Finally, and perhaps more importantly, agriculture received its most specific pledge in the speech made by Edward Heath in submitting Britain's application to the Ministers of the Six at Paris on October 10, 1961. In a speech otherwise regarded as one of wholehearted commitment to Europe, Heath enumerated three necessary safeguards for the vital interests of British farmers: a transition period of 12–15 years, British retention of the ability "to use such measures as are necessary to safeguard our farmers' standard of living," and special arrangements for horticulture.⁵³ Eventually, the problem would be that the Six would find each of these elements largely unacceptable, and Britain would have to retreat from them if she wished to reach agreement.

Thus from July, 1961 to the January, 1963 de Gaulle veto, the British government was seeking to placate specific economic objections of the major interest groups in order to insure that Common Market entry would have domestic political support. The groups could not assert their inherent involvement in the central political issue of whether Britain should merge her destiny with that of the Europeans; but because the issue was incompletely politicized (i.e., treated as functional or economic, despite publicization), these groups still could claim direct concern with portions of the European issue involving trade, full employment, and agricultural arrangements. The groups thus retained an approximate concurrent majority role in the formulation of Britain's negotiating position.

C. Common Market II: 1966–67. The orienta-

⁵¹ Cmnd. 1249 (Her Majesty's Stationery Office, December 19, 1960), paragraph 37, cited in *British Farmer*, No. 163 (January 7, 1961).

⁵² Hansard, *Parliamentary Debates* (Commons), 645 (July 31, 1961), c. 1483–88.

⁵³ *The United Kingdom and the European Economic Community*, Cmnd. 1565, Vol. 36 (HMSO, November, 1961), pp. 14–15.

tion of the Wilson government toward Europe provides some useful contrasts to the 1956–60 and 1961–63 periods. During the 1966–67 attempt at Common Market entry,⁵⁴ the effectiveness of politicization rendered group interests relatively insignificant. Unlike Macmillan, Harold Wilson was thoroughly political in the way he handled the European negotiations and the way he presented them to the British public, as well as in his decision criteria.⁵⁵ Wilson's effort was thus virtually unimpeded by the requirements of pressure-group politics.

This difference of approach resulted from a combination of external events and conscious choices. The urgency of events, particularly the collapse of alternatives, resulted in a strong propensity toward politicization; and the nature of Wilson's choices, most notably the decision to treat the European commitment as a major national departure, (i.e., as a matter of general, or high, politics rather than of functional, or low, politics) resulted in an effective politicization.

Sectional pressure groups therefore found themselves in a position of unprecedented weakness. Effective politicization had closed the channel of functional representation, and a number of conditions, including the existence of bipartisan agreement over European policy, discouraged group access through the channel of party government. Unlike Macmillan, Wilson made no pledges to pressure groups; he also sought entry with far fewer conditions.⁵⁶

⁵⁴ The Labour government's approach to the EEC began in November 1966 with exploratory talks. The application was formalized in May 1967, then vetoed by de Gaulle in November 1967. Prime Minister Wilson initiated a renewal of the application in 1969–70, and Labour was then defeated in the June 1970 General Election. The incoming Heath government continued with the negotiations and reached agreement with the Six in June 1971. The later phase is beyond the scope of this paper.

⁵⁵ This political emphasis was quite obvious. See, e.g., Kitzinger, *The Second Try*, pp. 9–10.

⁵⁶ The unconditional nature of the Labour government's application is particularly visible when its terms, as set out by George Brown at The Hague on 3–4 July, 1967, are contrasted with those outlined by Edward Heath in Paris on 10 October, 1961. Thus, for example, on agriculture, Brown accepted the EEC's Common Agricultural Policy without safeguards, while Heath sought to retain Britain's existing safeguards for her farmers. On the Commonwealth, Brown cited only New Zealand and the need to avoid defaulting on the Commonwealth Sugar Agreement; Heath listed each country, requested "comparable outlets," and said Britain could not join if trade was cut with a severe loss to the Commonwealth. On EFTA, Brown asked only one year's standstill for its members "to make their own arrangements," while Heath said Britain could not join unless all seven members "could participate, from the same date, in an integrated European market." On collective action, Brown ac-

Moreover, Wilson centered the work of his negotiating team in the Cabinet Office, where he himself would be in control. The sectional interests found themselves fobbed off upon a consultative committee operating through the (short-lived) Department of Economic Affairs and providing them with occasional briefings and only *pro forma* consultation rather than genuine bargaining. Groups might still influence lesser technicalities, but they were left without a vestige of concurrent majority powers.

One lesson suggested by the 1966-67 period is that the perceived precedence of external international factors dictated that the European approach would be unimpeded by the stipulation of conditions. In its Common Market approach, the Macmillan government had acknowledged no such predominance of external concerns and had consequently devoted more attention to domestic "consensus" than to international "compatibility."⁵⁷ But Prime Minister Wilson's desire to avoid presenting President de Gaulle with a genuine pretext for another veto required a de-emphasis of domestic considerations, which, in turn, meant the demise of functional representation. Labeling the changed treatment as effective politicization implies that a broadly construed national interest had become the dominant concern; this heightened national interest transcended the previous pattern in which sectional pressure groups had exercised effective bargaining power in the absence of a clear-cut invocation of Britain's national interest. As in the case of the 1961 Common Market application, political authorities had again made the crucial decision in advance of, or without real regard for, the sectional pressure groups' strong articulation of their positions. But unlike the earlier period, during 1966-67 effective politicization continued beyond the initial decision phase and thus shaped the context in which the actual terms of the application were formulated.

V. Conclusions

The evidence of these successive periods is that group influence in foreign policy is in-

cepted the Rome Treaty and all "regulations, directives and . . . decisions taken under it"; Heath accepted the Treaty but proposed to negotiate over agreements reached among the Six since then. See Nora Beloff, "What Happened in Britain After the General Said No," in Uri, *From Commonwealth to Common Market*, pp. 81-83.

⁵⁷ The terms are those of Wolfram Hanrieder, "Compatibility and Consensus: A Proposal for the Conceptual Linkage of External and Internal Dimensions of Foreign Policy," *American Political Science Review*, 61 (December, 1967), 971-982.

versely proportional to effective politicization. The sectional pressure groups' domestic corporatist role, as expressed by the concurrent majority notion, appears to carry over into the realm of foreign policy making unless the issue becomes effectively politicized. The relationship between group influence and politicization, however, may be somewhat more complex than either the group politics or functionalist hypothesis implies. The evidence of the period following Macmillan's July, 1961 announcement that Britain would seek Common Market entry is that if political authorities do not consciously choose to treat an issue as broadly political, so that considerations of general national interest take precedence, then the sectional interests are likely to reassert their involvement in a situation where cost-benefit calculations become important criteria for judgment. The question of bipartisanship also assumes some importance in the limitation of group influence. As we have seen, Macmillan's hand was weakened by the absence of Labour Party backing in 1961-63. This lack forced him to seek additional support by making a succession of pledges, and it facilitated a measure of pressure-group access via the party government process (as opposed to the usual concentration of these groups on the administrative departments of government by means of functional representation). In contrast, Prime Minister Wilson's position was strengthened by opposition support for Common Market entry in 1966-67. Nonetheless, it would be wrong to conclude that simple bipartisanship, rather than the more complex phenomenon of politicization, best accounts for restricted pressure-group access. The simpler explanation proves inadequate when applied to the entire 1956-60 period, during which bipartisanship in a completely nonpoliticized setting coincided with an exceedingly high group access.

How then do the suggested group politics and functionalist hypotheses compare in terms of their prediction of the role of interest groups in the integrative process? The results indicate a confirmation of the group politics hypothesis—at least within the confines of the British case. During the FTA and EFTA phases, and again during the first set of negotiations for Common Market entry, pressure groups did influence policy, and they functioned to restrain Britain's movement toward participation in integrated European ventures. They exercised this role first by means of direct veto powers, then indirectly by operating to influence and qualify the terms of British association or membership.

Why were these groups generally opposed or at best guardedly favorable toward integration throughout a period when British membership of the EEC offered economic prospects ranging from breaking even⁵⁸ to clear and substantial advantages? Evidently, these group orientations reflected organizational rather than strictly economic considerations. While the CBI, NFU, and TUC are powerful and important bodies, as the peak organizations in their fields, they are amorphous and unwieldy. Group leaders lack the centralized authority which exists within an individual corporation, and policies tend to be developed on the basis of a lowest common denominator.⁵⁹ The groups' political muscle is consistently employed to protect sectors which could be sacrificed as uneconomic, even though the resultant balance of gain and loss might leave businessmen or farmers or trade unionists, on the whole, much better off.⁶⁰ An appropriate example is the operation of the National Farmers' Union during the 1961-63 negotiations for Common Market entry: British agriculture in general stood to gain from Common Market membership, but the NFU's considerable powers were employed to secure governmental pledges of safeguards for the horticultural sector, which by virtue of Britain's climate and geography was simply not in a position to compete successfully within an expanded EEC.

Another set of group considerations which is not exclusively economic involves the status of interest groups within the British political process. The groups explicitly claim a veto power within a setting of collectivist politics, and they are prone to resist any shift which, while promising material benefits, might threaten this status. Integration theorists have celebrated the phenomenon of Community-wide pressure groups emerging to press their common interest upon the EEC Commission at Brussels, as the Community organs have taken on decision-making functions once exclusively the province of national governments.⁶¹ This process is regarded as offering a new channel for sectional groups to influence policy outcomes, but even

in some future federal Europe with effective transnational interest groupings, a single national organization still would have considerably less influence than in a domestic context. It is thus readily understandable that during 1956-60 a group such as the TUC preferred that European cooperation proceed on the pattern of the intergovernmental OEEC rather than on the basis of supranational integration. During 1961-63, the NFU expressed a similar concern in its orientation toward the Common Market.

Finally, in assessing the reluctance of British interest groups to support European integration, one must consider the background variable of political culture. These groups have been characterized by a resistance to innovation, a distrust of complex written constitutional schemes, an insularity, and a sense of national self-reliance that in Britain—unlike the Continental countries—had not been discredited by occupation or defeat during World War II. In all these respects, the groups shared an orientation inherent in British life as a whole.

Taken together, their organizational nature, their position in corporatist politics, and perhaps the background effects of political culture, make interest groups highly complex and autonomous in their operations. To the extent that the functionalist view defines interest groups as representing more or less a mechanical vector sum of economic forces, it clearly needs modification.

As for the suggested hypothesis based on an extension of functionalism, the results of the British case require a rejection. Rather than facilitating integration, the weight of sectional pressure group influence, was almost always opposed or equivocal. Functionalist theory holds that interest groups will favor integration, particularly if economic benefits are involved, but in the British experience, so long as the country did not belong to a supranational grouping, this generally was not the case.

More broadly, functionalist theory, from its origins in Mitrany's writing, has viewed depoliticization along with a functional or technical treatment of issues, as the most likely route to integration. But to the extent the British operated this way in their relationship to Europe (in 1956-60, and again following the Macmillan decision in 1961-62), the approach simply did not work. Political judgments were indisputably the *sine qua non* for any significant British movement toward participation in European supranational integration. The fact that political authority, moved by considerations going well beyond material advantage or cost-

⁵⁸ Almost no serious assessment of the economic prospects of membership reached conclusions more pessimistic than these.

⁵⁹ This is not to imply that even fully centralized authorities can not make irrational or dysfunctional decisions.

⁶⁰ In this regard, they resembled American trade associations, which Bauer, Pool and Dexter found reluctant to take stands on issues on which unanimity was absent. (*American Business and Public Policy*, p. 337).

⁶¹ E. g., see Leon N. Lindberg, *The Political Dynamics of European Economic Integration* (Stanford: Stanford University Press, 1963).

benefit calculations, exercised the ultimate responsibility for such a step, implies a further difficulty for any extension of functionalist theory. For one thing, these results indicate that policy formation in an industrial democracy, even one that has corporatist politics, is not reducible to a mere interplay among organized interests. For another, it implies that movement toward such integration cannot be achieved simply by leaving matters on a technical level where interest groups are a leading policy influence. The problem is that the functionalist view may imply the operation of a benign invisible hand which will guide nations toward supranational integration in a nonideological, depoliticized, and overwhelmingly economic setting. But no more than in the realm of economics can the suitability of such a laissez faire process be taken for granted. In other words, what functionalism conceives of as a "painless transcendence of the nation-state"⁶² is unlikely to come about without conscious political intervention.⁶³

Moving beyond the confines of the British case, one can generalize about the importance of the distinctly political function.⁶⁴ At least in

⁶² Brenner, *Technocratic Politics and the Functional Theory of European Integration*, p. 1.

⁶³ There is also a normative implication here. It is that parties ought to play a greater role than pressure groups in the policy-making process. Unlike pressure groups, parties provide at least the opportunity for political leaders to make independent judgments on the basis of some broad interpretation of the national interest. But during the nonpoliticized phase of European policy making, those intimately involved in the process were nonelective, not responsible to the public, and concerned to maximize values less broad than those of the country as a whole. More generally, the functional representation process tends to manifest a static bias. It provides bargaining advantages for those interests which are better organized, while minimizing more diffuse considerations of public benefit. See Andrew Shonfield, *Modern Capitalism* (New York: Oxford University Press, 1965), p. 389. For other critical views, see Finer, *Anonymous Empire*, pp. 126-129; Sigmund Neumann, "Toward a Comparative Study of Political Parties," in *Modern Political Parties*, ed. Neumann (Chicago: University of Chicago Press, 1956), p. 397; and Bernard Crick, *Observer* (London), October 23, 1966. But for views which question the ability of parties to promote the broader interest, see J. R. Pennock, "'Responsible Government,' Separated Powers, and Special Interests: Agricultural Subsidies in Britain and America," *American Political Science Review*, 56 (September, 1962), 621 and 633; also Wilson, *Pressure Group*, pp. 210-211.

⁶⁴ Stanley Hoffmann's argument also seems applica-

ble here. He has indicated the importance of an irreducible political core of "high politics," which is not susceptible to gradual erosion through step-by-step functional or spillover processes. See "European Process at Atlantic Crosspurposes," p. 92.

This perspective and that of the functionalists need not conflict totally, in that (as noted earlier in this article), the functionalists have been primarily concerned with integration within existing communities, rather than with the geographic expansion of integration or the construction of new communities. Indeed, when Britain does become a member of the EEC, circumstances will then exist in which the functionalist hypothesis is likely to apply. Major portions of functionalist theory do in fact remain valid and theoretically fruitful, but it is clear from testing the functionalist hypothesis against the British case, as well as in considering the criticisms raised by other recent writings,⁶⁵ that the functionalist theory of supranational integration would seriously overreach itself if applied to this major kind of problem. Thus at least in the case of Britain's approach to Europe, another theoretical perspective, specifically that of group politics, seems to be in order.

ble here. He has indicated the importance of an irreducible political core of "high politics," which is not susceptible to gradual erosion through step-by-step functional or spillover processes. See "European Process at Atlantic Crosspurposes," p. 92.

⁶⁵ Lindberg and Scheingold find, directly contrary to Haas in *The Uniting of Europe*, that the successful transformation leading to the establishment of the Common Market itself was not due to functional spillover: "The transformation of the European Community cannot be adequately described within the context of the standard neofunctional model with its heavy emphasis on supranational institutions and functional linkages." *Europe's Would-Be Polity*, p. 243.

⁶⁶ E. G., Lindberg and Scheingold; Brenner, *Technocratic Politics*; and Roger D. Hansen, "Regional Integration: Reflections on a Decade of Theoretical Efforts," *World Politics*, 21 (January, 1969), 242-271.

Political Clientelism and Ethnicity in Tropical Africa:*

Competing Solidarities in Nation-Building

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The analysis presented here is an attempt to follow up, qualify, and elaborate upon, some earlier hypotheses about the role of political clientelism in the light of empirical data from West and Central Africa.¹ More specifically, the aim is, first, to use the concept of clientelism as an auxiliary tool to elucidate some of the ambiguities surrounding the relationship between ethnicity and nation-building in contemporary Africa; and, second, to provide what appears to be a "missing link" between the micro- and macrosociological or system-centered theories that have dominated the field of developmental politics since the last decade. Succinctly stated, this "missing link" is to be found in the varieties of personal and institutional linkages and reciprocities which, depending on the circumstances, have bridged or accentuated ethnic and other discontinuities within traditional and modernizing pluralistic polities.

Because it cuts across both "traditional" and "modern" referents, political clientelism, as a concept, has a heuristic value generally missing from the conceptual arsenal of either "modern" or "traditional" polities; it directs attention to processes of adjustment between traditional and modern patterns of behavior, expectations, and normative orientations to politics which might otherwise go unnoticed. As an analytical concept, clientelism brings into focus units of analysis, political actors, and relationships which have thus far received scant attention from political scientists.² As a developmental construct,

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¹ See René Lemarchand & Keith Legg, "Political Clientelism and Development: A Preliminary Analysis," *Comparative Politics*, Vol. 4, No. 2 (Jan. 1972), 149-178.

² A notable exception is William J. Foltz, "Social Structure and Political Behavior of Senegalese Elites," *Yale Papers in Political Science* No. 33; see also C. S. Whitaker's illuminating discussion of "clientage" phenomena in Northern Nigeria in *The Politics of Tradition* (Princeton: Princeton University Press, 1970), pp. 373-375. Nor is this neglect typical of political

it suggests new perspectives from which to look at processes of nation-building in Africa: Viewed from the micropolitical perspective of traditional patterns of interaction among groups and individuals, nation-building becomes not so much an architectonic, voluntaristic model divorced from the environmental materials available; it becomes, rather, a matter of how best to extend to the national level the discrete vertical solidarities in existence at the local or regional levels. Seen through the prism of traditional forms of clientelism, the processes of integration and disintegration that have attended the growth and decline of historic African polities may give us additional clues to similar processes in contemporary Africa. Finally, looked at from the standpoint of the exchange processes involved in clientelism, patterns of resource allocation and distribution may yet emerge as among the most meaningful indicators of political development; at least they may provide a useful corrective to the current bias toward power accumulation as a presumed prerequisite of political modernization.

This said, two cautionary remarks are in order: One is that the empirical data available are still far too limited and unreliable to permit anything like a full-fledged theory of political development; although an attempt has been made in the latter part of this discussion to relate clientelism to current theories of development, the accent here is primarily on "national integration." What follows, then, is better seen as a series of tentative and piecemeal hypotheses awaiting further testing and systematization. Moreover, if many of the observable patterns of behavior in contemporary Africa are indeed reminiscent or expressive of traditional client-patron relationships, it does not follow that *all* such patterns are reducible to clientelism.³ This kind of naive reductionism

scientists alone, as can readily be inferred from a content analysis of a recent issue of the *Canadian Journal of African Studies* 1 (Winter, 1969) devoted to "Rural Africa." Clientelism is conspicuously absent from the list of research priorities assigned to various cross-disciplinary subfields (Political Anthropology, Local Politics and Development Administration, Agricultural Economics, Rural Sociology and Communications, and Rural Geography).

³ Even where the evidence suggests the presence of clientelistic networks, these may in fact be so fluid and

would make unduly short shrift of the emergent integrative mechanisms, norms, and symbols attendant upon the rise of nationalist sentiment and activity, and hence ascribe to clientelism more credit than it actually deserves.

Clientelism, Ethnicity, and Integration

Although it would be too much to expect universal agreement on any single definition, for the purpose of this discussion political clientelism can be viewed as "a more or less personalized relationship between actors (i.e., patrons and clients), or sets of actors, commanding unequal wealth, status or influence, based on conditional loyalties and involving mutually beneficial transactions."⁴ However vague, this definition has a dual advantage. First, it is broad enough to encompass synchronic and diachronic variations (i.e., the various kinds of microlevel solidarities associated with the operation of traditional forms of clientelism, as well as the broader, overarching solidarities characteristic of the machine pattern). Second, it suggests alternative means for integration where coercive power is not sufficiently coercive to command widespread compliance and where conceptions of legitimacy are as yet too weak or circumscribed to produce consensus.

This type of situation is clearly the rule rather than the exception in most of Africa. Equally plain is that, whatever virtues clientelism may have in bringing people together, a more prevalent type of integration is that which has taken place at the local or regional level through horizontal solidarities associated with kinship or ethnic ties, or the extension of such solidarities on a more inclusive scale—in short, through ethnicity.⁵ It is less obvious, however,

that clientelism and ethnicity have seldom operated independently of each other, particularly in recent times. Just as ethnicity has sometimes been credited with integrative properties that really belong to the realm of clientelism, so clientelism as an integrative mechanism has often developed out of the exigencies of ethnic fragmentation.

Aside from the conceptual vagueness attached to each term, analysis of the relationship between clientelism and ethnicity is rendered peculiarly arduous by the similarity of the processes to which they refer. For if ethnicity is a matter of self-definition, and if "its ultimate referent is necessarily subjective,"⁶ the same can be said of patron-client relationships. Thus what from one standpoint might be regarded as an example of ethnicity in the sense in which Wallerstein uses the term—a redefinition of one's cultural identity through membership in a wider community, usually in the context of an urban situation⁷—might, from another angle, be viewed as a redefinition of rural clientelistic ties in an urban setting. Moreover, clientelism and ethnicity may have overlapping memberships, with some individuals solidly anchored in the ethnic substructure, and others acting as intermediary links between this substructure and the higher reaches of the clientelistic pyramid. What may be taken for a clear example of ethnicity at one level may be nothing more than the lower reticulations of a more extensive clientelistic network. A third source of confusion lies in the reciprocal processes of interaction which normally enter into the ethnicity-clientelism relationship. Rarely has this relationship been unidirectional or fixed. Just as

unstable as to defy analysis. Especially pertinent in this connection is Professor Young's query (in a personal communication): "Clientelism (in the Congo) is certainly crucial, but how can one deal with it when all is fluidity, when the networks are far more ephemeral, when mutual costs and benefits of maintenance of particular patron-client sets are apparently recalculated very frequently, and on the basis of very short-run contingencies?" In such a spasmodic environment the analysis of clientelism is evidently a far more risky endeavor than in settings where client-patron relationships are more clearly institutionalized and offer a basis for relatively stable interactions among actors. If for no other reason, our analysis is restricted to the latter type of situation, as illustrated by Senegal, Mali, the Ivory Coast, Northern Nigeria, Rwanda, Burundi and Liberia. Among other states that might fit into this category but which are not specifically dealt with in this paper one might mention the Sudan, Chad, Upper Volta, Niger, Dahomey, Sierra Leone and Mauritania.

⁴ Lemarchand & Legg.

⁵ The case for ethnicity as a source of national integration is stated in Immanuel Wallerstein, "Ethnicity

and National Integration in West Africa," *Cahiers d'Études Africaines*, 1 (October, 1960), 129-139; using "tribalism" in a very different sense from that which Wallerstein ascribes to the term, Richard Sklar makes a similar case in "The Contribution of Tribalism to Nationalism in Western Nigeria," *Journal of Human Relations*, 8 (Spring-Summer, 1960), 407-415. Unless otherwise specified, our use of the term ethnicity incorporates Wallerstein's notions of "tribe" (a reference group defined by its loyalty to a tribal government, in a rural setting) and "ethnicity" (denoting loyalty to a community detached from its traditional government, in an urban setting). Our assumption here is that the boundaries of ethnicity are inevitably self-ascriptive, regardless of the setting—a point persuasively argued by Paul Mercier, ("On the Meaning of Tribalism in Black Africa") in P. Van der Berghe, ed., *Africa: Social Problems of Change and Conflict* (San Francisco: Chandler Publishing Co., 1965), pp. 483-501.

⁶ See Aristide Zolberg, "Ethnicity and National Integration," a paper prepared for delivery at the 1966 Annual Meeting of the African Studies Association, Bloomington, Indiana, p. 2.

⁷ Wallerstein.

clientelism may in some cases weaken the strength of ethnic identification, the latter may in turn affect the shape and stability of clientelistic networks.

If their contours are often blurred by the complexity of their interrelationships, clientelism and ethnicity are nonetheless analytically separable—and so are their relative contributions to national integration. The first thing to note in this connection is that although clientelism and ethnicity are both relational concepts, involving a subjective definition of the position of an individual or group in relation to another, clientelism incorporates a potentially wider range of social referents and thus offers *wider scope for integration*. Clientelism in M. G. Smith's terms, "incorporates such differentiating factors as ethnicity, occupational status, lineage, and rural-urban distinctions, and defines the boundaries of the political society . . . in functional terms"⁸ or, one might add, if not always in the functional terms associated with traditional forms of clientelism, at least in terms of mutual interest. In the case of ethnicity, perceptions of mutual interest are dependent upon, and limited by, perceptions of cultural affinities; clientelism, on the other hand, extends these perceptions beyond the realm of primordial loyalties and establishes vertical links of reciprocity between ethnically or socially discrete entities.

This is not to argue that perceptions of mutual interest will necessarily prevail over ethnic loyalties and cause their extinction—though this possibility is by no means excluded. The mutations of ethnic loyalties entailed by the play of clientelism may range from extinction to dilution and accommodation, depending on the nature of the reciprocities involved and the level at which they operate.

Three possible types of situations suggest themselves: First, there may be a substitution of the client's original ethnic identity for that of his patron: A case in point is Gallais's description of the Bobo tribesman who, upon settling in Mopti (Mali), looks for a protector and having found one, becomes his client and through a kind of mimetic process, acquires not only the external behavioral patterns of his patron but his ethnic identity.⁹

Second, clientelism may cause a redefinition of one's original tribal identity by reference to a wider cultural focus. Though often conceptu-

alized in terms of "super-tribalization"¹⁰ or "ethnicity," much of this phenomenon is reducible to a quest for an alternative focus of security, in the form of clientelism, to that provided through clan or lineage affiliations. Thus, speaking of "associations founded by the urban tribal population" of Monrovia, Frankel notes that these associations "gain respectability from (the widespread custom of patronage), and the patrons, for their part, expect to be able to count on the support of their members for personal and political ends."¹¹

Third, clientelism may provide the cement by which ethnic identities are amalgamated within the boundaries of a more inclusive political system. This phenomenon calls to mind two different types of situations: (1) those in which clientelism acts as the major "social adhesive"¹² of the traditional political system, in effect extending to the traditional political arena the principle of reciprocity operative at the level of interpersonal relations (as in the emirate system of Northern Nigeria), and (2) those in which clientelism operates within the context of a nationwide political party and takes on the qualities of machine politics (as in the case of the *Parti Démocratique de la Côte d'Ivoire* [PDCI] in the Ivory Coast). However different the processes and reciprocities involved, in each case the result has been to accommodate ethnic discontinuities to the requirements of a more or less integrated political community.

As the foregoing suggests, although clientelism and ethnicity—like integration—are relative concepts, implying varying degrees of cohesiveness and identification, clientelism denotes a *greater variety of possible types and levels of integration*.

In the perspective of the modern African state system, such integrative potentialities as

⁸ The term "super-tribalization," suggested by Jean Rouch in "Migrations au Ghana," *Journal de la Société des Africanistes* 26, Nos. 1-2 (1956), 163-164, has more restrictive implications than Wallerstein's use of the concept of ethnicity as it suggests common cultural affinities between the group from which an individual is detached and that in which he is incorporated. For Wallerstein, ethnicity does not presuppose preexisting cultural bonds. Regardless of this distinction, the process to which these terms refer occurs on a far wider scale than that involved in ethnic mutation through the identification of a client with his patron; one might say that the latter is an individual mutation and the former a collective one usually taking place within the context of ethnic associations.

¹¹ Merran Fraenkel, *Tribe and Class in Monrovia* (London: Oxford University Press, 1964), p. 187.

¹² The expression is borrowed from C. S. Whitaker, *Politics of Tradition*, p. 373.

⁸ Michael G. Smith, *Government in Zazzau* (London: Oxford University Press, 1960), p. 260.

⁹ Jean Gallais, "Signification du Groupe Ethnique au Mali," *L'Homme* 2 (May-August 1962), p. 122.

may exist in clientelism operate essentially at two levels: Clientelism can be evaluated for its contribution to processes of unification among all or some of the parts of the ethnic subsystem, or to the adaptation of these parts to the central governmental and bureaucratic structures. In either case integration stems from the exchange of valued and presumably scarce resources, which in turn permits the attainment of individual or collective goals. Yet the kinds of resources, exchanges, and normative expectations involved are liable to differ significantly from one level to another, and so also will the implications for national integration.

By way of illustration one might compare the operation of traditional clientelism in Northern Nigeria with the machine form operative in Senegal. In his discussion of *bara*-ship in Nupe, Nadel calls attention to three different types of integrative responses elicited through the inducements of clientelism:¹³ first, the type of personal identification that originates from the desire of an inferior to obtain the political protection of a superior; second, that which results from the efforts of "impecunious craftsmen to obtain a prosperous customer"; third, that which, "over and above securing material assistance, stems from attachment to a man of rank and influence," thereby offering the client a chance "of becoming somebody," "of being lifted from the ignominy of a commoner's life into the sphere of rank and importance." Whether the motive happens to be a quest for protection, for material assistance, or for social mobility, integration at this level stems from an exchange of psychological and material rewards between a *superior and a subordinate*; extended to the emirate system the relationship remains basically the same: "The crux of the clientage relationship in all the emirates," writes C. S. Whitaker, "is that patronage, economic security, and protection can be exchanged for personal loyalty and obedience."¹⁴

Rather different is the type of clientelism through which the interests of maraboutic elites of Senegal are aggregated within the *Union Progressiste Sénégalaise* (UPS). What W. Foltz describes as "a pyramiding of clan alliances in super patron-client relationships" involves, in addition to personal bonds of friendships between the national elites and the maraboutic notables, certain calculated reciprocities, in the form of policy outputs and votes, between the party leadership and the leaders of the Muslim

brotherhoods. "The prerequisite of political action," according to Foltz, is "not that it be progressive, nationalist, or such, but that the spoils be fairly distributed. The parallels with American ethnic and urban politics are fairly close; as in American cities, long range planning is often sacrificed to short-run expediency."¹⁵ Clientelism, in short, becomes an adaptive device through which the demands of the maraboutic patrons are channeled into the political system and converted into policy outputs in return for electoral support.

In one case, integration depends on reciprocities between superiors and subordinates: its scope is limited to and at the same time delineates boundaries of the subsystem; the payments include predominantly affective rewards, in the form of identifying with a superior or receiving deference from an inferior. In the other case, integration hinges on reciprocities between equals; its scope transcends the boundaries of the ethnic or cultural subsystem; and the transactions are invested with a high degree of instrumental rationality.

If the first of our examples retrospectively suggests the nature of the obstacles placed in the way of national integration through the operation of microlevel clientelistic solidarities, the second indicates a far greater area of compatibility among the different levels at which clientelism operates. In the case of Senegal, the contribution of clientelism to nation-building would seem to involve several different types of integrative mechanisms: First, insofar as it implies a normative commitment to certain "instrumentalities and procedures for the achievement of goals and resolving conflicts,"¹⁶ clientelism may provide at least some of the ingredients which enter into processes of consensus formation at the national level. Second, to the extent that it creates functional reciprocities among otherwise unrelated ethnic groupings, it may also generate feelings of interdependence among them and help reduce the saliency of primordial loyalties. Third, through the vertical bonds it creates among hierarchically distinct groups and communities, clientelism may sup-

¹³ Foltz, "Social Structure and Political Behavior . . .," p. 153.

¹⁴ Myron Weiner, "Political Integration and Political Development," *Annals of the American Academy of Political and Social Science*, 358 (March, 1965), 53. The distinctions drawn in this paragraph lean heavily on Weiner's classification. For a somewhat different approach, see John N. Paden, "Conceptual Dimensions of National Integration Theory, with Special Reference to Inter-Ethnic (Horizontal) Integration," Internal Working Paper No. 2, Instability/Integration Project, Council for Intersocietal Studies, Northwestern University (January, 1968).

¹² Siegfried F. Nadel, *A Black Byzantium* (London: Oxford University Press, 1951), p. 123.

¹⁴ Whitaker, p. 374.

ply the critical links between rulers and ruled, and through the intervention of "brokers" help bridge the proverbial gap between the elites and the masses. These three types of integration can be referred to, respectively, as "subjective," "transactional," and "structural."

To emphasize the integrative potential of clientelism is not to deny the divisive tendencies inherent in the formation of factions and personal followings. Nadel's comments on the effects of factionalism in Nupe might apply to other contexts as well: "Rivalry and feuds between the leaders of local factions may throw the whole town, indeed the whole country, into a civil war. . . ."¹⁷ In specific situations, however, the mere existence of factionalism may reinforce rather than diminish popular allegiances to the system, and hence positively aid subjective integration at the national level. This is the core of the argument advanced by Gluckman in his discussion of "rebellion" (as distinct from "revolution") in a traditional monarchical context:

All sections struggle for the kingship and this unifies them. They seek to place their own prince on the throne; they do not try to become independent from the kingship. . . . It is a historical fact that these struggles kept component sections of the nation united in [a] conflicting allegiance about the sacred kingship.¹⁸

Gluckman's hypothesis cannot be accepted unreservedly, if only because much of the outcome would seem to depend on the character and composition of the factions involved, and the environmental conditions under which competition takes place. Nevertheless, it does not suggest that competition among patrons could encourage the cohesiveness of different ethnic segments in the lower reaches of the clientelistic pyramid.

Finally, although clientelism and ethnicity both provide opportunities for manipulation and adaptation, clientelism offers a significantly broader range of strategies for the achievement of national integration. For purposes of analysis these strategies may be conceptualized in terms of (1) a pyramiding of patron-client ties, (2) a spillover of reciprocities, and (3) changing partnerships, each involving specific changes in the character and structure of clientelistic relationships. What these changes consist of, and how they may affect national integration, will be discussed at greater length in a subsequent section. Suffice it to note here that

although these processes are induced and not spontaneously occurring, their viability depends in part on the environmental transformations arising from colonial rule and on the limitations imposed by the character of the ethnic and clientelistic subsystem.

Since political integration is often a function of the limitations and opportunities inherent in traditional clientelistic networks, it will be useful at this stage to take a brief look at some of the varieties of traditional dependency relationships encountered in Tropical Africa.

Varieties of Traditional Dependency Relationships

For most anthropologists, clientelism—or "clientage"—refers to a specific type of interpersonal relationship, more often than not institutionalized in the form of a contractual agreement between a superior and an inferior.¹⁹ Examples are the *bara*-ship of Nupe, the *buhake* in Rwanda, the *bugabire* in Burundi, the *okutoisha* in Ankole. Our concern here, however, is with the generic and the recurrent rather than the idiosyncratic or the unique. Taking as our criteria of differentiation (1) the occupancy of role sets, (2) the normative orientations of patrons and clients, (3) the types of transactions involved, and (4) the base values of differential control over resources, we find that at least four different types of political clientelism may be said to have guided social and political relationships in traditional Africa—the patrimonial, the feudal, the mercantile, and the "saintly" type (see typological chart).

Patrimonial clientelism is explicitly political in character. Historically associated with the emergence of archaic state systems in the Sudanic and Interlacustrine areas, it suggests a significant expansion in scale of the original community, along with an increasing differentiation of governmental structures. In Weber's terminology, it implies "[the transformation of the original community] into a stratum of aids to the ruler [and its dependence upon him] for maintenance through the usufruct of land, office fees, income in kind, salaries and hence through prebends."²⁰ A basic feature of this transformation is the recruitment of client-chiefs who stand in relation to the sovereign not unlike bureaucrats in relation to their superior, in the sense that "they [do] not claim their

¹⁷ Nadel, *Black Byzantium*, p. 126.

¹⁸ Max Gluckman. *Custom and Conflict in Africa* (Oxford: Basil Blackwell, 1963), pp. 45–47.

¹⁹ For a further elaboration of this definition, see Georges Balandier, "Les Relations de Dépendance Personnelle: Présentation du Thème," in *Cahiers d'Études Africaines*, 9, 35 (1969), 345–349.

²⁰ Cited in Hans H. Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1958), p. 297.

Typology of Dependency Relationships in Traditional Africa

Type of Clientelism	Occupancy of Role Sets	Normative Orientation	Type of Transaction	Base Values of Differential Control Over Resources
1. <i>Patrimonial</i> Example: Sudanic States; Interlacustrine States (Rwanda, Burundi, Ankole, Buganda)	King-chief; chief-subchief	Mutual trust and loyalty	Office (political or administrative)—service and support	Power
2. <i>Feudal</i> Example: Buhake (Rwanda) Bugabire (Burundi) Okutoisha (Ankole) Bara-ship (Nupe)	Notable or freeborn noble-serf or commoner	Deference-Affection	Protection—service and prestige	Status
3. <i>Mercantile</i> Example: Diula-Diatigi; diulaba-diulade; dillali-mai gida	Trader (or middleman)-associate or customer	Economic self-interest and mutual trust	Commercial exchange	Wealth
4. <i>"Saintly"</i> Example: Sheikh-talibé Murabit-tlamith	Religious leader (marabout)-follower (talibe)	Benevolence-Devotion	Salvation—Obedience and Service	Sanctity

NOTE: The categories included in the above typology are adapted from the process-model developed in Lemarchand and Legg, "Political Clientelism and Development: A Preliminary Analysis," *Comparative Politics*, Vol. 4 No. 2 (January, 1972); although the terms "patrimonial" and "feudal" are used in this article to denote a possible evolutionary sequence from one type of clientelism to another, in the present context they are merely intended to provide an analytic framework for distinguishing among types. This, however, is not meant as a counterargument to the evolutionary scheme formulated by Maquet in "Institutionalisation féodale des relations de dépendance: dans quatre cultures interlacustres," *Cahiers d'Études Africaines* (1969), 402-414, in which the author suggests the possibility of an historical movement from "patrimonial" to "feudal" clientelism.

position by right of inheritance or by virtue of any prior connexion with the area to which they were appointed."²¹ The underlying values, however, are those of mutual trust and loyalty. The basic transaction consists in the doling out of offices in return for administrative and political benefits. The base value through which initial control over scarce resources is obtained is political power defined in terms of the ruler's capacity to make effective use of a coercive apparatus, but as status differences emerge in consequence of differential success in military skills, exchange relations become increasingly differentiated from military ones, at which point, in Weber's terms, "princely prerogatives become patrimonial in character."

Unlike the foregoing, *feudal clientelism*, in the sense in which the term is here used, is not necessarily coextensive with the formal political hierarchy; it refers to the anthropologist's more circumscribed definition of "clientage"—i.e., to

an interpersonal bond between a superior and a subordinate, as between a freeborn noble and a commoner, involving "an exchange of protection, economic security and a position in the society in return for loyalty, obedience and service from the subordinate."²² Often institutionalized in the form of a ceremonial gift by the patron (as in traditional Nupe, where "the offering of a turban and a sword by the patron to his henchman sealed the pact"),²³ and involving contractual elements, it partakes of the qualities of the feudal homage. Yet, as a departure from what some might consider the hall-

²¹ Lucy Mair, *Primitive Government* (Baltimore: Penguin Books, 1962), p. 153.

²² Ronald Cohen, "The Dynamics of Feudalism in Bornu," *Boston University Papers on Africa*, Vol. 11 (Boston: Boston University Press, 1966), p. 91. For further characterizations of "feudal" clientelism, see Lucy Mair, "Clientship in East Africa," *Cahiers d'Études Africaines*, 2, 6 (1961), 315-326; Edward Steinart, "Vassal and Fief in Three Interlacustrine Kingdoms," *Cahier d'Études Africaines*, 7, 25 (1967), 606-624; Jacques J. Maquet, *The Premise of Inequality in Ruanda* (London: Oxford University Press, 1961).

²³ S. F. Nadel, *Black Byzantium*, p. 126.

mark of feudalism,²⁴ the base value through which a patron lays claim to the allegiance of his client lies in the differential social prestige and influence which goes along with one's membership in a "dominant" ethnic or status group.

Mercantile clientelism, traceable to the inception of the long-distance trans-Saharan trade, in the thirteenth or fourteenth centuries, has persisted in one form or another throughout and indeed long after the Malinke diaspora. Although in this case the participants are essentially concerned with the buying, storing, transport, and selling of marketable goods, out of these commercial transactions have developed more or less personalized relationships not only between buyers and sellers but *within the trading community*. The reciprocities which exist between the *diula* traders of Mali, Guinea, or Senegal and their hosts, the *diatigi*, are duplicated by similar relationships between the long-distance Hausa traders from up-country and their "landlords" in places like Kumasi, Freetown and Ibadan, and, in Ibadan between landlords (*mai gida*) and middlemen (*dillali*).²⁵ These reciprocities involve, besides material benefits, a sense of mutual trust between the parties. A landlord or a *diatigi* is one who not only has material resources at his disposal but

who also enjoys a certain "credit rating" from his reputation of trustworthiness and reliability. Although the anthropological literature dealing with this type of clientelism is scarce and the political science literature virtually nonexistent, the basic principle of social interaction in this case is unquestionably that of patron-client relationships defined in terms of reciprocal economic benefits and mutual loyalties.

The fourth type, here labeled somewhat uncritically "*saintly*" *clientelism*, expresses itself most conspicuously within the context of the Sufi brotherhoods (*tariqas*) of Senegal but is also found in Mauretania, Mali, Niger, and Northern Nigeria.²⁶ Sometimes referred to as "maraboutism," this type of relationship emphasizes the blind submission of the *talibé* to his *sheikh* or *muqaddam*. At the root of this dependence lies the disciple's conviction that his salvation depends on the intercession of his marabout, and the latter's realization that his status in society depends on the personal devotion of his followers, as well as their contributions in kind or cash. The *sheikh-talibé* relationship among the Mourides of Senegal represent the most striking example of this type of reciprocity; but one might also cite as an example the nexus of mutual obligations which to this day links the maraboutic elites of Mauretania and their followers. Whether it expresses itself in a secular or religious context, this type of clientelism still provides the basic articulation of social and political processes among individuals and groups in the predominantly Islamicized areas of West Africa.

Like all classifications, the foregoing is unabashedly arbitrary. None of the above categories can be treated in isolation. As the case of prerevolutionary Rwanda suggests, the patrimonial and feudal types may coexist with and indeed reinforce each other, as seems to have happened in the case of the mercantile and patrimonial variants at particular junctures of the evolution of the Sudanic states.²⁷

²⁴ For Fallers and Lombard, a feudal relationship can only obtain among equals (i.e., among nobles); a clientele relationship, on the other hand, establishes reciprocities between nobles and commoners. See Lloyd A. Fallers, *Bantu Bureaucracy* (London: Oxford University Press, 1956), and Jacques Lombard, "La Vie Politique dans une ancienne société de type féodal: Les Bariba du Dahomey," *Cahiers d'Études Africaines*, 1,3 (1960), 5-45.

²⁵ On the sociopolitical role of the *diula* in contemporary Bamako, see Claude Meillassoux, *Urbanization of an African Community* (Seattle and London: University of Washington Press, 1968), and his article on "The Social Structure of Modern Bamako," in *Africa*, 35, 2 (1965), 125-142. Cf. Majhemout Diop, *Histoire des classes sociales dans l'Afrique de l'Ouest* (Paris: 1971), pp. 148-152. For an excellent account of the *mai gida-dillali* relationship in contemporary West Africa, see Polly Hill, "Landlords and Brokers: A West African Trading System," *Cahiers d'Études Africaines*, 6, 23 (1966), 349-367; and her "Markets in Africa," *The Journal of Modern African Studies*, 1, 4 (1963), 441-455. See also, V. R. Dorjahn and C. Fyfe, "Landlord and Stranger: Change in Tenancy Relations in Sierra Leone," *Journal of African History*, 3, 3 (1962), 391-399. Perhaps the most illuminating discussion of this type of clientelism, from the standpoint of the political scientist, is Abner Cohen, "The Social Organization of Credit in a West African Cattle Market," *Africa*, 35, 1 (1965), 8-20. For an interesting discussion of the role of the *diatigi* in traditional African society, see in particular the contributions of J. L. Boutiller and J. L. Amselle in C. Meillassoux, ed., *The Development of Indigenous Trade and Markets in West Africa* (Oxford: 1971), pp. 240-252, 253-265.

²⁶ For a detailed analysis of the sociopolitical role of the Mouride brotherhoods of Senegal, see Donal B. Cruise O'Brien, *The Murids of Senegal: The Socio-Economic Structure of an Islamic Order* (Ph.D. Thesis, University of London, 1969). See also Lucy Behrman, "The Political Influence of Muslim Brotherhoods in Senegal" (unpublished ms.), and "The Political Significance of the Wolof Adherence to Muslim Brotherhoods in the 19th Century," in *African Historical Studies* 1 (1968), pp. 60-78. For a broader perspective, see J. Spencer Trimingham, *The Influence of Islam upon Africa* (New York: Praeger, 1968).

²⁷ Under what conditions a particular form of clientelism brings another into being is a question that lies beyond the scope of this paper. What seems clear, how-

Moreover, although client-patron relations are here conceptualized in terms of dyadic relationships for purposes of clarity, this is not meant to exclude the possibility of middlemen or brokers. In prerevolutionary Rwanda, the chief was not only a client of the sovereign but also a broker between the sovereign and the subchief; in Senegal the *sheikh-talibé* relationship was and still is mediated by regional officers (*fode*) and assistants (*tamsirs*) who might also qualify as middlemen; in Mali the *diatigi* were almost by definition ("the stranger's master") cast in the role of brokers between the *diulas* and the host community, a function they still perform in many of the trading centers of West Africa.²⁸ Client-patron ties, in short, are not necessarily dyadic and unidirectional, but may involve networks of reciprocities which in turn produce reversible relationships among the parties. Depending upon their position in the society, one man's patron may act as another

man's client. Finally, because of its somewhat static qualities, the above typology underplays possibilities of conflicting clientelistic solidarities, as well as the potential for conflict that may arise from the juxtaposition of vertical and horizontal solidarities. While suggesting too much in the way of an integrative potential, it may also suggest the wrong kind of potential for integration.²⁹

Aside from illustrating the variety of traditional dependency relationships that might conceivably be subsumed under the rubric of political clientelism, this classification sketches out how one type of relationship can be distinguished from another, and how their projections into the political space of contemporary Africa can be analyzed. Presumably, before one can infer a set of causal relationships between traditional and modern forms of clientelism, one must ask certain preliminary questions about the variables determining each type. Who are the patrons, and who are the clients? How compatible are their status position, normative orientations, and transactional roles with conditions of rapid social change? In what ways do various types of differential control over resources "fit" with the conditions of the colonial and postcolonial situations? The answers to these questions are likely to vary not only with the heritage of specific colonial policies but with the characteristics of each type of clientelism.

Along with these differences, however, we find certain basic uniformities about how, in Africa as elsewhere, power is organized and generated. Power is not so much a commodity

ever, is that there is no logical relationship between forms of government and types of clientelism (except where governmental and clientelistic structure merely duplicate each other, as in the case of patrimonialism). If there is any common denominator to these forms of clientelism, it is to be found, in Ronald Cohen's words, "in conditions of personal insecurity, a lack of widespread use of payment for specific purposes, lack of adequate social control outside such (clientelistic) relationships, and the inability of the kinship units to perform all the required and desired services for individuals." See Cohen, "The Dynamics of Feudalism in Bornu," p. 91. Given our ignorance, the most that can be said at this point is that the emergence of mercantile clientelism, as a historical phenomenon, has been dependent upon, as well as limited by, the growth of centralized state structures; conversely, the expansion of state structures has itself been conditioned by the economic and political resources made available through the development of regional trade circuits. For further evidence on this point, see J. Vansina, "Long-distance Trade Routes in Central Africa," *Journal of African History*, 3, 3 (1962), 375-388.

²⁸ The brokerage function traditionally performed by the *diatigi* is described by Charles Monteil in the following terms: "Dans les centres soudanais les étrangers sont parqués dans un quartier spécial. Les simples passagers doivent se présenter au chef de la localité et ce chef leur assigne un hôte ou diatigi ('le maître de l'étranger'). Cet hôte est, à proprement parler, le répondant de l'étranger et celui-ci ne doit agir que par son intermédiaire. . . . Souvent par leur nombre et leur richesse, non moins que par l'importance du personnel qu'ils emploient, les étrangers sont assez puissants pour jouir d'une indépendance relative, moyennant paiement de redevances. Voire même ils arrivent à constituer un état dans l'état, et dominer les pouvoirs locaux." Charles Monteil, "Les Empires du Mali," *Bulletin du Comité d'Etudes Historiques et Scientifiques de l'AOF*, 12, 3-4 (1929), 326. See also Félix Dubois, *Tombouctou la mystérieuse* (Paris, 1897), p. 295.

²⁹ A further qualifier is that the above typology only deals with highly institutionalized forms of clientelism and thus excludes societies in which clientelism, though never firmly institutionalized, did in fact play a major role in the definition of traditional power relationships. Traditional Ibo society is a case in point:

"As in the urban communities of Boston, New Haven, and New York, political survival in an Ibo community hinged fundamentally on the leader's ability to mould and maintain a personal following. . . . In the Ibo context, as in the American, the politician acted as a broker of conflicting interests and personalities and as a provider of personal favors. His political success, in short, was a function both of his ability to resolve disputes and his generosity." (Howard E. Wolpe, "Port Harcourt: Ibo Politics in Microcosm," *The Journal of Modern African Studies*, 7 [1969], 475.)

Such noninstitutionalized forms of clientelism are obviously relevant to our discussion; their omission from our typology is only justified by the lack of relevant anthropological and historical data and the consequent difficulties involved in the task of classification and comparison.

that some just happen to possess by virtue of their privileged membership in any particular group as a relational phenomenon. Inherent in each of the clientelistic patterns noted above is a relationship of reciprocity between an individual (or group of individuals) whose influence stems from his ability to provide services, goods or values that are so desired by others as to induce them to reciprocate these gratifications in the form of alternative services, goods, and values.³⁰ And since the norm of reciprocity is by nature malleable and indeterminate in terms of the obligations it implies—being, in Gouldner's terms "like a kind of plastic filler, capable of being poured into the shifting crevices of social structures, serving as a kind of all-purpose moral cement"³¹—one can see why, as a role model, clientelism should have persisted over time, through adjustment and adaptation. Although social mobilization may lead to different partnerships, the role model may remain basically unaffected. The evidence on this score is indeed abundant. Far from evident, however, are the types of adjustment that have taken place over the years and the conditions that have caused one type of adjustment to occur rather than another.

Irrespective of the types of clientelism operative in the traditional society, three distinctive patterns of adjustment can be discerned: First and most prevalent has been the *spillover of clientelistic reciprocities* from one field of activity to another. Reciprocities that were once restricted to a specific type of exchange have thus led to cumulative or alternative exchanges among parties. A case in point in the transformation of "feudal" into "saintly" patron-client ties within the framework of the Muslim brotherhoods of Senegal, and the projection of both into the context of "clan politics."³² Similarly, the patrimonial ties that once existed among the ruling aristocracy of Rwanda provided the foundation for the establishment of patron-client ties of a feudal variety between Hutu and Tutsi and for the extension of dependency relationships based on the appropriation of land to a

dependency relationship based on the ownership of cattle.³³ A more recent example is the "spillover" of mercantile clientelism into the realm of local politics in Ibadan: "Of all the patrons," writes Abner Cohen, "the cattle landlords are the most powerful as they have dominated the [Hausa] Quarter politically ever since its foundation in 1916. The Chief of the Quarter has always been a cattle landlord and, since 1930, he has also been the chief of the cattle market."³⁴ In each of these cases a new type of relationship has been added to or substituted for a preexisting one, leading to a transactional integration at the national or regional level.

Second, clientelism can lead to a *pyramiding of client-patron ties*, and, through the recruitment of new brokers, to an expansion of local or regional reciprocities on a more inclusive scale. The appropriation of landed fiefs under the hegemony of a royal clan, accompanied by the appointment of client-chiefs, was indeed a critical feature of the twin processes of state-building and territorial aggrandizement that took place throughout the Interlacustrine zone.³⁵ Similarly, much of the structural integration conducted under the auspices of colonial powers can be viewed in terms of a pyramiding of patron-client ties.

Third, there may be a *substitution rather than an extension of patron-client relationships*, leading to a continuation of reciprocal exchanges but in a totally different partnership. Here again the evidence, both historical and contemporary, is considerable, though not always as conclusive as one might hope. Perhaps the most striking example of the phenomenon is offered by the Rwandese revolution of 1959–62, leading to the eviction of the Tutsi patrons and a redefinition of clientelistic partnerships within the Hutu stratum.³⁶

Empirically, these types of adjustments are neither self-sustaining nor mutually reinforcing. Although the building-up of dominant parties sharing the qualities of political machines is in part traceable to the selective use of one or

³⁰ The basic theoretical work on this theme is Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, 1967). The relevance of Blau's insights to an understanding of traditional African political structures is suggested by Polanyi's description of reciprocity patterns in precolonial Dahomey; see Karl Polanyi, *Dahomey and the Slave Trade: An Analysis of an Archaic Economy* (Seattle & London: University of Washington Press, 1966), especially pp. 33–95.

³¹ Alvin W. Gouldner, "The Norm of Reciprocity: A Preliminary Statement," *American Sociological Review*, 15 (1960), 175.

³² Foltz, "Social Structure & Political Behavior of Senegalese Elites."

³³ See Claudine Vidal, "Le Rwanda des Anthropologues ou le Fétichisme de la Vache," *Cahiers d'Etudes Africaines*, 9, 35 (1969), 384–401.

³⁴ Abner Cohen, "The Social Organization of Credit . . .," p. 12.

³⁵ A very similar process appears to have taken place in Northern Nigeria; see, for example, M. G. Smith, *Government in Zazzau*, pp. 6–8.

³⁶ See René Lemarchand, *Rwanda and Burundi* (London: The Pall Mall Press, 1970), and "Les relations de clientèle comme agent de contestation: Le cas du Rwanda," *Civilisations*, 18, 4 (1968), 553–573; see also Yves Person, *Samori: Une Révolution Dioula, Mémoires de l'IFAN*, No. 80 (Dakar, 1968); for another historical example of the same phenomenon, see Moussa Oumar Sy, "Le Dahomey: Le Coup d'Etat de 1818," *Folia Orientalia*, 6 (1965), 205–238.

more of these adjustive mechanisms, their mere occurrence is no guarantee that the end result will be the creation of a nationwide political machine. Whether operating individually or in conjunction with each other, these processes may lead to widely different types of relationships between the clientelistic substructures and the center, as well as between the constituent segments of each substructure. Just as one may conceive of different types of machines operating alongside each other, one may also envisage the possibility of the party machine being superimposed upon, and in some ways tributary of, microlevel solidarities of a traditional type. Similarly, one may conceive of a machine being wholly centered upon the party structure (as in the Ivory Coast), or, on the contrary, revolving almost exclusively around bureaucratic structures of a traditional or neotraditional type (as in Ethiopia, or in Burundi until 1967). In short, before one can generalize about the effects of these internal adjustments on the political systems of contemporary Africa, it may be useful to consider the conditions which in recent times have reinforced, hampered, or altered the operation of traditional forms of clientelism.

These conditions can best be understood by looking at (1) the processes of incorporation achieved through the imposition of colonial controls; (2) the alternative structures of opportunity made available through the introduction of new resources; and (3) the new competitive processes arising from the conjunction of (1) and (2). Only by taking each of these variables into account is it possible to gain a clear picture of the fourth and most crucial variable—ethnicity.

Patterns of Incorporation, Allocation, and Competitiveness

Difficult though it may be to generalize about the effects of colonial rule upon traditional forms of clientelism, the weight of evidence suggests important areas of compatibility between the two. At the broadest level of generalization one might argue, first, that the colonial situation has created a psychological climate eminently congenial to the maintenance of a "dependence complex"³⁷ among Africans.

³⁷ The expression, borrowed from Dominique O. Mannoni, *Psychologie de la Colonisation* (Paris: Editions du Seuil, 1950), carries unfortunate connotations as it suggests a cultural "hang up" of some sort or another, evenly distributed among all Africans, regardless of rank or status. I substantially agree with Professor Uphoff's objection (in a personal communication), to the effect that one is here dealing not so much with a "dependency complex" as with "more or less fruitful role relationships" (resource-exchange relationships) which a person having few resources

Whether or not Mannoni's depiction of the personality structure of the Malagasy can be applied across the board to Africans in general, a convincing case can be made for the view that the homage of dependence sought by European colonizers through the projection of their own "Prospero complex" onto the African scene must have fostered similar dispositions on the part of traditional patrons toward their clients.³⁸ Second, the conditions of rapid social change generated through various transformative influences—economic, social, and political—probably intensified the clients' need for something or somebody to depend on; the heightened feelings of insecurity released by social change, along with the resultant quest for anxiety-reducing mechanisms, have operated to reinforce the strength of client-patron relationships. Third, for reasons of practicality or opportunism, European administrators have often been very tolerant toward existing forms of clientelism, and sometimes equally receptive to the creation of new ones. Where these expressed the structure of political relations—as in the case of patrimonial clientelism—and where their suppression would have entailed greater costs than seemed justifiable or tolerable, the tendency of European officials has been to use traditional clientelistic structures as instruments for mediating and legitimizing their rule.

seeks to establish. Creating a client-patron relationship is often the most fruitful. Has anybody found a "dependency complex" in a wealthy market-woman, or a powerful chief, or even a clan head? I suggest that what Europeans have observed is a function of power (or powerlessness) rather than of "Africanness." What should be emphasized here is the persistence of certain psychological orientations to politics, which often tend to outlive the specific power relationships from which they originally stemmed. Externalization of the superego, leading to its identification with an outside authority or "patron" figure, is certainly a key characteristic of clientelistic orientations, a point persuasively argued by James Scott in *Political Ideology in Malaysia* (New Haven and London: Yale University Press, 1968), p. 80 ff. For a discussion of the affective component which enters into the patron-client nexus, see Abram Cohen, *Custom and Politics in Urban Africa* (Berkeley and Los Angeles: University of California Press, 1969), p. 91.

³⁸ See Dominique O. Mannoni, *Psychologie de la Colonisation*; for further insights into the influence of the colonial framework upon the behavior and orientation of indigenous actors see J. P. Nettl and Robert Robertson, *International Systems and the Modernization of Societies* (New York: Basic Books, 1968), esp. pp. 63-127. The notion of "inheritance situation" discussed by the authors provides a useful analytic framework for an understanding of the persistence of clientelistic norms after independence; see in particular their discussion of "Benefactor and Beneficiary as Their Reference Groups," pp. 72-81. Their analysis of the process of the "role-taking of the benefactor by the beneficiary" evokes some of the familiar dimensions explored by Mannoni in the work cited above.

Yet if there are excellent reasons for viewing "the pyramiding of patron-client ties" as a by-product of European rule, one can easily exaggerate the extent to which traditional African societies really lent themselves to this kind of manipulation. Not only can one discern major discontinuities in the overall distribution and extensiveness of clientelistic networks in any given territory (suggesting corresponding variations in the serviceability of the incorporated units); one can also perceive fundamental differences in the degree to which European policies proved compatible with the preservation of traditional forms of clientelism.

If the first type of discontinuity may clearly be attributed to the different patterns of incorporation achieved through the more or less erratic tracing of colonial boundaries, there are several possible explanations for the differential impact of colonial policies on traditional forms of clientelism.

One such explanation lies in the hypothesis advanced by Ronald Cohen that "given a similar incorporative experience differences in traditional African political structures will create significant differences in the way these systems and the people within them adapt to the modern nation-state."³⁹ Another possible explanation has to do with the official policies and practices of European powers, and in particular the extent to which the effective practice of indirect rule presupposed the retention of clientelism of a feudal variety. Where the preservation of "feudal clientelism" was more or less consistently regarded as a necessary adjunct of indirect rule (as in Northern Nigeria), feudal and patrimonial forms of clientelism have evidently reinforced each other, structurally or normatively.⁴⁰ But one must also note the persistence of clientelistic patterns where indirect rule was neither regarded as legitimate nor sys-

tematically pursued, and where the social system made no allowance for the operation of feudal clientelism. Thus, drawing upon his personal experience as a former colonial administrator, Pierre Alexandre notes that in those parts of French-speaking West Africa that had a high rate of turnover among European officials, "African officials, shop clerks or cocoa brokers and catechists were, on the whole, permanent;" African "middlemen" were thus cast in the role of patrons in relation to the masses, with "clientelism cutting across tribal divisions, or, more widely, across traditional jural relationships."⁴¹

A third explanatory factor, operating independently of the formal assumptions underlying colonial policies, involves the variable assessments of the economic implications of client-patron relationships made by European administrators. Where traditional clientelism was found incompatible with the immediate or long-range economic objectives of the colonial authorities and accordingly done away with, as in Rwanda in the early 1950s, its suppression had profoundly destabilizing consequences for the internal cohesion of the community at large.

Economic motives may have precisely the opposite results, however, perpetuating or even accentuating the patron-client nexus at the community level. Here two different types of situations may be distinguished: One, most clearly suggested by the case of the Mouride brotherhoods of Senegal, involved a deliberate attempt by the European colonizer to draw maximum benefits from the manipulation of clientelistic relations. Although this may not have been the sole motive, the prospects of economic development offered by the marabouts' control over most of the peanut producing areas, and indeed their control of the labor force needed for agricultural development, were certainly instrumental in prompting the French to preserve by all possible means the "saintly" relationship which to this day links the *talibés* to their *sheikhs*—and in trying to establish a similar, though far less "saintly," relationship between themselves and the *sheikhs*.⁴²

Recognition or reinforcement of clientelistic patterns may also occur in response to the economic demands of indigenous local communities. Meillassoux's discussion of the role of *diula* in the Ivory Coast provides a good illustra-

³⁹ Ronald Cohen, "Research Directions in Political Anthropology," *Canadian Journal of African Studies*, 3, 1 (1969), p. 27.

⁴⁰ In Bornu, according to Cohen, "patron-client relationships continue to be of great importance . . . and people still utilize their associations with important superiors in the emirate, not the Nigerian, social system"—a statement that might also apply, *mutatis mutandis*, to contemporary Rwanda society within the context of an all-Hutu social system, and to Burundi within what threatens to become an all-Tutsi social system. (The above quotation is from Ronald Cohen's comments (p. 105) on Elizabeth Colson's article, "Competence and Incompetence in the context of Independence," *Current Anthropology*, 8, 1-2 (1967), 92-100. For a discussion of the transformations that have affected the operation of patron-client ties in contemporary Rwanda, see René Lemarchand, "Political Instability in Africa: The Case of Rwanda and Burundi," *Civilisations* 16, 3 (1966), 319, and Lemarchand, *Rwanda and Burundi*.

⁴¹ From Alexandre's comments on Colson's article, p. 100.

⁴² Behrman, "The Political Influence of Muslim Brotherhoods in Senegal."

tion.⁴³ Although they were initially denied access to their original trading areas by the French army, supposedly because of the need to set up a "commercial blockade" to facilitate the subjugation of the local communities, the *diula* traders were subsequently allowed to resume their commercial activities after their former trading partners, the Gouro, repeatedly demanded that right for them. Writing in the early sixties, Meillassoux noted:

*Diula relations with the Gouro compared to the precolonial period are apparently the same: In addition to kola—of which they largely control the trade to-day—they buy coffee and cocoa. They even improved their position in regard to their geographical extension. . . . They are the only intermediaries at the first level in the coffee-buying process.*⁴⁴

In each of these two instances, then, economic considerations have tended not only to encourage existing clientelistic networks but also to facilitate the forging of new links between the center and the periphery. Thus, through their economic base in the Gouro milieu, the *diula* were able to capitalize upon the new opportunities opened by European commerce, and to establish themselves as intermediaries between local producers and European firms; similarly, through their economic and religious influence in the countryside, the maraboutic elites of Senegal were able to provide the crucial connecting links between the rural communities and the central governmental-bureaucratic structures.

As the foregoing makes plain, the new allocative centers set up under the colonial powers have generated new resources and linkages. How these in turn have affected African societies can best be understood by reference to (1) the type of resources generated by the colonial environment, (2) the channels through which these resources have been allocated, and (3) the transaction flows attendant upon the emergence of new reciprocities.

Taking the term "resources" in its broadest sense, as Dahl does, to describe "anything that can be used to sway the specific choices or the strategies of another individual," we discover that each of the patterns of adjustment noted earlier are reducible to the adaptation of clientelistic strategies to different resources—or, if one takes the view that clientelism is itself a resource, to different combinations of resources.

"Political resources," writes Dahl, "can be pyramided in much the same way that a man who starts out in business sometimes pyramids a small investment into a large corporate empire."⁴⁵ Just as the building up of corporate and African empires can be seen as one example of pyramiding of clientelistic and other resources, the fashioning of new political entities under imperial rule may be viewed in basically the same terms.

In the latter case, however, allowance has to be made for the introduction of new resources—in the form of wealth, education, technological skills, and the like—and new types of reciprocities. The result has been a shift of emphasis away from symbolic or affective rewards to expectations of material benefits, in turn leading to a sloughing off of the "mechanical" solidarities characteristic of feudal clientelism for a type of relationship, if not always based on "organic" solidarities in Durkheim's sense, at least much closer to those associated with "machine politics." Conceptualizing this transformation in terms of different perceptions of a patron's competence, one might argue with Colson⁴⁶ that the trend in recent times has been to dissociate political competence from traditional status distinctions and to identify it with the ability to manipulate technical and political resources effectively. European patrons were sought after not only because of their political "pull" but because they controlled the technical resources and know-how through which the demands of their clients could be met; by the time African "bosses" stepped into the shoes of the European patrons, however, political competence meant in essence the ability to keep in tow a reliable clientele through the dispensation of prebends. Political parties in many instances took on the qualities of political machines.

Thus, another way of looking at political change in colonial and independent Africa is from the standpoint of the new partnerships established at the center, first between Europeans and Africans, and later among Africans. But these new partnerships did not always exclude the maintenance of traditional clientelistic solidarities at the local level, or indeed, the reinforcement of such solidarities through a "spill-over" effect.

Therefore the weakness of African political machines may be due not, as some have suggested,⁴⁷ to their operating at the national

⁴³ Claude Meillassoux, *Anthropologie Economique des Gouro de Côte d'Ivoire* (Paris: Mouton & Co., 1964), and "The Gouro-Peripheral Markets between the Forest and the Sudan," in Paul Bohannan and George Dalton, eds., *Markets in Africa* (Garden City: Anchor Books, 1965), pp. 67-92.

⁴⁴ Bohannan and Dalton, p. 86.

⁴⁵ Robert A. Dahl, *Who Governs?* (New Haven & London: Yale University Press, 1961), p. 226, 227.

⁴⁶ Colson, "Competence and Incompetence in the Context of Independence."

⁴⁷ See James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63, 4 (1969), 1142-1158.

rather than the local level, but to their operating at cross-purposes with local machines rendered all the more powerful by their control of traditional and modern political resources.

For an example of how the "spillover" of reciprocities at the local or regional level may inhibit national integration one can do no better than quote C. S. Whitaker on the organization of the NPC in Northern Nigeria:⁴⁸

The structure of the NPC fits conveniently into this structure of traditional [clientage] relationships in at least two important respects. First, by virtue of powers it exercised through control of the government, the party was a principal agency of patronage offices, loans, scholarships, contracts, and other opportunities sought by the upwardly mobile. This could be accomplished either directly and formally or indirectly and informally through the medium of the party or ex-party men who dominated the public boards, corporations, and commissions. Second (and of greater consequence in terms of winning mass support), the interlocking directorate of local administrative and party personnel inescapably bound humble persons to traditionally august figures in their capacity as party men. The dependency that derived from the vast network of clientage relationships inherent in the traditional society were transferred to the party. Loyalty to the NPC became a way of defraying traditional political obligations.

In the national context of Nigeria, the "traditionally august figures" of the Northern Region played the role of brokers or middlemen; poised between the national and regional cultures and communities, they became the manipulators of first- and second-order resources—i.e., of patronage offices, loans, scholarships, and contracts on the one hand, and of contacts and connections on the other.⁴⁹ Similar roles have been played, with greater or lesser degrees of success by the *ganwa* in Burundi, the *sheikhs* in Senegal, the *hassan* and *zawya* in Mauretania, the *saza* chiefs in Buganda, and some traditional chiefs in the Ivory Coast, Ghana, Niger, and Upper Volta.

The relative importance of traditional patrons in the mediating process, both before and

after independence, must be seen in the light of the brokerage functions they once performed at the juncture of external and internal clientelistic channels, the latter operating within the African society and the former through the sponsorship of the European oligarchy. As brokers they were all in some degree concerned with the transmission of the same resource, namely to maintain and facilitate strategic contacts and communication with individuals holding direct access to political resources. They became, in Boissevain's words, "professional manipulators of people and information to bring about communication."⁵⁰ Following up Boissevain's thesis that "brokerage is a business [in which] a broker's *capital* consists of his personal network of relations with people, and his *credit* of what others think his capital to be," one could regard the passage from colonial to independent status as representing successive transfers to capital and credit from one set of brokers to another—presumably from "traditional" to "modern" brokers. Yet in many instances, traditional patrons (chiefs, subchiefs, landlords, etc.) managed to retain a high credit rating among their people even when their capital at the national level was low or nonexistent. Thus, in the early sixties, the traditional Tutsi chiefs of Rwanda had lost virtually all contacts with the Belgian administration; but in spite of their loss of capital, certain segments of the peasantry persisted in believing that they still enjoyed considerable influence in administrative spheres. Conversely, a transfer of political credit from the traditional patrons to the new recipients of wealth and influence did not necessarily imply a commensurate transfer of capital from one category to the other. Thus instead of speaking of "patron" and "mass" parties to characterize African political groups, it might be more useful to distinguish between different patron parties enjoying different credit ratings and capital assets in different circumstances and environments.⁵¹

⁴⁸ Boissevain, "Patrons and Brokers."

⁴⁹ C. S. Whitaker, *The Politics of Tradition*, p. 375.

⁵⁰ The distinction between first and second order resources is drawn from Jeremy Boissevain, "Patrons and Brokers," (unpublished ms.). According to Boissevain a broker differs from a patron in that the former has control over second resources (i.e., contacts and connections) and the latter over first order resources; although the distinction between types of resources holds important implications in the context of colonial and independent Africa, it has relatively little utility for distinguishing between patrons and brokers. A more useful discussion of brokerage functions will be found in Marc J. Swartz, "The Political Middleman," in *Local-Level Politics: Social and Cultural Perspectives*, ed. Marc J. Swartz (Chicago: Aldine, 1968), pp. 199-204.

⁵¹ One might, for example, compare the different ways in which recruitment and patronage have affected the fortunes of the NCNC and the NPC in Nigeria. As Wolpe's study of Port Harcourt politics so plainly indicates, the growth of the NCNC has tended to reflect "profound alterations in the structures and criteria of political recruitment." With the opening of new resources at each stage of the NCNC's historical development, a new category of elites emerged, displacing the old, and assuring that wealth and numbers became the most significant political resources. At this stage "the wealthy businessmen and traders became party 'patrons,' the petty traders and contractors, party workers (or town councillors). . . . Simultaneously, the proportion of administrators and clerical functionaries on the one hand, and of professionals, educators and journalists, on the other, declined" (Wolpe, "Port Harcourt . . ." pp. 482-483). In the

That some traditional patron parties managed to keep relatively high credit ratings in times of high demand-inflation (as in Northern Nigeria, Burundi, and even Ghana), while others did not, invites consideration of the different transaction flows generated through clientelistic structures.⁵² The analysis of transaction flows between traditional patrons and clients can easily be distorted by the normative idiom in which these relationships are phrased. Where the notion of an opposition is ruled out by the nature of the relationship (as in Northern Nigeria),⁵³ one might all too easily assume the existence of mutually indulging transactions when in fact there is little more than grudging acquiescence on the part of clients. Yet transactions of a mutually rewarding type clearly did occur during and after the colonial period between, say, the *diula* traders of the Ivory Coast and their traditional "clients" (the Gouro); the "saintly" patrons of Senegal and their *talibés*; the traditional Emirs of Northern Nigeria and some of their subordinates. In exchange for salt, cattle and *sombe*, the Gouro gave the *diula* the kola, coffee and cocoa which enabled them to become wealthy middlemen; in exchange for a promise of salvation, marabouts received from the *talibés* the labor they needed to cultivate their fields; in return for the dispensation of psychological and material gratifications, the Fulani Emirs received deference, prestige, and services from their clients, and in time, they received the political support they needed to retain control over the provincial legislature. Because of the Emirs' control over the recruitment process, and because such control was regarded as the "natural" expression of their traditional prerogatives, "those elected became, by the same logic, the clients of the Emirs."⁵⁴

case of the NPC, by contrast, control over the party apparatus remained firmly in the hands of the emirs throughout the preindependence period and for some time thereafter; moreover, neither wealth nor numbers played a significant part in recruitment processes or patronage. Status, rather, was the key factor. Thus if the picture conveyed by Port Harcourt politics in the fifties reminds one of contemporary New Haven or New York, this could hardly be said of Kano or Zaria. In one case power belonged to what Dahl called the "ex-plebes," in the other to the "patricians." See Robert Dahl, *Who Governs*, pp. 11-24 and pp. 32-51.

⁵³ See B. J. O. Dudley, "Traditionalism and Politics: A Case Study of Northern Nigeria," *Government and Opposition*, 2, 4 (1967), 516.

⁵⁴ Dudley, p. 518.

In the case of Nigeria, restriction of the scope of mutually advantageous transactions to the northern region, coupled with the cultural differences between North and South, has undoubtedly stimulated feelings of regional particularism and even secessionist aspirations in the North. In both the Ivory Coast and Senegal, however, the expansion of reciprocities on a broad geographical and socioeconomic scale has tended to favor a similar extension of integrative bonds. The profits accumulated by the Ivoirien planters were also shared by the *diula* intermediaries and even by the chiefs as "some profited directly from the new crops and most benefited at least from the increased wealth of their kinsmen;"⁵⁵ similarly, while some of the marabouts of Senegal proved eminently useful in aggregating support for the UPS elites (in particular through the sale and distribution of party cards),⁵⁶ they also received substantial rewards from the party leadership, in hard cash as well as in the form of policy outputs. In each country national integration was achieved through a widening of clientelistic ties accompanied by mutually rewarding transactions.

Clearly, mutually rewarding exchanges at one level can create basic incompatibilities at another; but one can also conceive of mutually rewarding exchanges deteriorating over time into mutually depriving ones, with similarly dysfunctional consequences for national integration. Consider the case of Mali: Initially the *diula* community shared a close identity of interest with the Union Soudanaise (US) elites, largely because of their "opposition to European commercial policies and interests."⁵⁷ They provided the US elites with strategic contacts along the Niger, among both rural and urbanized Africans, and in turn expected their contribution to the cause of Malian nationalism to be repaid in the form of new economic opportunities for themselves and their clientele.⁵⁸ Yet by the mid-

⁵⁵ Ruth Schachter Morgenthau, *Political Parties in West Africa* (Oxford: The Clarendon Press, 1964), p. 171.

⁵⁶ Donal Cruise O'Brien, "The Murids of Senegal . . ."

⁵⁷ Thomas Hodgkin & Ruth Schachter Morgenthau, "Mali," in James S. Coleman and Carl G. Rosberg, *Political Parties and National Integration in Tropical Africa* (Berkeley & Los Angeles: University of California Press, 1964), p. 223.

⁵⁸ These expectations were natural considering the tenor of the economic policies contemplated by the US leaders: "At its extraordinary congress of 1960, the Union Soudanaise confirmed publicly some of its leaders' long-held economic views: The party resolved to launch 'immediately and vigorously' a program of economic decolonization; to create a new economic structures . . . to develop communications, intensify agricultural production, launch new industries, stress mineral exploration. . . ." Aristide Zolberg, "Political Revival in Mali," *Africa Report*, 10, 7 (1965), 18.

'sixties, the socialist options of the Keita regime had alienated a substantial number of *diula*, and the events of 1967 further intensified their sense of disaffection toward the regime.⁵⁹ The recent history of Guinea tells a somewhat similar story: The *diula* played a crucial role in the diffusion of the PDG ideology in the late 'fifties, but as in Mali, and for much the same reasons, their opposition has since become a source of major concern for the regime.

Implicit in the foregoing is that with the opening or closing off of political resources and the proliferation of new brokerage roles, competition is likely to set in among different clientelistic networks. Although the shape of competitive patterns and their implications for national integration can only be ascertained empirically, at least two generalizations suggest themselves: one is that competition is likely to take place at different levels depending on the time period considered; another is that the rate and character of clientelistic competition tends to vary with the nature of the resources made available at any given time.

A major consequence of the opening, and subsequent exhaustion, of the political resources released through the manipulation of nationalist ideology and propaganda has been to intensify clientelistic competition between African and European patrons and in time between different sets of African patrons. Each type of competition has tended to interact with and affect the outcome of the other. The history of Rwanda, for example, clearly shows the destabilizing consequences of competitive strivings between African and European patrons upon preexisting clientelistic networks. Already in the 'twenties, as a result of "their involvement in ever-widening spheres of African life, [the White Fathers] assumed more and more the character of patrons within the existing system. . . . The Hutu accepted these new patrons, seeing in them an additional source of security."⁶⁰ The increments of security gained by Hutu clients inevitably meant a loss of social prestige for the Tutsi patrons; the adverse consequences which this implied for the applica-

tion of indirect rule caused the administration to intervene and reverse the trend. By the late 'fifties and early 'sixties, however, the approach of independence raised the prospects of new political opportunities for the Hutu; and the ever-expanding involvement of the Church and the administration in the process of institutional transfer, while adding significantly to the rate of competition between Hutu and Tutsi elites, provided the Hutu elite with the resources which made possible the disengagement of the Hutu masses from the hold of their traditional patrons.

If the case of Rwanda shows an intensification of inter-African rivalries through European involvement, the history of the Ivory Coast in the late forties illustrates precisely the opposite. In Rwanda the conflict of interest between Hutu and Tutsi elites (which might be conceptualized in terms of competition between a presumptive and an incumbent set of brokers) was too fundamental not to supersede their common grievances against their European patrons; in the Ivory Coast the conflict of interest between African and European planters (also reducible to a competition between different sets of brokers) was too deep, and the economic deprivations it entailed for the Africans too widespread, not to supersede inter-African rivalries. Moreover, the concurrence of economic interests between traditional chiefs and African planters further diminished the possibilities of such rivalries.⁶¹ This situation played directly into the hands of the nationalist elites, converting the indigenous Ivoirien planters into fervent PDCI militants, and providing at the same time the economic resources through which further support could be generated from below and new networks of clientelism activated. The significance of the contribution (economic and political) made by African planters to the cause of Ivoirien nationalism becomes all the more apparent when compared with the very different situation then confronting African planters in Ghana: "In Ghana there

⁵⁹ For an excellent discussion of Mali's socialist options under Modibo Keita, see John N. Hazard, "Marxian Socialism in Africa: The Case of Mali," *Comparative Politics*, 2, 1 (1969), 1-16. The political implications of Mali's decisions to reenter the franc zone, in 1967, are analyzed in Francis Snyder, "An Era ends in Mali," *Africa Report*, 14; 3, 4 (1969), 16-23.

⁶⁰ Alison Des Forges, "Kings without Crowns: The White Fathers in Rwanda," in Daniel McCall, Norman R. Bennett, and Jeffrey Butler, eds., *Eastern African History*, Boston University Papers on Africa, Vol. III (New York: Praeger, 1969), p. 181.

⁶¹ Ruth Schachter Morgenthau, *Political Parties in French-Speaking West Africa*, p. 171. Equally worth emphasizing are the patron-client relationships which in many cases prevailed between migrant Mossi workers and Ivoirien planters; these relationships help explain why the Mossi minority resident in the Ivory Coast never (at least until recently) became the source of acute intergroup tensions. According to M. Dupire, "L'attitude des immigrants Mossi envers les autochtones est faite de respect, de soumission et de crainte. Ils conservent souvent d'excellentes relations avec leurs anciens patrons, qu'ils assistent en reconnaissance des chances qu'ils leur ont offertes." See M. Dupire, "Planteurs Autochtones et Etrangers en Basse Côte d'Ivoire," *Etudes Eburnéennes*, 8 (1960), 50.

were no European planters and the African farmers were much less involved in nationalist activities."⁶²

That the rate and character of clientelistic competition have tended to reflect the nature of the resources available is again nowhere better illustrated than in Rwanda:⁶³ Only with the opening of new political resources, through elections and constitutional reforms, did the conflict of interest between Hutu and Tutsi elites assert itself in any structured fashion; only when they realized the futility of attaining their political goals through electoral processes did the Hutu elites actively court the support of European patrons; and it was largely through the manipulation of violence that the Hutu elites were able to aggregate a solid political clientele. If violence made security a scarce resource, clientelism in turn provided the means by which security could be obtained.

In this case, the conjunction of electoral processes and violence provided the key resources through which a traditional form of clientelism reasserted itself—albeit in somewhat modified fashion—within an ethnically homogeneous stratum. Elsewhere, however, elections, with or without violence, provided the conditions for the emergence of a different type of clientelism, identified with dominant parties often sharing the characteristics of political machines. The irony is that the same forces that encouraged the birth of political machines also helped activate the ethnic solidarities which in many instances destroyed the machine as a form of political organization.

Ethnicity as an Independent Variable

This brings us to a consideration of ethnicity as an independent variable: How does ethnicity—whether taken to refer to the "givens" or "as-

sumed givens of social existence"⁶⁴—affect the scope, stability, and functioning of clientelistic networks? The answer is anything but clear. At least three possible types of situation suggest themselves, each in some way contradicting the other: The activation of ethnic solidarities may lead to a rupture of client-patron ties (as in Rwanda); or it may set sharp cultural or geographical limitations (or both) on the scope of clientelistic networks (as in Nigeria); or it may coexist with or indeed foster clientelistic ties on a nationwide basis (as in the Ivory Coast, Liberia, and, in a qualified sense, Senegal).

What might otherwise seem like a thoroughly confusing situation becomes somewhat more comprehensible in the light of the following caveats: (1) Although clientelism and ethnicity are in many ways interrelated, they refer to basically different levels of solidarity; whereas clientelism describes a personalized relationship, ethnicity is fundamentally a group phenomenon;⁶⁵ hence, there is no compelling reason to expect concomitant variations between ethnic and client-patron solidarities. (2) Just as group solidarities can be activated at many different levels and with varying degrees of intensity, one must also expect wide variations in the extent to which ethnic solidarities can be said to conflict with or reinforce patron-client solidarities. Whether or not the activation of solidarities in one sphere leads to a weakening of such solidarities in the other is an empirical question that can only be answered in the light of specific situations. (3) Seldom has there been a direct relationship between ethnicity and clientelism; as our previous discussion makes clear, the relationship between the two can best be seen in the light of certain intervening variables, structural and conjunctural.

These intervening variables may conveniently be analyzed under the following three major headings: (1) the nature of the structural "fit" between ethnic or cultural boundaries on the one hand, and status differences between patron and clients on the other; (2) the actor's perceptions of the relative costs and benefits involved in the maintenance of particular patron-client sets; (3) the pattern of resource allocation prevailing at any given time.

"By itself," writes M. G. Smith, "clientage is

found in Wolpe, "Port Harcourt: Ibo Politics in Microcosm," esp. pp. 478-485.

⁶⁴ Clifford Geertz, "The Integrative Revolution," in Clifford Geertz, ed., *Old Societies and New States* (New York: The Free Press of Glencoe, 1963), p. 109.

⁶⁵ I am grateful to Professor Crawford Young for forcefully reminding me of this fact.

⁶² Morgenthau, p. 170. This is why the CPP in Ghana never developed the characteristics of a "machine" in the sense in which the term might apply to the PDCI in the Ivory Coast. Feith's description of the CPP as a machine which "exists almost exclusively to stay in power" conveys an oversimplified view of both the CPP and machine politics and thus fails to bring out the essential differences between the CPP and the PDCI: the latter's original clientele was not made up of "verandah boys" but of rural workers; its resource base is still overwhelmingly dependent upon a plantation economy; and its "bosses" are themselves closely connected with the rural sector, most of them still owning large plantations. As of 1969, the *Bureau Politique* and *Comité Directeur* of the PDCI comprised, respectively, five and eight planters, and at least five of the members of the High Court of Justice and the State Security Court were planters. Cf. Edward Feith, "Military Coups and Political Development: Some lessons from Ghana and Nigeria," *World Politics*, 20, 2 (1968), 179-194.

⁶³ Further evidence of this phenomenon may also be

unlikely to dissolve sectional boundaries wherever differences of culture, religion or race are prominent."⁶⁶ By the same token, where cultural cleavages not only are prominent but tend to coincide with patrons and clients, these cleavages may help break up the patron-client nexus. Thus, the greater the cultural differences between patrons and clients, and the more conspicuous the social distance between them, the greater the likelihood of violent ethnic strife in conditions of rapid social mobilization.⁶⁷ Rwanda is an obvious example: Just as the spread of egalitarian ideas among Westernized Hutu elements threatened the "premise of inequality" which underlay the traditional order, the emergence of this presumptive elite, together with the support they received from the European administration and the Church, provided the conditions through which a new category of patrons was able to displace the old, and a "new" and ethnically homogenized society substituted for the old pluralistic order. Although Burundi has tended toward a similar polarization of ethnic loyalties, the greater flexibility of its stratification system and the absence of a rigorous coincidence of ethnic and status distinctions have significantly delayed ethnic conflict.

Yet, even where cultural or ethnic differences between patrons and clients might otherwise spell a lack of cohesiveness, preexistent clientelistic activities may reorient the actors' perceptions of their mutual interest so as to promote transactional integration among different ethnic segments, whether at the local or national levels. This phenomenon is perhaps best illustrated by the solidarities that have developed between the Hausa landlords of Ibadan and the predominantly Yoruba Action Group leaders of the Western Region of Nigeria; what initially began as a mutually advantageous economic transaction between cattle landlords and Yoruba butchers developed into similarly advantageous reciprocities between the Action Group leadership and the Hausa landlords:

When during the 1950's the butchers affiliated themselves within . . . the Action Group, which was until 1962 in power in the Western Region,

⁶⁶ M. G. Smith, "Institutional and Political Conditions of Pluralism," in L. Kuper and M. G. Smith, *Pluralism in Africa* (Berkeley and Los Angeles: University of California Press, 1969), p. 56.

⁶⁷ This tends to confirm the more general hypothesis developed by Dahrendorf in his theory of social conflict: The more consistent the criteria of differentiation between groups—i.e., the more they tend to define and isolate groups from each other through cumulative cleavages—the greater the likelihood of violence among them. See Ralf Dahrendorf, *Essays in the Theory of Society* (Stanford: Stanford University Press, 1968).

the Hausa landlords reacted not only by joining the same party themselves, but also by dragging almost the whole Hausa Quarter with them in joining it, and in successive elections the Ibadan Hausa gave their votes to it . . . [In return for their votes], the party not only prevented hostile action being taken against the cattle landlords, but even tried to prevent individual butchers from defaulting, by exerting pressure on these to honor their obligations, and sometimes by granting loans to those among them who did not have the cash to pay.⁶⁸

Although integration in this case occurred partly because mercantile clientelism had spilled over into the political sphere, economic interests will not everywhere and inevitably assert their primacy and lead to political compromise. Moreover, compromise may also be due to several other contributory factors. It may be due to perceptions of external threats that may submerge perceptions of local antagonism, or—as in the case discussed above—to the relative smallness of the migrant community and its consequently weak political leverage regionally or nationally.

Where no prior transactional integration exists among groups, how does the allocation of new resources affect interethnic or intercommunal relations? The answer depends in part on the character of the resources available at any given time, and on the sources of control over these resources. Where politically significant resources tend to become the monopoly of a particular ethnic or communal group, the clientelistic solidarities arising from the exploitation of these sources may possibly reinforce ethnic or communal solidarities. Take the example of the Eastern Region of Nigeria: The displacement of "strangers" (Sierra Leonians, Yoruba, etc.) in favor, first, of Onitsha Ibo, and then Owerri Ibo, has reflected basic changes in the political significance attached to specific resources (first education, then wealth, and finally "numbers"). But it has also tended to accentuate the communal bases of clientelistic politics among Ibo. Moreover, in time the preeminence of "numbers" at the national level has tended to relegate the Ibo as a group to a comparatively minor position in the federal superstructure, and hence has dissipated communal differences within the Ibo community while at the same time exacerbating ethnic and regional tensions between Ibo and non-Ibo.

But this is by no means the only conceivable pattern. Where ultimate control over financial and economic resources is vested in nationally

⁶⁸ Abner Cohen, "The Social Organization of Credit . . ." pp. 16-17; see also his *Custom and Politics in Urban Africa*.

oriented politicians, the distributive capacities of the system can be used to foster new patterns of reciprocity among groups, associated with the techniques of machine politics. What is involved here is the creation of new solidarities based on expectations of concrete, short-run benefits. Although the men in charge of running the machine may occasionally bolster their authority by charisma or by coercion, they can best be thought of as political entrepreneurs. Their job is to weld together disparate ethnic segments through the allocation of prebends.

Nowhere is this type of clientelism better illustrated than by the development of the PDCI in the Ivory Coast between 1952 and 1959. According to Zolberg,

the PDCI secured support by co-opting ethnic leaders into the organization by means of a major distributive effort, itself made possible by the sheer accident of prosperity stemming from high prices for tropical commodities during and immediately after the Korean war, a prosperity which was later bolstered by protectionist props extended by France toward its showcase colony. . . . The entire system of communications between the center and the localities took the form of ethnic channels, through official representatives such as members of the Territorial (later National) Assembly, or other forms of ethnic clientship.⁶⁹

Although the PDCI must certainly be regarded as the prototype of the African machine, elements of the machine pattern can also be detected in the UPS of Senegal, the True Whig party of Liberia, and, to a lesser extent, the Parmehutu of Rwanda. Several other parties shared at one time or another machine characteristics—the CPP in Ghana (at least until 1960), the SLPP in Sierra Leone, and each of the three major Nigerian parties in existence until 1959. That few of these “machines” were able to maintain themselves in power raises the question of the part played by ethnicity in hastening or precipitating their demise.

Two general answers have been suggested—one related to the character of the ethnic balance in any given state, and the other to the conditions affecting the enlistment of ethnic support. Arguing from a somewhat broader perspective, James Scott hypothesizes that where a single ethnic group held a dominant position, and provided the basis for the construction of a political machine, “the excluded ethnic groups . . . demanded at the very least more regional autonomy and launched secessionist revolts in some areas. The resulting threat to the territorial integrity of the state was

commonly the occasion for military takeovers.”⁷⁰ Moreover, the elimination of electoral processes, according to Scott, deprives the machine of the incentives it needs to hold its clientele. In the absence of electoral pressures, expectation of payoffs are bound to decline. The bonds of cohesion which initially tied ethnic segments to the machine are thus gradually loosened, introducing possibilities of ethnic strife or secession.

Formulated in these terms, neither hypothesis is entirely convincing. Can one really visualize (as Scott does) the ethnic map of Nigeria as being characterized by a “dominant ethnic group”? Could not the Baule of the Ivory Coast just as well qualify as a dominant ethnic group—or for that matter the Wolof of Senegal? And if one associates ethnic dominance not with numbers but with social status and power would it not be legitimate to regard the Americo-Liberians of Liberia as “dominant”? Moreover, granting that in the American context electoral processes were instrumental in keeping political machines going, in Africa these processes have usually released those very centrifugal forces that spelled the defeat of the machine as a form of political organization. Pushing the logic of the argument a step further, one is entitled to wonder why American machines declined while electoral pressures did not. Much more relevant is Scott’s contention that “the durability of the machine as a political form is maximized . . . where it is part of a larger growing economy that can afford its expensive habits, and where its bosses do not have a monopoly of coercive authority.”⁷¹ The crucial factor, however, lies in the pattern of resource allocation, in the extent to which resources (economic and political) are allowed either to move evenly from the center to the periphery, or to accrue exclusively to a particular region or ethnic group. The Ivory Coast provides the clearest example of the first pattern: except for the Agni of Sanwi, and, to a lesser extent, the Bete (two of the least favored groups), resources, whether in the form of jobs, material payoffs, sinecures of one kind or another, or social overhead capital, are fairly evenly distributed among the representatives of the various ethnic segments. This is not to deny the existence of gross disparities of income between the Ivoirien bureaucratic-planter oligarchy on the one hand and ordinary peasants on the other;⁷² the only point here is that the

⁶⁹ Scott, “Corruption, Machine Politics and Political Change,” *American Political Science Review*, 63, 3 (1969), p. 1157.

⁷⁰ Scott, p. 1158.

⁷¹ The point is persuasively argued in Basil Davidson, “The Outlook for Africa,” *The Socialist Register* 1966 (New York: Monthly Review Press, 1966).

⁶⁸ Zolberg, “Ethnicity and National Integration,” p. 7.

oligarchy represents a fairly wide cross-section of the ethnic interests at stake.

In Nigeria, by contrast, resource allocation has tended to reflect and reinforce the fragmented pattern of cultural loyalties.

The parties themselves were part of rival business and financial structures which existed to make money for the individuals concerned and provide financial backing for the parties. . . . Each regional government has gradually acquired a number of public boards by 1959 which could be used as means of dispensing patronage to party worthies.⁷³

Not only did the machines reinforce the ethnic loyalties of their clientele; electoral processes offered new opportunities for the creation of satellite machines in regions other than those in which the parent organization operated. The effect has been to exacerbate communal tensions so that the stability of the entire federal structure is threatened.

In short, the higher the distributive capacity of the system, the more congenial the environment to the operation of machine-like clientelism; a high distributive capacity alone, however, is not a sufficient condition to ensure the stability of the system. Even more important is the capacity of the machine to strike an adequate balance of patronage along the vertical axis of class stratification and the horizontal dimension of ethnic cleavages. An exceedingly lopsided distribution of spoils on each of these dimensions may bring disaffection, revolt, or ethnic strife.

There is yet another factor to consider, perhaps the most elusive: the social context of machine politics. Drawing upon Kilson's insight that "the principle of reciprocity refers to a fundamental aspect of the traditional or neo-traditional patterns of political action [and hence] is the most effective way (for Africans) to maximize material and prestige benefits derived from politics, as well as to minimize the losses," one might look upon African machines as the embodiment of at least one aspect of African tradition.⁷⁴ Not all African machines, however, exhibit the characteristics of traditional reciprocity patterns; even where the lat-

ter prevail there is still considerable room for variation in the way in which tradition and modernity combine and coalesce. Leaving aside those rare instances where the machine operates in the context of a no-party state and becomes synonymous with palace politics (as in Ethiopia or Burundi between 1962 and 1968), at least two different types of machines can be distinguished: the more orthodox, PDCI-type of machine, in which patronage becomes the essential source of cohesion; and the "neo-traditional machine," such as the UPS, in which exchange processes between the center and the periphery are mediated by, and contingent upon, the operation of traditional forms of clientelism at the local level. In the latter case, traditional micro-level solidarities provide the essential linkages between the party and the masses; the machine is superimposed upon, and in some ways tributary to, the clientelistic subsystem.

Furthermore, in the more orthodox type machine, ethnic segments are incorporated through a mixture of patronage and co-optation; vertical solidarities are maintained through material inducements; the party structure defines the organization of the machine, and is coextensive with its field of operation. The neo-traditional machine, on the other hand, seeks to enlist the support of micro-level clientelistic structures through bargaining with traditional patrons who act as brokers between the party elites and the masses; vertical solidarities are maintained in part through material inducements but mainly through perpetuation of deference patterns between the brokers and their traditional clientele; moreover, the party structure is not synonymous with the traditional clientelistic substructures but adjacent to it. The party structure in Senegal, for example, "remains separate from the brotherhoods"; and if the marabouts are in no position to control the party, there are, likewise, limits to how far the party can impose its policies upon the marabouts.⁷⁵

The integrative action patterns (what Bailey refers to as "bridge-actions")⁷⁶ through which links are established between the subsystem and

⁷³ Kenneth W. J. Post, *The Nigerian Federal Elections of 1959* (London: Oxford University Press, 1963), p. 58. The prime example of a machine-type clientelism is offered by the NCNC, between 1955 and 1959. The political fortunes of the NCNC were heavily (if not exclusively) dependent on the direct or indirect financial support it received from Marketing Board and Eastern Region Finance Corporation, (via the African Continental Bank). See Post, p. 56-58.

⁷⁴ Martin Kilson, *Political Change in a West African State* (Cambridge: Harvard University Press, 1966), p. 268.

⁷⁵ See Lucy Behrman, "The Political Influence of Muslim Brotherhoods in Senegal."

⁷⁶ Frederick G. Bailey, *Tribe, Caste and Nation: A Study of Political Activity and Political Change In Highland Orissa* (Manchester: Manchester University Press, 1960), p. 248. In contrast with the types of strategy described earlier, usually originating from the center, a "bridge-action," in the context of this discussion, refers to the motives which cause the actors in the subsystem (whether patrons or clients) to establish a new type of relationship with another system.

the macropolitical structure are likely to vary substantially from one type of machine-clientelism to the other. In each case subordinate political actors cast their lot with the central authorities because they expect certain rewards to accrue from this relationship. In the context of the more orthodox type of political machine (like the PDCI) expectations of patronage are paramount; the securing of material benefits becomes an end in itself. For traditional brokers operating in the context of a "neo-traditional" machine, however, expectations of material payoffs are only part of a larger complex of motivations. Material incentives may only serve the purely instrumental purpose, of maximizing the symbolic or affective rewards to which the brokers consider themselves entitled. Commenting on the motives which actuate "aspiring *samba linguers*" in Senegal, Foltz notes that "money is a major goal, but primarily because it can bring with it a greater following and thus increased prestige." This characterization applies equally well to the behavior of maraboutic elites. For some the resources of the political kingdom may yield handsome dividends in cash as well as other tangible rewards; but this merely strengthens their prestige as religious leaders since, as Foltz points out, "it is popularly felt that a marabout's potential for conferring grace and ultimate salvation is reflected in his personal wealth and magnificence."⁷⁷ Here the bridge-action by which the client-patron relationship is connected to the political system is really an attempt by traditional patrons to use the resources of modern politics to reaffirm a traditional relationship.

Not all African machines, therefore, are equally dependent upon the allocation of material resources. Whereas the PDCI and the TANU, for example, are both potentially vulnerable to what Bienen calls the "vicious circle of underdevelopment" (that is, "limited resources—weak organization—limited resources"),⁷⁸ this would seem less of a danger for a neo-traditional party machine such as the UPS, in which traditional reciprocity patterns provide alternative resources for the consolidation and manipulation of grass-roots support.

To return to the question raised earlier: the incorporation of ethnic loyalties into the framework of a political machine does not necessarily do away with the brokers' loyalty to their group of origin; it merely enlarges the scope of their loyalties at the same time that it redefines

their conceptions of self-interest. In other words, the relationship between ethnicity and clientelism cannot be reduced to a zero-sum game, in which increments of clientelism augment a person's sense of self-interest in the same proportion that they diminish his loyalty to the tribe. Just as the building of modern states on the debris of the old often tends to require the adjustment of tradition to modernity, rather than the substitution of one for the other, integrative processes in Africa are more often than not dependent on a redefinition of individual conceptions of self-interest in the context of multiple group loyalties.

Political Clientelism and Development²

In trying to clarify the relationship between political clientelism and ethnicity, and the relationship of both to national integration, I have only touched upon one dimension of the developmental process. It may be useful, therefore, in conclusion, to shift to certain broader aspects of political development. Since there are almost as many definitions of political development as there are of clientelism, assessments of how one relates to the other will necessarily depend on one's conceptualization of each.

Political development can be looked at in one of two ways—as synonymous with the modernization historically experienced by the West, or, following Huntington's lead, as a process of institution building "independent of, although obviously affected by, the process of modernization."⁷⁹ The first of these approaches focuses on those forces of change leading to a dispersion of power; the second on the containment of such forces for purposes of power accumulation. In one case analysis centers upon such well-known facets of the "developmental syndrome" as (1) differentiation of political roles and structures; (2) equality as the "central operative ideal" of society; (3) capacity as "not only the logical imperative of system maintenance, but also the enhanced adaptive and innovative potentialities possessed by man for the management of his environment, human and nonhuman."⁸⁰ In the other the microscope is turned on "the institutionalization of organizations and procedures" as the absolute precondition of political development.

It takes no special gift of intuition to realize that the second of these approaches is clearly more compatible with our earlier characteriza-

⁷⁷ Foltz, "Social Structure and Political Behavior . . ."

⁷⁸ Henry Bienen, *Tanzania: Party Transformation and Economic Development* (Princeton: University Press, 1967), p. 412.

⁷⁹ Samuel P. Huntington, "Political Development and Political Decay," *World Politics*, 17, 3 (1965), 393.

⁸⁰ James S. Coleman, "Introduction" in James S. Coleman, ed., *Education and Political Development* (Princeton: Princeton University Press, 1965), p. 15

tion of machine-clientelism, and that neither is particularly congenial to traditional forms of clientelism. Indeed, in the light of the assumption which underlies the first of these approaches, the contribution of traditional clientelism to political development might seem negligible if not counterproductive: "Political development," writes Coleman, "can be regarded as the acquisition by a political system of a *consciously-sought, and qualitatively new and enhanced political capacity* as manifested in the successful institutionalization of (1) *new patterns of integration . . . and (2) new patterns of participation and resource distribution. . .*"⁸¹ (my emphasis). Making due allowance for the variations noted earlier among different types of traditional clientelism, one might argue that the differentiation of political roles and structures along functional lines is hardly compatible with the dichotomous, particularistic basis of role-differentiation between patrons and clients; that the principle of equality is violated by the very definition of traditional clientelism; and that the distributive capacities of the political system are lessened rather than enhanced by the "prebendal" features of traditional clientelism and the subjective criteria it implies for allocating resources. Moreover, since traditional forms of clientelism have rarely been coextensive with the boundaries of the newly created states, "the institutionalization of political organizations and procedures" would seem to require rather different materials than those available from traditional clientelistic networks.

The trouble with this line of reasoning is, first, that it so reifies traditional forms of clientelism as to deny their adaptive capacities; and second, that it overstresses the compartmentalization between "traditional" and "modern" forms of clientelism, thus ignoring the contribution that one makes to the other. Each of these points requires elaboration.

The first point has already received partial substantiation from our previous discussion: That certain forms of traditional clientelism are more adaptable than others, and may lend themselves to innovation both on a horizontal and vertical plane can be explicitly inferred from our analysis of the "spillover" and "pyramiding" processes. Suffice it to note here that some of the specific features conventionally associated with modernization can just as well be identified with the operation of traditional clientelism: the patron-client relationship may contain, in germ as it were, the elements of a further hierarchical differentiation of political

roles and structures; in spite of traditional status differences between patrons and clients, the reciprocities attendant upon this relationship may lead to an equalization of wealth among the parties; and the distributive capacities of the system may be enhanced as a result of the brokerage functions performed by local patrons and the control they exercise at the subsystem level.

That there may be limits to how far traditional patrons can be relied upon to accelerate processes of modernization is equally obvious; yet the very obstacles to mass political mobilizations that may be raised by traditional clientelism can be turned into major assets in the sphere of institution building. Traditional clientelism regulates political mobilization along traditional patron-client lines and maintains a measure of cohesion among ethnic segments, meanwhile permitting the accumulation of political and economic capital at the center. To the extent that it does these things, traditional clientelism releases those very resources that are needed for the "institutionalization of organizations and procedures," while at the same time insulating the ethnic subsystem from the disruptive impact of modernization. The main dilemma in this case is that those very forces that encourage the growth of political institutions at the center may in time generate their own destruction. The more power that is accumulated at the center, the greater the potential for initiating social and economic change, and hence also the greater the chances that modernization may sap the strength of traditional clientelistic networks.

Whether power accumulation at the center leads to a decay of traditional clientelism on the periphery, or, on the contrary, provides the support necessary for its continuation, depends in part on the balance of reciprocities between the central political institutions and the clientelistic subsystem. As long as exchange processes are seen as mutually satisfying by the machine politicians at the center and the traditional brokers in the countryside, the system retains its homeostatic qualities. Preservation of the system's equilibrium, however does not necessarily lead to political development. When the power asymmetry between patrons and clients is matched by gross disparities of wealth and income, and when the resources generated through this relationship become the privilege of a sybaritic class of political entrepreneurs, the chances for political development are slim. Liberia is a prime example: Although the True Whig party represents one of the most successful efforts at institutionalization in contempo-

⁸¹ Coleman, p. 15.

rary Africa (indeed to the point where it is now regarded as too successful by the incumbent elites), Liberia is hardly a showcase of successful political development. "Economic growth has not spread its benefits evenly but has tended to make the rich richer,"⁸² meanwhile keeping the populations of the rural hinterland in a state of near stagnation. Economic choices in Liberia, as elsewhere in Africa, have paid relatively little attention to conventional criteria of long-range economic and political development; the volume and quality of immediate political and economic payoffs have been the only yardsticks for evaluating the soundness of social and economic policies. Although similar criticisms have been leveled against the Ivoirien plantocracy,⁸³ the evidence in this case suggests a far less lopsided pattern of exchange, as well as a more conscious attempt to de-emphasize short-run benefits for long-term developmental goals.

The next point is that the juxtaposition of modern and traditional value orientations at different levels of the clientelistic structure may meet the requirements of modern organizational forms at the center and yet impose serious handicaps on the system's capacity to innovate. What Powell describes as the "rural problem-solving system"⁸⁴—whereby demands for goods and services in the rural sector are met by governmental agencies in return for voting support—is not a very widely accepted procedure in contemporary Africa; the least that can be said is that in specific instances the social context imposes major limitations on how far the local traditional patrons can tolerate reformist moves without at the same time destroying their own bases of support. The setbacks suffered by the "rural animation" program in Senegal illustrate the point: Having noted that "determined maraboutic opposition is reserved for those governmental ventures which have more than mere technical content, those inspired by theories of socialist development, which are designed not only to improve methods of cultivation but also to bring radical changes in the social structure of rural Senegal," Donal Cruise O'Brien goes on to observe that "rural animation . . . was the worst of all these schemes from a maraboutic point of view."⁸⁵ Only to the extent that exchange processes are perceived as

mutually beneficial by government officials and rural patrons is there a reasonable chance of harnessing developmental goals to the clientelistic substructure. This is where the cooperative movement in Senegal, in contrast with rural animation schemes, offered genuine scope for innovation.⁸⁶

Exchange processes are conditioned not only by perceptions of self-interest but by the overall availability of resources at the center. Thus where clientelism becomes a technique for neutralizing a political opposition absorbing at the same time the economic and financial resources needed for development, the result is evidently dysfunctional in terms of the developmental process.⁸⁷ Much, of course, depends on the overall volume of disposable resources, and the relative costs involved in the diversion of such resources. In an environment of scarcity, where the demands for jobs or sinecures become ever more pressing with every new generation of university graduates, clientelism as a preventive political technique may lose its instrumental value for the achievement of long-range development goals.

Clearly, considerably more empirical data must be assembled before the patron-client model can serve as a general instrument for assessing developmental strategies. Much of what has been said in this section, therefore, must be seen as a starting point for further investigation rather than a conclusive formulation of the relationship of clientelism to development. At least three major avenues of inquiry suggest themselves:

(1) There is, first, the problem of determining the relationship of different types of clientelism to patterns of resource allocation. How effective, for example, is the party-institutionalized type of clientelism, as compared to the traditional or neo-traditional types, in promoting what Uphoff calls "political market integration" and "increases in factor endowments?"⁸⁸ What are the intervening variables that need to be taken into account in order to assess the rela-

⁸² O'Brien.

⁸³ See "Tubman Asks for Opposition," *West Africa*, January 3, 1970.

⁸⁴ Samir Amin, *Le Développement du Capitalisme en Côte d'Ivoire* (Paris: Editions de Minuit, 1967).

⁸⁵ John D. Powell, "Peasant Society and Clientelist Politics," *American Political Science Review*, 64 (June, 1970), 411-425.

⁸⁶ O'Brien, *The Murids of Senegal*

⁸⁷ Absorption of opposition elements (mainly student and trade-union leaders) through clientelistic techniques has been a standard procedure in both Senegal and the Ivory Coast; how far this technique can be used without posing a major threat to development goals remains to be seen. For further information on the sources and implications of the *mouvements contestataires* in each country, see *Le Monde* (Sélection Hebdomadaire), December 18-24, 1969.

⁸⁸ See Norman T. Uphoff, "Ghana and Economic Assistance: Impetus and Ingredients for a Theory of Political Development," a paper prepared for delivery at the annual meeting of the American Political Science Association, Los Angeles, 1970.

tive advantages and disadvantages attendant upon each type of clientelism? What sorts of factors or circumstances are likely to bring about the conversion of one type of clientelism into another, and with what consequences for the political system? So far only the most speculative answers can be given to these questions.

(2) Closely related to the foregoing is the question of how new political resources are likely to affect the operation of preexisting patron-client networks, and, conversely, how new types of resources, whether defined in terms of status, wealth, authority or legitimacy, can be generated out of such networks. Passing reference must be made in this connection to the wealth of interesting insights to be gleaned from Ilchman and Uphoff's seminal work on *The Political Economy of Change*: Besides offering a much more rigorous conceptualization of "political resources" than has been attempted in this article, by emphasizing the productivity and convertibility of political resources made available through self-interested exchanges, the authors suggest new and theoretically significant perspectives from which to look at clientelistic reciprocities.⁸⁹

(3) A third area of investigation concerns the relationship between external and internal forms of dependency. However much disagreement there may be among political scientists about the extent of economic, social, and political control exercised over their former dependencies by metropolitan powers, it is difficult not to recognize the reality of such control. How these external links of dependency may in

turn affect the operation of patron-client ties at attention from students of African politics. It the domestic level has as yet received little attention may be, however, that just as the international dimensions of clientelism can help us elucidate some of its domestic manifestations, the latter may in turn give us valuable insights into what Nkrumah once referred to, not inaccurately, as "clientele sovereignty." If nothing else, some of the phenomena currently lumped together under the convenient rubric of "neo-colonialism" may thus be brought into a universe of discourse more amenable to dispassionate inquiry.⁹⁰

In view of what remains to be done to operationalize further the concept of clientelism, some may well feel that our claim to have identified the "missing link" in current theories of development is grossly exaggerated. Some questions may also arise about the universality of the phenomenon described, or the presumed generality of its manifestations. Certainly, the conclusions one may reach about the effect of clientelism on political conflict, or the absence of conflict, are bound to reflect the nature of the empirical data from which one seeks to generalize. That something does lie between "ethnicity" and "class" in contemporary Africa is nonetheless undeniable. Thus, whether referred to as "political clientelism," "the patron-client nexus," or "dependency relationships," the kind of interpersonal relationships described in this article may yet emerge as one of the central foci of inquiry in African scholarship. My hope, in the meantime, is that it can stimulate some fresh thinking on the part of those interested in political development.

⁸⁹ Warren E. Ilchman and Norman T. Uphoff, *The Political Economy of Change* (Berkeley and Los Angeles: University of California Press, 1969). Other discussions of political exchange that might interest social scientists dealing with clientelist politics, include: Anthony Heath, "Exchange Theory," *British Journal of Political Science*, Vol. 1, Part 1 (January 1971), 91-119, and Georges Dupré and Pierre-Philippe Rey, "Réflexions sur la pertinence d'une théorie de l'histoire des échanges," *Cahiers Internationaux de Sociologie*, 46 (Jan.-June 1969), pp. 133-162.

⁹⁰ These and other questions are tentatively dealt with in my unpublished manuscript "Political Exchange, Clientelism, and Development in Tropical Africa", a paper presented for delivery at the annual meeting of the Southern Political Science Association, Atlanta, 1970.

Patron-Client Politics and Political Change in Southeast Asia

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The analysis presented here is an effort to elaborate the patron-client model of association, developed largely by anthropologists, and to demonstrate its applicability to political action in Southeast Asia. Inasmuch as patron-client structures are not unique to Southeast Asia but are much in evidence particularly in Latin America, in Africa and in the less developed portions of Europe, the analysis may possibly have more general value for an understanding of politics in less developed nations.

Western political scientists trying to come to grips with political experience in the Third World have by and large relied on either (or both) of two models of association and conflict. One model is the horizontal, class model of conflict represented most notably by Marxist thought. It has had some value in explaining conflict within the more modern sector of colonial nations and in analyzing special cases in which rural social change has been so cataclysmic as to grind out a dispossessed, revolutionary agrarian mass. By and large, however, its overall value is dubious in the typical nonindustrial situation where most political groupings cut vertically across class lines and where even nominally class-based organizations like trade unions operate within parochial boundaries of ethnicity or religion or are simply personal vehicles. In a wider sense, too, the fact that class categories are not prominent in either oral or written political discourse in the Third World damages their *a priori* explanatory value.

The second model, and one which comes much closer to matching the "real" categories subjectively used by the people being studied, emphasizes primordial sentiments (such as ethnicity, language, and religion), rather than horizontal class ties. Being more reflective of self-identification, the primordial model naturally helps to explain the tension and conflict that increasingly occurred as these isolated, ascriptive groups came into contact and competed for power. Like the class model, however—although less well developed theoretically—the primordial model is largely a conflict model and is of great value in analyzing hostilities between more or less corporate and ascriptive cultural groupings.¹ Important as such

conflict has been, it hardly begins to exhaust the political patterns of Southeast Asia and Africa, let alone Latin America. If we are to account, say, for intra-ethnic politics or for patterns of cooperation and coalition building among primordial groups, then the primordial model cannot provide us with much analytical leverage.

The need to develop a conceptual structure that would help explain political activity that does not depend solely on horizontal or primordial sentiments is readily apparent in Southeast Asia.² In the Philippines, for example, class analysis can help us understand the recurrent agrarian movements in Central Luzon (e.g. Sakdalistas and Huks) among desperate tenants and plantation laborers; but it is of little help in explaining how Magsaysay succeeded in weaning many rebels away from the Huks, or, more important, in analyzing the normal patterns of political competition between Philippine parties. In Thailand, primordial demands may help us discern the basis of dissident movements in North and Northeast Thailand, but neither primordialism nor class analysis explains the intricate pattern of the personal factions and coalitions that are at the center of oligarchic Thai

tive Revolution," in Geertz, ed., *Old Societies and New States* (New York: The Free Press of Glencoe 1963); and Max Gluckman (*Custom and Conflict in Africa* [Oxford: Basil and Blackwell, 1963]).

² A number of political studies of Southeast Asia have dealt with factionalism or patron-client ties. The most outstanding is Carl Landé's *Leaders, Factions and Parties: the Structure of Philippine Politics*, Monograph No. 6 (New Haven: Yale University—South East Asia Studies, 1964). For the Thai political system, Fred W. Riggs' *Thailand: The Modernization of a Bureaucratic Polity* (Honolulu: East-West Center Press, 1966) and David A. Wilson's *Politics in Thailand* (Ithaca: Cornell University Press, 1962) pursue a similar line of analysis; and for Burma, see Lucian W. Pye, *Politics, Personality, and Nation-Building: Burma's Search for Identity* (New Haven: Yale University Press, 1962). Some notable attempts to do comparable studies outside Southeast Asia are: Colin Leys, *Politicians and Policies: An Essay on Politics in Acholi Uganda 1962–1965* (Nairobi: East Africa Publishing House, 1967); Myron Weiner, *Party-Building in a New Nation: The Indian National Congress* (Chicago: University of Chicago Press, 1967); Paul R. Brass, *Factional Politics in an Indian State* (Berkeley and Los Angeles: University of California Press, 1965); Frederick G. Bailey, *Politics and Social Change: Orissa in 1959* (Berkeley and Los Angeles: University of California Press, 1963).

¹ Two influential anthropologists who employ this mode of analysis are: Clifford Geertz ("The Integra-

politics. The almost perpetual conflicts between the central Burman state and its separatist hill peoples and minorities are indeed primordial, communal issues, but communalism is of no use in accounting for the intra-Burman struggles between factions within the Anti-Fascist People's Freedom League (AFPFL) or, later, within the military regime. Ethnicity and class do carry us far in explaining racial hostilities and intra-Chinese conflict in Malaya, but they are less helpful when it comes to intra-Malay politics or to interracial cooperation at the top of the Alliance party.³

As these examples indicate, when we leave the realm of class conflict or communalism, we are likely to find ourselves in the realm of informal power groups, leadership-centered cliques and factions, and a whole panoply of more or less instrumental ties that characterize much of the political process in Southeast Asia. The structure and dynamics of such seemingly *ad hoc* groupings can, I believe, be best understood from the perspective of patron-client relations. The basic pattern is an informal cluster consisting of a power figure who is in a position to give security, inducements, or both, and his personal followers who, in return for such benefits, contribute their loyalty and personal assistance to the patron's designs. Such vertical patterns of patron-client linkages represent an important structural principle of Southeast Asian politics.

Until recently the use of patron-client analysis has been the province of anthropologists, who found it particularly useful in penetrating behind the often misleading formal arrangements in small local communities where inter-personal power relations were salient. Terms which are related to patron-client structures in the anthropological literature—including "clientelism," "dyadic contract," "personal network," "action-set"—reflect an attempt on the part of anthropologists to come to grips with the mosaic of nonprimordial divisions. Informal though such networks are, they are built, they are maintained, and they interact in ways that will permit generalization.

Although patron-client analysis provides a solid basis for comprehending the structure and dynamics of nonprimordial cleavages at the local level, its value is not limited to village studies. Nominally modern institutions such as bureaucracies and political parties in Southeast

Asia are often thoroughly penetrated by informal patron-client networks that undermine the formal structure of authority. If we are to grasp why a bureaucrat's authority is likely to depend more on his personal following and extrabureaucratic connections than on his formal post, or why political parties seem more like *ad hoc* assemblages of notables together with their entourages than arenas in which established interests are aggregated, we must rely heavily on patron-client analysis. The dynamics of personal alliance networks are as crucial in the day-to-day realities of national institutions as in local politics; the main difference is simply that such networks are more elaborately disguised by formal facades in modern institutions.

In what follows, I attempt to clarify what patron-client ties are, how they affect political life, and how they may be applied to the dynamics of Southeast Asian politics. After 1) defining the nature of the patron-client link and distinguishing it from other social ties, the paper 2) discriminates among different varieties of patron-client bonds and thereby establishes some important dimensions of variation, and 3) examines both the survival of and the transformations in patron-client links in Southeast Asia since colonialism and the impact of major social changes (such as the growth of markets, the expanded role of the state, and so forth) on the content of these ties.

I. The Nature of Patron-Client Ties

The Basis and Operation of Personal Exchange.

While the actual use of the terms "patron" and "client" is largely confined to the Mediterranean and Latin American areas, comparable relationships can be found in most cultures and are most strikingly present in preindustrial nations. *The patron-client relationship—an exchange relationship between roles—may be defined as a special case of dyadic (two-person) ties involving a largely instrumental friendship in which an individual of higher socioeconomic status (patron) uses his own influence and resources to provide protection or benefits, or both, for a person of lower status (client) who, for his part, reciprocates by offering general support and assistance, including personal services, to the patron.*⁴

³ Class as well as ethnicity is relevant to Malay-Chinese conflict, since the different economic structure of each community places them in conflict. Many a rural Malay experiences the Chinese not only as pork-eating infidels but as middlemen, money lenders, shopkeepers, etc.—as the cutting edge of the capitalist penetration of the countryside.

⁴ There is an extensive anthropological literature dealing with patron-client bonds which I have relied on in constructing this definition. Some of the most useful sources are: George M. Foster, "The Dyadic Contract in Tzintzuntzan: Patron-Client Relationship," *American Anthropologist*, 65 (1963), 1280-1294; Eric Wolf, "Kinship, Friendship, and Patron-Client Relations," in Michael Banton, ed., *The Social Anthropology of Complex Societies*, Association of Applied Social Anthropology Monograph #4 (Lon-

In the reciprocity demanded by the relationship each partner provides a service that is valued by the other. Although the balance of benefits may heavily favor the patron, some reciprocity is involved, and it is this quality which, as Powell notes, distinguishes patron-client dyads from relationships of pure coercion or formal authority that also may link individuals of different status.⁵ A patron may have some coercive power and he may also hold an official position of authority. But if the force or authority at his command are alone sufficient to ensure the compliance of another, he has no need of patron-client ties which require some reciprocity. Typically, then, the patron operates in a context in which community norms and sanctions and the need for clients require at least a minimum of bargaining and reciprocity; the power imbalance is not so great as to permit a pure command relationship.

Three additional distinguishing features of patron-client links, implied by the definition, merit brief elaboration: their basis in inequality, their face-to-face character, and their diffuse flexibility. All three factors are most apparent in the ties between a high-status landlord and each of his tenants or sharecroppers in a traditional agrarian economy—a relationship that serves, in a sense, as the prototype of patron-client ties.⁶

First, there is an imbalance in exchange between the two partners which expresses and reflects the disparity in their relative wealth, power, and status. A client, in this sense, is someone who has entered an unequal exchange relation in which he is unable to reciprocate fully. A debt of obligation binds him to the patron.⁷ How does this imbalance in reciprocity

arise? It is based, as Peter Blau has shown in his work, *Exchange and Power in Social Life*,⁸ on the fact that the patron often is in a position to supply unilaterally goods and services which the potential client and his family need for their survival and well being. A locally dominant landlord, for example, is frequently the major source of protection, of security, of employment, of access to arable land or to education, and of food in bad times. Such services could hardly be more vital, and hence the demand for them tends to be highly inelastic; that is, an increase in their effective cost will not diminish demand proportionately. Being a monopolist, or at least an oligopolist, for critical needs, the patron is in an ideal position to demand compliance from those who wish to share in these scarce commodities.

Faced with someone who can supply or deprive him of basic wants, the potential client in theory has just four alternatives to becoming the patron's subject.⁹ First he may reciprocate with a service that the patron needs badly enough to restore the balance of exchange. In special cases of religious, medical, or martial skills such reciprocation may be possible, but the resources of the client, given his position in the stratification, are normally inadequate to re-establish an equilibrium. A potential client may also try to secure the needed services elsewhere. If the need for clients is especially great, and if there is stiff competition among patron-suppliers, the cost of patron-controlled services will be less.¹⁰ In most agrarian settings, substantial local autonomy tends to favor the growth of local power monopolies by officials or landed gentry. A third possibility is that clients may coerce the patron into providing services. Although the eventuality that his clients might turn on him may prompt a patron to meet at least the minimum normative standards of exchange,¹¹ the patron's local power and the ab-

don: Tavistock Publications, 1966), pp. 1–22; J. Campbell, *Honour, Family, and Patronage* (Oxford: Clarendon Press, 1964); John Duncan Powell, "Peasant Society . . .," p. 412, Carl Landé, *Leaders, Factions and Parties . . .*," Alex Weingrod, "Patrons, Patronage, and Political Parties," *Comparative Studies in Society and History*, 10 (July, 1968), pp. 1142–1158.

⁵ Powell, "Peasant Society and Clientelist Politics," *American Political Science Review*, 64 (June, 1970), 412.

⁶ Another comparable model, of course, is the lord-vassal link of high feudalism, except in this relationship the mutual rights and obligations were of an almost formal, contractual nature. Most patron-client ties we will discuss involve tacit, even diffuse standards of reciprocity. Cf. Ruston Coulborn, ed., *Feudalism in History*, (Princeton: Princeton University Press, 1956).

⁷ In most communities this sense of obligation is a strong moral force, backed by informal community sanctions that help bind the client to the patron. A good account of how such feelings of debt reinforce social bonds in the Philippines is Frank Lynch's description of *utang na loob* in *Four Readings in Philip-*

pine Values, Institute of Philippine Culture Papers, No. 2 (Quezon City: Aleneo de Manila Press, 1964).

⁸ Peter M. Blau, *Exchange and Power in Social Life* (New York: Wiley, 1964), pp. 21–22. Blau's discussion of unbalanced exchange and the disparities in power and deference such imbalance fosters is directly relevant to the basis of patron-client relationships.

⁹ These general alternatives are deduced by Blau (p. 118) and are intended to be exhaustive.

¹⁰ Later, we will examine certain conditions under which this may actually occur.

¹¹ There is little doubt that this last resort usually acts as a brake on oppression. The proximate cause for many peasant uprisings in medieval Europe during hard times often involved revocation of small rights granted serfs by their lords—e.g., gleanings, rights, use of the commons for pasturage, hunting and fishing privileges, reduction of dues in bad crop years—rights which offered a margin of security. Such re-

sence of autonomous organization among his clients make this unlikely. Finally, clients can theoretically do without a patron's services altogether. This alternative is remote, given the patron's control over vital services such as protection, land, and employment.

Affiliating with a patron is neither a purely coerced decision nor is it the result of unrestricted choice. Exactly where a particular patron-client dyad falls on the continuum depends on the four factors mentioned. If the client has highly valued services to reciprocate with, if he can choose among competing patrons, if force is available to him, or if he can manage without the patron's help—then the balance will be more nearly equal. But if, as is generally the case, the client has few coercive or exchange resources to bring to bear against a monopolist-patron whose services he desperately needs, the dyad is more nearly a coercive one.¹²

The degree of compliance a client gives his patron is a direct function of the degree of imbalance in the exchange relationship—of how dependent the client is on his patron's services. An imbalance thus creates a sense of debt or obligation on the client's part *so long as it meets his basic subsistence needs* and represents, for the patron, a "‘store of value’—social credit that . . . (the patron) can draw on to obtain advantages at a later time."¹³ The patron's domination of needed services, enabling him to build up savings of deference and compliance which enhance his status, and represents a capacity for mobilizing a group of supporters when he cares to. The larger a patron's clientele and the more dependent on him they are, the greater his latent capacity to organize

group action. In the typical agrarian patron-client setting this capacity to mobilize a following is crucial in the competition among patrons for regional preeminence. As Blau describes the general situation,

The high-status members furnish instrumental assistance to the low-status ones in exchange for their respect and compliance, which help the high-status members in their competition for a dominant position in the group.¹⁴

A second distinguishing feature of the patron-client dyad is the *face-to-face*, personal quality of the relationship. The continuing pattern of reciprocity that establishes and solidifies a patron-client bond often creates trust and affection between the partners. When a client needs a small loan or someone to intercede for him with the authorities, he knows he can rely on his patron; the patron knows, in turn, that "his men" will assist him in his designs when he needs them.¹⁵ Furthermore, the mutual expectations of the partners are backed by community values and ritual.

In most contexts the affection and obligation invested in this tie between nonrelatives is expressed by the use of terms of address between partners that are normally reserved for close kin. The tradition of choosing godparents in Catholic nations is often used by a family to create a fictive kinship tie with a patron—the godfather thereby becoming like a brother to the parents.¹⁶ Whether the model of obligation established is father-son, uncle-nephew, or elder-younger brother, the intention is similar: to establish as firm a bond of affection and loyalty as that between close relatives. Thus while a patron and client are very definitely alive to the instrumental benefits of their association, it is not simply a neutral link of mutual advantage. On the contrary, it is often a durable bond of genuine mutual devotion that can survive severe testing.

The face-to-face quality of the patron-client dyad, as well as the size of the patron's resource base, limits the number of direct active ties a single patron can have.¹⁷ Even with vast

volts, even though they generally failed, served as an object lesson to neighboring patrons. Cf. Friedrich Engels, *The Peasant War in Germany* (New York: International Publishers, 1966); Norman Cohn, *The Pursuit of the Millennium* (New York: Harper, 1961); and E. B. Hobsbawm, *Primitive Rebels* (New York: Norton, 1959).

¹² Blau, *Exchange and Power in Social Life*, pp. 119–120, makes this point somewhat differently: "The degree of dependence of individuals on a person who supplies valued services is a function of the difference between their value and that of the second best alternative open to them." The patron may, of course, be dependent himself on having a large number of clients, but his dependence upon any *one* client is much less than the dependence of any one client upon him. In this sense the total dependence of patron and client are similar, but almost all the client's dependence is focused on one individual, whereas the patron's dependence is thinly spread (like that of an insurance company—Blau, p. 137) across many clients. Cf. Godfrey and Monica Wilson, *The Analysis of Social Change; Based on Observations in Central Africa* (Cambridge: The University Press, 1945), pp. 28, 40.

¹³ Blau, p. 269.

¹⁴ Blau, p. 127.

¹⁵ The classic analysis of the functions of gift-giving (prestation) in creating alliances, demonstrating superiority, and renewing obligations, is Marcel Mauss, *The Gift: Forms and Functions of Exchange in Archaic Societies* (Glencoe, Illinois: The Free Press of Glencoe, 1954).

¹⁶ See Sidney Mintz and Eric Wolf, "An Analysis of Ritual Co-Parenthood (Compadrazgo)," *Southwestern Journal of Anthropology* 6 (Winter, 1948) pp. 425–437.

¹⁷ Carl Landé, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics," Unpublished manuscript (March, 1970), p. 6.

resources, the personal contact and friendship built into the link make it highly unlikely that an active clientele could exceed, say, one hundred persons. The total following of a given patron may be much larger than this, but normally all except 20–30 clients would be linked to the patron through intermediaries. Since we are dealing with positive emotional ties (the ratio of “calculation” to affection may of course vary), a leader and his immediate entourage will be comparatively small.

The third distinctive quality of patron-client ties, one that reflects the affection involved, is that they are *diffuse*, “whole-person” *relationships rather than explicit, impersonal-contract bonds*. A landlord may, for example, have a client who is connected to him by tenancy, friendship, past exchanges of services, the past tie of the client's father to his father, and ritual coparenthood. Such a strong “multiplex” relation, as Adrian Mayer terms it,¹⁸ covers a wide range of potential exchanges. The patron may very well ask the client's help in preparing a wedding, in winning an election campaign, or in finding out what his local rivals are up to; the client may approach the patron for help in paying his son's tuition, in filling out government forms, or in getting food or medicine when he falls on bad times. The link, then, is a very flexible one in which the needs and resources of the partners, and hence the nature of the exchange, may vary widely over time. Unlike explicit contractual relations, the very diffuseness of the patron-client linkage contributes to its survival even during rapid social change—it tends to persist so long as the two partners have something to offer one another.¹⁹ Just as two brothers may assist each other in a host of ways, patron-client partners have a relationship that may also be invoked for almost any purpose; the chief differences are the

greater calculation of benefits and the inequality that typifies patron-client exchange.

The Distinctiveness of the Patron. The role of patron ought to be distinguished from such role designations as “broker,” “middleman,” or “boss” with which it is sometimes confounded. Acting as a “broker” or “middleman”—terms which I shall use interchangeably—means serving as an intermediary to arrange an exchange or transfer between two parties who are not in direct contact. The role of middleman, then, involves a three party exchange in which the middleman functions as an agent and does not himself control the thing transferred. A patron, by contrast, is part of a two-person exchange and operates with resources he himself owns or directly controls.²⁰ Finally, the terms “middleman” and “broker” do not specify the relative status of the actor to others in the transaction, while a patron is by definition of superior rank to his client.

Important as this distinction is, it is easily lost sight of for two reasons. First, it is not always a simple task to determine if someone personally controls the resources he uses to advance himself. What of the case in which a civil servant distributed the subordinate posts in his jurisdiction to create an entourage? Here it would seem that he was acting as a patron, inasmuch as the jobs he gave out were meant as *personal* gifts from the store of scarce values he controlled and were intended to create a feeling of personal debt and obligation among recipients. The social assessment of the nature of the gift is thus crucial. If we were to find, on the other hand, that the civil servant was viewed as someone who had acted as an agent of job-seekers and put them in touch with a politician who controlled the jobs, then he would be acting as a broker. It is only natural that many an ambitious public official will seek to misrepresent acts of brokerage or simple adherence to the rules as personal acts of patronage, thereby building his following.²¹ To the extent that he

¹⁸ Adrian C. Mayer, “The Significance of Quasi-Groups in the Study of Complex Societies,” in Michael Banton, ed., *The Social Anthropology of Complex Societies*, pp. 97–122. Mayer would call a short-term, contractual interaction that was limited in scope a *simplex* tie.

¹⁹ In another sense the patron-client dyad is fragile. Since it is a diffuse, noncontractual bond, each partner is continually on guard against the possibility that the other will make excessive demands on him, thus exploiting the friendship. A patron may, for example, prefer to hire an outsider for an important job because he can then contractually insist that the work be of top quality. With a client, it would be a delicate matter to criticize the work. As in friendship, “the diffuseness of the [patron-client] obligation places a corresponding demand for self-restraint on the parties if the relationship is to be maintained.” William A. Gamson, *Power and Discontent* (Homewood, Illinois: Dorsey, 1968), p. 167.

²⁰ A *broker* does, in a real sense *have a resource: namely, connections*. That is, the broker's power—his capacity to help people—is predicated on his ties with third parties.

²¹ U.S. Congressmen spend a good portion of their time trying to seize personal credit for decisions which benefit their constituents whether or not they had anything to do with the decision—as broker or patron. For similar reasons, cabinet ministers in Malaysia and elsewhere have travelled about the country with government checks in hand, making grants to mosques, temples, and charitable groups in a way that will dramatize the largesse as an act of personal patronage. Every government decision that benefits someone represents an opportunity for someone to use that act to enlarge the circle of those personally obligated to him.

succeeds in representing his act as a personal act of generosity, he will call forth that sense of personal obligation that will bind his subordinates to him as clients.²²

A second potential source of confusion in this distinction is that the terms designate roles and not persons, and thus it is quite possible for a single individual to act both as a broker and a patron. Such a role combination is not only possible, but is empirically quite common. When a local landowning patron, for example, becomes the head of his village's political party he is likely to become the middleman between many villagers and the resources controlled by higher party officials. In this case he may have clients for whom he also serves as broker. The diffuse claims of the patron-client tie actually make it normal for the patron to act as a broker for his clients when they must deal with powerful third parties—much as the patron saint in folk Catholicism who directly helps his devotees while also acting as their broker with the Lord.²³ If on the other hand, the political party simply gives the local patron direct control of its programs and grants in the area, it thereby enhances his resources for becoming a patron on a larger scale and eliminates the need for brokerage.

Patrons ought finally to be differentiated from other partly related terms for leadership such as "boss," "caudillo," or "cacique." "Boss" is a designation at once vague and richly connotative. Although a boss may often function as a patron, the term itself implies (a) that he is the most powerful man in the arena and (b) that his power rests more on the inducements and sanctions at his disposal than on affection or status. As distinct from a patron who may or may not be the supreme local leader and whose leadership rests at least partly on rank and affection, the boss is a secular leader *par excellence* who depends almost entirely on palpable inducements and threats to move people. As we shall show later, a settled agrarian environment with a recognized status hierarchy is a typical setting for leadership by patrons, while a more mobile, egalitarian environment is a typical setting for the rise of bosses. The final two terms, "caudillo" and "cacique" are most commonly used in Latin America to designate the regional—often rural—bosses. Again the implication is that coercion is a main pillar of power, and in the case of the *caudillo*, a personal following is

common.²⁴ A "cacique" or "caudillo" may act as patron to a number of clients but he typically relies too heavily on force and lacks the traditional legitimacy to function mainly as a patron. At best, a *cacique* or *caudillo* may, like a boss, be a marginal special case of a patron, providing that a portion of his following is beneath him socially and bound to him in part by affective ties. Over time, however, a metamorphosis may occur. Just as the successful brewer owner of late 18th century England might well anticipate a peerage for his son, the *cacique* who today imposes his rule by force may do well enough to set his son up as a landowner, whose high status and legitimacy strengthens his role as patron.

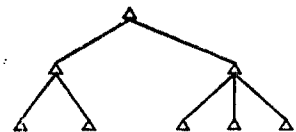
Patron and Clients as Distinctive Groupings.

To this point, the discussion has centered on the nature of the single link between patron and client. If we are to broaden the analysis to include the larger structures that are related by the joining of many such links, a few new terms must be introduced. First, when we speak of a patron's immediate following—those clients who are directly tied to him—we will refer to a patron-client *cluster*. A second term, enlarging on the cluster but still focusing on one person and his vertical links is the patron-client *pyramid*. This is simply a vertical extension downward of the cluster in which linkages are introduced beyond the first-order.²⁵ Below are typical representations of such links.

Patron-Client Cluster



Patron-Client Pyramid



Although vertical ties are our main concern, we will occasionally want to analyze *horizontal dyadic ties*, say, between two patrons of comparable standing who have made an alliance. Such alliances often form the basis of *factional systems* in local politics. Finally, patron-client

²² And it naturally follows that in underdeveloped countries, where the patrimonial view of office is especially strong, a public post could be a client-creating resource.

²³ Foster, "The Dyadic Contract in Tzintzuntzan," pp. 1280-1294.

²⁴ For good descriptions of both types of leadership, see Eric R. Wolf and E. C. Hansen, "Caudillo Politics: A Structural Analysis," *Comparative Studies in Society and History*, 9, 2 (January, 1967), 168-179 and Paul Friedrich, "The Legitimacy of a Cacique," in Marc J. Swartz, ed., *Local Level Politics: Social and Cultural Perspectives* (Chicago: Aldine 1968), pp. 243-269.

²⁵ The terms "cluster," "network," and "first" and "second" orders are adapted from a somewhat similar usage by J. A. Barnes, "Networks and Political Processes," in Swartz, ed., pp. 107-130.

networks are not ego-focused but refer to the overall pattern of patron-client linkages (plus horizontal patron alliances) joining the actors in a given area or community.

Patron-client clusters are one of a number of ways in which people who are not close kin come to be associated. Most alternative forms of association involve organizing around categorical ties, both traditional—such as ethnicity, religion, or caste—and modern—such as occupation or class—which produce groups that are fundamentally different in structure and dynamics. The special character of patron-client clusters stems, I believe, from the fact that, unlike categorically-based organizations, such clusters a) have a basis of *membership that is specific to each link*,²⁶ and b) are *based on individual ties to a leader* rather than on shared characteristics or horizontal ties among followers.

Some other important distinctions between categorical and patron-client groupings follow from these particular principles of organization. Here I rely heavily on Carl Landé's more elaborate comparisons between dyadic followings and categorical groups.²⁷

1. *Members' Goals*: Clients have particularistic goals which depend on their personal ties to the leader, whereas categorical group members have common goals that derive from shared characteristics which distinguish them from members of other such groups.
2. *Autonomy of Leadership*: A patron has wide autonomy in making alliances and policy decisions as long as he provides for the basic material welfare of his clients, whereas the leader of a categorical group must generally respect the *collective* interest of the group he leads.
3. *Stability of Group*: A patron-client cluster, being based on particularistic vertical links, is highly dependent on its leader's skills and tends to flourish or disintegrate depending on the resources of the leader and the satisfaction of individual client demands. A categorical group, by contrast, is rooted more firmly in horizontally shared qualities and is thus less dependent for its survival on the quality of its leadership and more durable in its pursuit of broader, collective (often policy) interests.

4. *Composition of Group*: Patron-client clusters, because of the way they are created, are likely to be more heterogeneous in class composition than categorical groups which are based on some distinctive quality which members share. *By definition*, patron-client pyramids join people of different status rankings while categorical groups *may or may not* be homogeneous in status.

5. *Corporateness of Group*: In a real sense a patron-client cluster is not a group at all but rather an "action-set" that exists because of the vertical links to a common leader—links which the leader may activate in whole or in part.²⁸ Followers are commonly not linked directly to one another and may, in fact, be unknown to each other. An organized categorical group, by contrast, is likely to have horizontal links that join members together so that it is possible to talk of a group existence independent of the leader.

Although this listing is not exhaustive, it does illustrate the special character of patron-client networks. Bearing in mind the generic qualities of these ties, we now turn to the range of variation within the genus.

II. Variation in Patron-Client Ties

One could potentially make almost limitless distinctions among patron-client relationships. The dimensions of variation considered here are selected because they seem particularly relevant to our analytical goal of assessing the central changes in such ties within Southeast Asia. Similar distinctions should be germane to the analysis of other preindustrial nations as well.

The Resource Base of Patronage. A potential patron assembles clients on the basis of his ability to assist them. For his investment of assets, the patron expects a return in human resources—in the form of the strength of obligation and the number of clients obligated to him. The resource base or nature of the assets a patron has at his disposal can vary widely. One useful basis for distinguishing among resources is the directness with which they are controlled. Patrons may, in this sense, rely on a) their own knowledge and skills, b) direct control of personal real property, or c) indirect control of the property or authority of others (often the public). The resources of *skill and knowledge* are most recognizable in the roles of lawyer,

²⁶ Mayer, "The Significance of Quasi-Groups . . ." p. 109.

²⁷ Landé, "Networks and Groups . . ." (unpublished manuscript), pp. 6-12.

²⁸ Mayer, p. 110.

doctor, *literatus*, local military chief, teacher—religious or secular. Those equipped with these skills control scarce resources than can enhance the social status, health, or material well being of another. Inasmuch as such resources rest on knowledge, they are less perishable than more material sources—although the *time* of the expert *is* limited—and can be used again and again without being diminished. Such resources are relatively, but not entirely, secure. In the case of lawyers and literati, for example, the exchange price of their services depends respectively on the continued existence of a court system and the veneration of a particular literary tradition, both of which are subject to change. The value of a local military chief's protection is similarly vulnerable to devaluation once the nation state has established local law and order.

Reliance on *direct control of real property* is a second common means of building a clientele. Traditionally, the typical patron controlled scarce land. Those he permitted to farm it as sharecroppers or tenants became permanently obligated to him for providing the means of their subsistence. Any businessman is in a similar position; as the owner of a tobacco factory, a rice mill, or a small store he is able to obligate many of those of lower status whom he employs, to whom he extends credit, or with whom he does business. This kind of resource, in general, is more perishable than personal skills. A landlord has only so many arable acres, a businessman only so many jobs, a shopkeeper so much ready cash, and each must carefully invest those resources to bring the maximum return. Like any real property, moreover, private real property is subject to seizure or restrictions on its use.

A third resource base available to the potential patron is what might be called *indirect, office-based property*. Here we refer to patrons who build a clientele on the strength of their freedom to dispense rewards placed in their trust by some third party (parties). A village headman who uses his authority over the distribution of communal land to the poor or the distribution of *corvée* labor and taxation burdens in order to extend his *personal* clientele would be a typical example of traditional office-based patronship. One can classify similarly office-holders in colonial or contemporary settings whose discretionary powers over employment, promotion, assistance, welfare, licensing, permits, and other scarce values can serve as the basis of a network of personally obligated followers. Politicians and administrators who exploit their office in this way to reward clients while violating the formal norms of public conduct are, of course, acting corruptly. Finally,

we should add private-sector office-holders in the private sector such as plantation managers, purchasing agents, and hiring bosses, who may also use their discretionary authority to nurture a clientele.

Indirect, office-based property is least secure in many respects, as its availability depends on continuity in a position that is ultimately given or withdrawn by third parties. A landlord will usually retain his local base whereas an office-holder is likely to be swept out by a new victor at the polls or simply by a power struggle within the ruling group. In spite of the risks involved, these posts are attractive because the resources connected with many of them are far greater than those which an individual can amass directly.

The categories of resources just discussed are not mutually exclusive. It is common, for example, for a patron to have a client who is obligated to him by being a tenant on his land and also by having secured an agricultural loan through his patron's chairmanship of the ruling party's local branch. The resources that cement a dyadic tie may thus be multiple—it is often a question of deciding which is the predominant resource. Much the same analysis can be made of a patron-client cluster or network, since a patron may have clients who are bound to him by quite different resources, and it is often important to determine what the main resource is that holds the cluster together.

Resource Base of Clientage. As the other member of a reciprocating pair, the client is called upon to provide assistance and services when the patron requires them. The variation in the nature of such assistance is another means of distinguishing one patron-client dyad from another. Here one might want to differentiate: (1) *labor services and economic support*, as provided by a rent-paying tenant or employee, (2) *military or fighting duties*, such as those performed by members of a bandit group for their chief, and (3) *political services* such as canvassing or otherwise acting as an agent of a politician. Within the "political service" category one may wish to separate electoral services from nonelectoral political help. I should add here that the term "clients" can refer to those who are in the middle of a patron-client pyramid—being a client to someone higher up and a patron to those below. In this case, a superior patron will be interested in his client's potential services, but those services will include the size, skills, assets, and status of the client's own subordinate following.

Just as a patron-client dyad can be distinguished by the main resource base of clientage

so can a patron-client cluster be categorized by the modal pattern of client services for the cluster or pyramid as a whole.

Balance of Affective and Instrumental Ties. By definition, instrumental ties play a major role in the patron-client dyad. It is nonetheless possible to classify such dyads by the extent to which affective bonds are also involved in the relationship. At one end of this continuum one might place patron-client bonds which, in addition to their instrumental character, are reinforced by affective links growing, say, from the patron and the client having been schoolmates, coming from the same village, being distant relatives, or simply from mutual love. Comparable affective rewards may also spring from the exchange of deference on the one hand and *noblesse oblige* on the other in a settled agrarian status network—rewards that have value beyond the material exchanges they often involve.²⁹ At the other end of the spectrum lies a dyadic tie much closer to an almost neutral exchange of goods and services. The more purely coercive the relationship is and the less traditional legitimacy it has, the more likely that affective bonds will be minimal.

This distinction has obvious analytical value. If we were to look at a patron's entire following, we would be able to classify each vertical bond according to the ratio of affective to instrumental rewards involved. (One could, of course, do the same for horizontal alliances. Using this criterion we could identify a set of followers among whom the ratio of affective to instrumental ties was relatively high, reflecting perhaps distant kinship, old village or neighborhood ties, or comparable bonds. The loyalty of this set of followers would be less dependent upon a continued flow of material benefits, simply because their loyalty is partly based on non-material exchanges. As we move beyond this partly affective following to a patron's other supporters, the weight of instrumental, usually material, ties becomes relatively more important. The nature of a man's following—the balance of affective to instrumental ties obligating his clients to him—can tell us something about its stability under different conditions. When a patron increases his material resource base, it is his instrumental following that will tend to

grow rapidly, and when he is in decline, that same following will shrink rapidly, as clients look for a more promising leader. The degree of dependence on material incentives within a following is, in principle, a quality one could measure by establishing how much more than their present material rewards a rival patron would have to offer to detach a given number of another's clients.

The affective-instrumental distinction just made leads to a similar, but not identical, distinction between the *core* and *periphery* of a man's following. These categories actually are distributed along a continuum; at the *periphery* of a man's following are those clients who are relatively easy to detach while at the *core* are followers who are more firmly bound to him. The periphery is composed of clients bound largely by instrumental rewards, while the core is composed of clients linked by strong affective ties, *as well as* clients who are attracted to a patron by such strong instrumental ties that they seem unbreakable.³⁰ This amounts, in effect, to a distinction between a man's virtually irreducible following and his more or less fluctuating, "fair-weather" following. Patrons can then be differentiated by the size of their core-following relative to their peripheral-following. A landlord or a businessman will generally have a sizable core group composed both of his friends, kin, etc., and of his tenants or employees. This nucleus is his initial following; his clientele may grow larger, but it is unlikely to contract further than this durable core. A politician or bureaucrat, on the other hand, unless he is privately wealthy, is likely to have a comparatively smaller core group composed mostly of those with whom he has strong affective ties and, hence, a relatively large proportion of "fair-weather" clients. The blows of fortune such a politician or administrator suffers are more likely to be instantly and fully reflected in a reduction of the size of his clientele, which is largely a calculating one. Politicians, and bureaucrats, because they have smaller core followings and because they can, through their office, often tap vast resources, are apt to have meteoric qualities as patrons; the landholder, by contrast, is likely to cast a steadier, if dimmer, light.

Balance of Voluntarism and Coercion. There are obvious and important differences in the degree of coercion involved in a patron-client bond. At one end are the clients with virtually no choice but to follow the patron who directly

²⁹ There is no contradiction, I believe, in holding that a patron-client link originates in a power relationship and also holding that genuine affective ties reinforce that link. Affective ties often help legitimate a relationship that is rooted in inequality. For an argument that, in contrast, begins with the assumption that some cultures engender a psychological need for dependence, see Dominique O. Mannoni, *Prospero and Caliban: The Psychology of Colonization* (New York: Praeger, 1964).

³⁰ F. G. Bailey uses the terms "core" and "support" in much the same fashion: see his "Parapolitical Systems," in Swartz, ed., *Local-Level Politics*, pp. 281-294.

controls their means of subsistence. Here one might place a tenant whose landlord provides his physical security, his land, his implements and seed, in a society where land is scarce and insecurity rife. Nearer the middle of this continuum would perhaps be the bonds between independent smallholders who depend on a landlord for the milling and marketing of their crops, for small loans, and for assistance with the police and administration. Such bonds are still based on inequality, but the client, because he has some bargaining power, is not simply putty in his patron's hands. Finally, let us assume that an electoral system has given clients a new resource and has spurred competition among patrons for followings that can swing the election to them. In this case the inequality in bargaining power is further reduced, and the client emerges as more nearly an independent political actor whose demands will receive a full hearing from his patron.

In general, the oppression of the client is greater when the patron's services are vital, when he exercises a monopoly over their distribution, and when he has little need for clients himself. The freedom of the client is enhanced most when there are many patrons whose services are not vital and who compete with one another to assemble a large clientele—say for electoral purposes.

The greater the coercive power of the patron vis-à-vis his client, the fewer rewards he must supply to retain him. A patron in a strong position is more likely to employ *sanctions*—threats to punish the client or to withdraw benefits he currently enjoys—whereas a relatively weaker patron is more likely to offer inducements—promises to reward a client with benefits he does not now enjoy.³¹ In each instance, superior control over resources is used to gain the compliance of followers, but the use of sanctions indicates a higher order of power than the use of inducements.

Assessment of the coercive balance and of the ratio of sanctions to inducements can be made not only for a dyad but also for a patron-client cluster or pyramid. The cluster of a local baron with a private army may be held intact by a mix of deference and sanctions, while a campaigning politician may build a cluster simply with favors if he has no coercive power or traditional legitimacy. Each cluster or pyramid has its special vulnerability. The coercive cluster will be jeopardized by a breach of the patron's local power monopoly, and a cluster

based on inducements will be in danger if its leader's income or access to public funds is cut off.

Durability Over Time. Patron-client dyads may be rather ephemeral, or they may persist for long periods.³² In a traditional setting they are likely to last until one of the partners has died. Knowing how durable such ties are can also tell us something about the structure of competition over time. Where dyads are persistent they tend to produce persistent factional structures with some continuity in personnel over time, at least stable clusters or pyramids that may recombine in a variety of ways but are constructed from the same components. Where dyads are fragile, personal alignments may undergo an almost total reordering within a decade.

Since patron-client clusters are based ultimately on power relations, they will endure best in a stable setting that preserves existing power positions. A particular patron will thus retain his clients as long as he continues to dominate the supply of services they need. A patron is also likely to keep his followers if the scope of reciprocity that binds them is greater. That is, the more of the client's vital needs a patron can meet (i.e., if he can supply not only land and security but also influence with the administration, help in arranging mortgages or schooling, and so forth), the greater the tendency for the tie to be invoked frequently and to endure over long periods. Compared with patrons who can provide only legal services, only financial help, or only educational advantages, the *multiplex* bond between patron and client is a solid linkage that serves many needs; since it is more of a whole-person tie, it will be called into action often.

Homogeneity of Following. A patron may have a heterogeneous set of followers drawn from all walks of life, or he may have a following composed, say, of only his poor sharecroppers or only clerical subordinates in his office.³³ The proportion of a man's supporters who share social characteristics and the salience of those social characteristics to them constitute a measure of how homogeneous a following is. Since a patron, by definition, occupies a higher social station than his clients, the greater the homogeneity in a following, the greater the latent

³¹ Here I follow the distinctions made in Blau, *Exchange and Power in Social Life*, pp. 115–118. Other power theorists have made the same distinction.

³² Both J. A. Barnes, "Networks and Political Process," in Swartz, pp. 107–130, and Powell, *Peasant Society* . . . , p. 413, discuss this variable.

³³ This variable thus relates not to a dyad but to the following in a cluster or pyramid.

shared interests among followers that might threaten the relationship. When a landed patron whose clients are his tenants, for example, sells off what had been common pasture land, all his tenants are equally affected. Their shared situation and the common experiences it provides create a potential for horizontal ties, whereas a heterogeneous clientele lacks this potential.

Field Variables. Occasionally, we will want to describe and contrast configurations of patron-client clusters within a political arena rather than dealing with a single cluster or dyad. Four particularly useful distinctions in this respect are a) the degree of monopoly over local resources by a single patron, b) the degree of monopoly over links to other structures by a single patron, c) the *density* of patron-client linkages in the population,⁸⁴ and d) the extent of differentiation between different pyramids and clusters. The first two variables are self-explanatory and measure the degree of dominance exercised by a patron over local and supralocal resources. "Density" refers to the proportion of a given population that is a part of the patron-client network. In some situations, for example, a large part of the lower classes may not actually have any vertical links of clientage to a patron. To gauge accurately the explanatory power of patron-client politics in a political field requires that we know for how much of the population such ties are effective. Finally, the degree of differentiation among clusters is a means of discerning whether one cluster looks pretty much like the next one or whether many clusters are socioeconomically distinct. In the classical feudal situation, the pyramidal structure of one lord's small domain was similar to that of his neighbor—the social structure of the landscape resembled a repetitive wallpaper pattern—and competition was thus between almost identical units. In other circumstances, pyramids may be differentiated by predominant occupation, by institutional affiliation, and so forth, so that the seeds of a distinctive and perhaps durable *interest* have been sown.

III. Survival and Development of Patron-Client Ties in Southeast Asia

A. Conditions for Survival

As units of political structure, patron-client clusters not only typify both local and national politics in Southeast Asia, they are also as char-

acteristic of the area's contemporary politics as of its traditional politics. In one sense, the "style" of the patron-client link, regardless of its context, is distinctively traditional. It is particularistic where (following Parsons) modern links are universal; it is diffuse and informal where modern ties are specific or contractual; and it produces vertically-integrated groups with shifting interests rather than horizontally-integrated groups with durable interests. Despite their traditional style, however, patron-client clusters both serve as mechanisms for bringing together individuals who are not kinsmen and as the building-blocks for elaborate networks of vertical integration. They cannot, therefore, be merely dismissed as vestigial remains of archaic structures but must be analyzed as a type of social bond that may be dominant in some contexts and marginal in others.

In my view, most of traditional and contemporary Southeast Asia has met three necessary conditions for the continued vitality of patron-client structures: (1) *the persistence of marked inequalities in the control of wealth, status, and power which have been accepted (until recently) as more or less legitimate*; (2) *the relative absence of firm, impersonal guarantees of physical security, status and position, or wealth*, and (3) *the inability of the kinship unit to serve as an effective vehicle for personal security or advancement*.

The first condition is more or less self-evident. A client affiliates with a patron by virtue of the patron's superior access to important goods and services. This inequality is an expression of a stratification system which serves as the basis for vertical exchange. Classically in Southeast Asia, the patron has depended more on the local organization of force and access to office as the sinews of his leadership than upon hereditary status or land ownership. Inequalities were thus marked, but elite circulation tended to be comparatively high. With the penetration of colonial government and commercialization of the economy, land ownership made its appearance (especially in the Philippines and Vietnam) as a major basis of patronage. At the same time access to colonial office replaced to some extent victory in the previously more fluid local power contests as the criterion for local patronage. Although land ownership and bureaucratic office have remained two significant bases of patronship in postcolonial Southeast Asia, they have been joined—and sometimes eclipsed as patronage resources—by office in political parties or military rank.

If inequities in access to vital goods were alone sufficient to promote the expansion of pa-

⁸⁴ My use of the term is adapted from Barnes, p. 117.

tron-client ties, such structures would predominate almost everywhere. A second, and more significant, condition of patron-client politics is the absence of institutional guarantees for an individual's security, status, or wealth. Where consensus has produced an institutionalized means of indirect exchange—one that is legally based, uniformly enforced, and effective—impersonal contractual arrangements tend to usurp the place of personal reciprocity. A patron-client dyad, by contrast, is a *personal* security mechanism and is resorted to most often when personal security is frequently in jeopardy and when impersonal social controls are unreliable. In this context, direct personal ties based on reciprocity substitute for law, shared values, and strong institutions. As Eric Wolf has noted, "The clearest gain from such a (patron-client) relation . . . is in situations where public law cannot guarantee adequate protection against breaches of non-kin contracts."³⁵

It is important to recognize the unenviable situation of the typical client in less developed nations. Since he lives in an environment of scarcity, competition for wealth and power is seen as a zero-sum contest in which his losses are another's gain and vice-versa.³⁶ His very survival is constantly threatened by the caprice of nature and by social forces beyond his control. In such an environment, where subsistence needs are paramount and physical security uncertain, a modicum of protection and insurance can often be gained only by depending on a superior who undertakes personally to provide for his own clients. Operating with such a slim margin, the client prefers to minimize his losses—at the cost of his independence—rather than to maximize his gains by taking risks he cannot afford. When one's physical security and means of livelihood are problematic, and when recourse to law is unavailable or unreliable, the social value of a personal defender is maximized.

The growth of strong, institutional orders that reduce the need for personal alliances was a rare occurrence—the Roman and Chinese imperial orders being the most notable exceptions—until the 19th and 20th centuries, when modern nation-states developed the technical means to impose their will throughout their territory. Before that, however, the existence of a fair degree of local autonomy was inevitable,

given the limited power available to most traditional kingdoms. The greater that autonomy, or what might be called the *localization* of power, the more decisive patron-client linkages were likely to be. In settings as diverse as much of Latin America, feudal Europe, and precolonial Southeast Asia, the localization of power was pervasive and gave rise to networks of patron-client bonds. From time to time in Southeast Asia a centralizing kingdom managed to extend its power over wide areas, but seldom for very long or with a uniform system of authority. A typical Southeast Asian kingdom's authority weakened steadily with increasing distance from the capital city. Beyond the immediate environs of the court, the ruler was normally reduced to choosing which of a number of competing petty chiefs with local power bases he would prefer to back.³⁷ Such chiefs retained their own personal following; their relationship to the ruler was one of bargaining as well as deference; and they might back a rival claimant to the throne or simply defy demands of the court when they were dissatisfied with their patron's behavior. Thus, the political structure of traditional Southeast Asia favored the growth of patron-client links, inasmuch as it was necessary for peasants to accommodate themselves to the continuing reality of autonomous personal authority at almost all levels.

The localization of power is in many senses as striking a characteristic of contemporary as of traditional Southeast Asia. As Huntington aptly expressed it, "The most important political distinction among countries concerns not their form of government but their degree of government."³⁸ Many of the outlying areas of Southeast Asian nations, particularly the upland regions of slash-and-burn agriculturalists, are only intermittently subject to central government control and continue to operate with much autonomy. By far the most important manifestation of the localization of power, however, has occurred within the very bureaucratic and political institutions that are associated with a central state. The modern institutional framework is a relatively recent import in Southeast Asia; it finds minimal support from indigenous social values and receives only sporadic legal enforcement. With the exception of North Vietnam and Singapore, where a portion of the intelligentsia with modernizing ide-

³⁵ Wolf, "Kinship, Friendship, and Patron-Client Relations . . .," p. 10.

³⁶ In this connection, see my *Political Ideology in Malaysia* (New Haven: Yale University Press, 1968), chapter 6; and for zero-sum conceptions among peasants, see George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist*, 65 (April, 1965), 293–315.

³⁷ See, for example, Edmund R. Leach, "The Frontiers of Burma," *Comparative Studies in Society and History*, 3 (October, 1960), 49–68.

³⁸ Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 3.

ologies and popular backing have taken power, these new institutions do not command wide loyalty and must therefore fight for survival in a hostile environment. The net effect of this fragile institutional order is to promote the growth of personal spheres of influence within ministries, administrative agencies, and parties. Sometimes the vertical links are strong (e.g., Thailand) and sometimes a high degree of decentralization or "sub-infeudation" occurs (as in parliamentary Burma from 1955 to 1958). In either case, what replaces the institution are elaborate networks of personal patron-client ties that carry on more or less traditional factional struggles rather than operate as agents of a hierarchical organization. Patron-client politics are thus as much a characteristic of faction-ridden central institutions as of the geographical periphery in these nations.

The third condition under which patron-client bonds remain prominent relates directly to the capacity of such ties to foster cooperation among nonkin. As a mechanism for protection or for advancement, patron-client dyads will flourish when kinship bonds alone become inadequate for these purposes.

Although kinship bonds are seldom completely adequate as structures of protection and advancement even in the simplest societies, they may perform these functions well enough to minimize the need for nonkin structures. Such is the case among small isolated bands of hunters and gatherers, among self-sufficient, corporate lineages and within corporate villages.³⁹ None of these conditions, however, is particularly applicable to Southeast Asian societies. The highland areas are inhabited by poorly integrated minorities but only rarely are these minorities so isolated as to lack economic and political ties with the larger society. Corporate lineages, outside traditional Vietnam, are uncommon in low-land Southeast Asia where bilateral kinship systems lead to overlapping kindreds rather than mutually exclusive lineages. Finally, corporate village structures (except in Java and perhaps Vietnam's Red River delta) are not typical of Southeast Asia. The scope for nonkin ties in general, and patron-client links in particular, has thus been quite wide throughout the region.

Even when government did not impinge

much on their activities, villagers in traditional Southeast Asia still had need of extrakin and extravillage contacts. They needed to secure marriage partners, to assure themselves protection and contacts for the limited but vital trade carried on between villages, and finally to establish an outside alliance in case a village quarrel forced them to seek land and employment elsewhere.⁴⁰ If vertical dyadic ties were of some value in the traditional context, they assumed a more decisive role in the colonial and postcolonial periods. First, the commercialization of the economy and the growth of markets enhanced the value of cooperative arrangements among nonkin. Both corporate kin groups and corporate village structures had depended on a certain level of economic autarchy for their vitality—an autarchy which colonial economic policy quickly eroded. These corporate structures (where they existed) tended "to lose their monopoly over resources and personnel in situations where land and labor became free commodities."⁴¹ As the communal land controlled by the village dwindled, as outsiders came increasingly to own land in the village, and as villagers increasingly worked for nonkin, the value of patron-client links increased for all concerned.

In traditional Southeast Asia, as in feudal Europe, then, the inability of kindreds to provide adequate protection and security fostered the growth of patron-client structures. The limited effectiveness of kindreds as units of cooperation and security was further reduced by the new structures and uncertainties of the colonial economy. Within this new economy, the goals of wealth, protection, power, and status could not be realized without outside links to nonkin (and often nonvillagers), and the establishment of these links, for the most part, followed the patron-client model.

The relative decline in the protective capacity of kindreds (which, given the absence of strong, predictable new institutions would widen the scope of patron-client ties) accelerated the *political* transformation of the colonial and postcolonial period as well. Both administration and electoral politics created new political units that did not generally coincide with the kindred or with the traditional village. As

³⁹ Corporate villages are included here since they generally stress shared kinship links to a common ancestor. Part of the corporate character of the Javanese village was perhaps further reinforced as a consequence of the collective exactions required by Dutch colonial policy. "Sanctioned reciprocity" is probably a better term for village structures in Java and Tokin than "corporate."

⁴⁰ Richard Downs, "A Kelantanese Village of Malaya," in Julian H. Steward, ed., *Contemporary Change in Traditional Societies*, Vol. II: *Asian Rural Societies* (Champaign-Urbana, Illinois: University of Illinois Press, 1967), p. 147.

⁴¹ Wolf, "Kinship, Friendship, and Patron-Client Relations . . .," p. 5. For a brilliant account of the same process in England, see Karl Polanyi, *The Great Transformation* (Boston: Beacon Press, 1957).

Gallin has shown for electoral politics in Taiwan, the political vitality of the corporate lineage is sapped by changes in the governing unit which no longer permit a single lineage to dominate. The lineage thus loses much of the material basis of its previous solidarity, and new dyadic ties become the means by which winning coalitions are built in the new unit.⁴² Political consolidation, like economic consolidation, beyond minimal kin and village units can thus enlarge the potential role of such non-kin structures as patron-client clusters.

Considering all three criteria, Southeast Asian states, like most traditional nations, satisfy most conditions for the survival of patron-client structures as a common means of cooperation. First, the disparities in power and status that form the basis of this kind of exchange have, if anything, become more marked since the colonial period. Second, nonkin structures of cooperation have always been important in the complex societies of Southeast Asia and have become more significant because of the new economic and political dependencies introduced by colonialism and nationhood. Finally, with the possible exceptions of Singapore and North Vietnam, the nations of Southeast Asia have not developed strong modern institutions which would begin to undermine purely personal alliance systems with impersonal guarantees and loyalties.

At this point in the argument, it is essential to show how patron-client structures, as one form of vertical cleavage, coexist with communalism, another form of vertical cleavage, in Southeast Asia. If loyalty to an ethnic or religious group is particularly strong it will mean that the only possible partners in most patron-client dyads will be other members of the same community. Since the community is a categorical group which excludes some possible dyadic partnerships, it represents a different form of cleavage from patron-client links. Vertical dyadic bonds can, nonetheless, coexist with communal cleavage in at least three ways: 1) as intercommunal patron-client ties above corporate communities; 2) as intercommunal patron-client ties above factionalized communities, and, 3) as intracommunal patron-client structures. First, when communal groups do deal corporately with the outside—as quite a few small, highland tribes do in Southeast Asia—we may get patron-client ties that join their leaders, as clients, to regional or national leaders. If two distinct corporate communities were

linked through their leaders who were clients of an outside leader, the structure could look like that in Figure A.

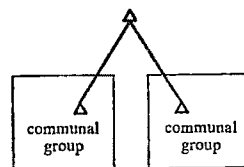


Figure A

More often in Southeast Asia, however, the second situation prevails in which a number of *patrons with separate followings within the same communal group* compete for the most advantageous links to the outside. A simple representation of this pattern is presented in Figure B, in which two patrons are linked as

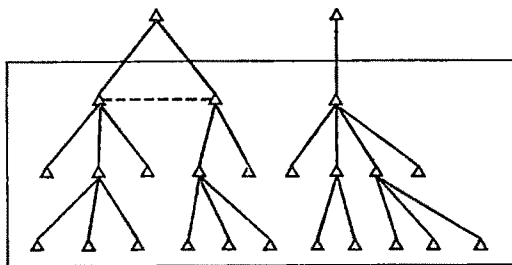


Figure B

clients to an outside patron and thereby have established a working alliance against a third patron in the same communal group who is linked to a different outside leader. Here, the communal group is rent by factionalism and has multiple ties to the outside world.⁴³ The vertical links outside the communal group, however, are likely to be somewhat weaker or more tentative than links within the community. This is so because all competing subordinate patrons and their clientele fall within a communal unit which shares a potentially strong interest; if the communal group as a whole were threatened, the shared parochial links would serve as the basis for a unity that might supersede any exterior patron-client links. The situation described in Figure B is only likely to arise, then, if there are no salient

⁴² Bernard Gallin, "Political Factionalism and Its Impact on Chinese Village School Organization in Taiwan," in Swartz, ed., pp. 377-400.

⁴³ A combination of situations one and two would occur when the tacit rules within a communal group allowed patron-client conflict but forbade the losing or weaker patrons within the communal group from maintaining ties to outside leaders.

collective threats to the communal group as a whole.

The mixture of communalism and patron-client structures portrayed in Figures A and B focuses on the extracommunal patron-client links that achieve a measure, however weak, of intercommunal integration. A third mixture of communal and dyadic association focuses instead on intracommunal politics alone. This would be represented by just the boxed portion of Figure B, which indicates that, even if communal conflict is widespread, it may well be that the *intracommunal* politics of each contending group is best described by the patron-client model.

The salience of communal feeling, especially in Malaysia, Burma, and Laos, but also in Indonesia and Vietnam makes such mixtures of communalism and patron-client politics common. Except at the apex of the political structure where a leader may have leaders of smaller communal groups as his clients, most patrons have followings that are almost exclusively drawn from their own community. Intercommunal integration tends to take place near the apex of the political structure with the base of each communal pyramid remaining largely separate. The links that represent this integration tend, moreover, to be fragile and to disintegrate in the face of a communitywide threat. Both communalism and patron-client links share the political stage, but patron-client structures are most prominent in periods of peace and stability. In addition, the process of politics *within* each communal group—in effect holding communal affiliation constant—is usually best analyzed along patron-client lines. In nations such as Thailand, the Philippines, and Cambodia, which are comparatively homogeneous culturally, there are few communal barriers to the proliferation of patron-client linkages. Thus, the patron-client model can be applied to those nations in its “pure” form since communal affiliation is not important in creating discontinuous patron-client networks.

B. The Transformation of Traditional Patron-Client Ties

1. The General Trend. The typical patron in traditional Southeast Asia was a petty local leader. Unlike the representative of a corporate kin group or a corporate village structure (rare outside Vietnam and Java, respectively), the local patron owed his local leadership to his personal skills, his wealth, and occasionally to his connections with regional leaders—all of which enhanced his capacity to build a personal following. The fortunes of such petty leaders

waxed or waned depending on the continuing availability of resources and spoils which served to knit together a following. Perhaps the most striking feature of local patron leadership in Southeast Asia was its fluidity and instability which contributed to a relatively high rate of local elite circulation. In contrast to India where hereditary office-holding and landholding provided somewhat greater continuity, the typical local leader in Southeast Asia had put together many of the necessary resources of wealth, force, connections, and status on his own and could probably only promise his son a slight advantage in the next round. Two important reasons for this oscillation in local power are a) the weakness of the central state, which lacked either the force or durability to sustain and guarantee the continuation of local power elites, and b) the relative ease with which clients in a slash-and-burn economy could, if dissatisfied simply move to another area, thus undermining their ex-patron's basis of power.⁴⁴

Patron-client systems have survived—even flourished—in both colonial and postindependence Southeast Asia. There have been important changes, however. New resources for patronage, such as party connections, development programs, nationalized enterprises, and bureaucratic power have been created. Patron-client structures are now more closely linked to the national level with jobs, cash, and petty favors flowing down the network, and votes of support flowing upward. In the midst of this change, old style patrons still thrive. Highland leaders, for example, still operate in a personal capacity as patron/brokers for their people with lowland leaders. Landowners in the Philippines and elsewhere have used their traditional control of land and the tenants who farm it to win positions of local or regional party leadership. Whatever the particular form they take patron-client networks still function as the main basis of alliance systems among nonelites throughout Southeast Asia.

The nature of patron-client bonds within Southeast Asia has varied sharply from one period to the next and from one location to another. Different resources have risen or plummeted in value as a basis of patronage depending upon the nature of the political system:

(See table on next page.)

⁴⁴ See, for example, Edmund R. Leach, *The Political Systems of Highland Burma* (Cambridge: Harvard University Press, 1954), and J. M. Gullick, *Indigenous Political Systems of Western Malaya*, London School of Economics Monographs on Social Anthropology #17 (London: University of London/Athlone Press, 1958).

Secular Trends in the Nature of Patron-Client Ties in Southeast Asia

<i>Quality</i>	<i>Traditional</i>	<i>Contemporary</i>
1. Duration of bond	more persistent	less persistent
2. Scope of exchange	multiplex	[increasingly] simplex
3. Resource base	local, personal	external links, office-based
4. Affective/instrumental balance	higher ratio of affective to instrumental ties	lower ratio of affective to instrumental ties
5. Local resource control	more local monopoly	less local monopoly
6. Differentiation between clusters	less differentiation	more differentiation
7. Density of coverage	greater density	less density

The capacity to mobilize an armed following was particularly valuable in the precolonial era; access to colonial office was a surer basis of patronage than armed force in the colonial period; and the ability to win electoral contests often became the central resource with the advent of independence. Not only have resource bases proved mercurial over time, but the nature of patron-client ties in the indirectly ruled highland areas has remained substantially different from lowland patterns. Amidst this variety and change, it is nevertheless possible to discern a number of secular trends in the character of patron-client bonds. Such trends are far more pronounced in some areas than others, but they do represent directions of change that are important for our analysis.

(1) In comparison with more bureaucratic empires, patron-client bonds in precolonial Southeast Asia were not, as I have pointed out, markedly persistent. With the quickening of social change brought about by the commercialization of the economy and the penetration of the colonial state into local affairs, however, a patron's resource base became even more vulnerable to the actions of outside forces over which he had little or no control. It was an ingenious patron indeed who could survive the creation of the colonial state, the export boom, the depression of the 1930s, the Japanese occupation, and independence with his resources and clientele intact. The major exception to this trend was the colonial period in indirectly ruled areas where colonial military and financial backing of traditional rulers, if anything, brought a stability—or stagnation—to political systems that had been more chaotic. Elsewhere, patron-client links tended to become more fragile and less persistent.

(2) With the differentiation of the economy and its effects on the social structure, the scope of exchange between patron and client tended to narrow somewhat. Where traditional patrons could generally serve as all purpose protectors, the newer patron's effectiveness tended to be more specialized in areas such as political influ-

ence, modern sector employment, or administrative influence. Although patron-client ties remained flexible and personal, the more limited capacities of the patron tended to make relationships less comprehensive and hence less stable.⁴⁵

(3) The traditional patron for the most part operated with personally controlled local resources. One effect of the colonial period—and independence as well—was to increase radically the importance of external resources for local patronage. A following based on purely local office or landholding was seldom sufficient to sustain a patron in a new environment where schools, agricultural services, regional banks, and public employment represented competing sources of patronage. The growing role of outside resources, in most cases, thus led to competition among patrons, each of whom recruited followings with the particular resources at his command.⁴⁶ In addition, since those who controlled the new resources were generally office-holders subject to transfers or political changes at the center, the new patrons were less secure than older patrons and probably more inclined to maximize their gains over the short run.

(4) Because the new patron-client ties were weaker and less comprehensive, and because the new patrons were often from outside the local community, the instrumental nature of the exchange became more prominent. A relationship that had always involved some calculations of advantage lost some of its traditional legitimacy and grew more profane. Patron-client exchanges became more monetized, calculations more explicit, and concern centered

⁴⁵ Again, indirectly ruled areas were often exceptions in that local rulers tended to take on new powers under the colonial regime and thus became more comprehensive patrons than in the past.

⁴⁶ For Malaysia, M. G. Swift, *Malay Peasant Society in Jelebu* (London: University of London, 1965), pp. 158–60 captures this shift in local power. A general treatment of such changes is contained in Ralph W. Nicholas, "Factions: A Comparative Analysis," in M. Banton, general ed., *Political Systems and the Distribution of Power*, Association of Applied Social Anthropology Monograph #2 (London: Tavistock Publications, 1965), pp. 21–61.

more on the rate of return from the relationship rather than on its durability. This trend meant that newer patron-client clusters were likely to have a comparatively large "fair-weather" periphery, a comparatively small core-following, and a less "constant" patron as well.

(5) The breakdown of local patron monopolies follows logically from most of the changes we have already discussed. Where one local landowner or traditional leader had once dominated he now faced competitors who might be local administrators of state welfare in loan programs, teachers in new secular schools, a local trader or businessman, or the resident manager of a foreign-owned plantation. Factional strife which reflects this competition was most common in villages where socioeconomic change and government penetration had been far-reaching, and less common in more traditional areas.⁴⁷

(6) As differentiation occurred within the local societies, they gave rise to patron-client clusters that were distinct. A bureaucrat might have a following primarily within his agency, a businessman among his laborers, and a landowner among his tenants. This process of differentiation among clusters provided the potential basis for durable group interests inasmuch as many clusters now had an institutional distinctiveness.

(7) While the changes we have examined may have assisted the vertical integration of patron-client pyramids, they tended to reduce the universality of coverage. That is, more and more people in the new market towns and cities, on plantations, and on small plots they rented from absentee landlords were no longer attached—or were very weakly attached—to patrons. These new elements of the population varied greatly in their interests and their levels of organization, but, in any event, they fell outside the older patron-client network.

⁴⁷In his study of politics in an Indonesian town, Clifford Geertz has shown that the more traditional hamlets were more likely to be united under a particular leader than were hamlets which had changed more; *The Social History of an Indonesian Town* (Cambridge: M.I.T. Press, 1965), Chapter 6. This finding is corroborated by Feith's study of the 1955 Indonesian elections; Herbert Feith, *The Indonesian Elections of 1955*, Interim Report Series, Modern Indonesia Project (Ithaca: Cornell University, 1961), pp. 28–30. A comparative study of two Burmese villages also supports this conclusion: cf. Manning Nash, *The Golden Road to Modernity* (New York: Wiley, 1965). In this context, directly ruled lowland areas tended to develop factional competition among different patrons, while less directly ruled areas (especially highland areas) more frequently retained some unity behind a single patron who remained their broker with the outside world.

While some long run trends in patron-client ties seem clear, it is difficult to say anything about the balance between voluntarism and coercion over time. On the one hand, changes in the economy have made clients less autonomous and more dependent on patrons for protection against a fall in world prices, for cash advances before the harvest, and so forth. Also contributing to a decline in the client's bargaining position is the imported legal system of property guarantees which allow a wealthy man, if he so chooses, to resist pressures for redistribution that operated in a traditional setting. On the other hand, the breakdown of local patronly monopolies and the exchange resources that electoral systems often place in the hands of clients work in the opposite direction. Given these contradictory tendencies, one can draw the tentative conclusion that patron coerciveness has declined only where extra-local resources and competitive elections are common and has elsewhere either increased or remained the same.

In general, patron-client ties have tended to become more instrumental, less comprehensive, and hence less resilient. They still represented diffuse personal bonds of affection when compared to the impersonal, contractual ties of the marketplace, but the direction of change is eroding their more traditional characteristics. Even this supple traditional protective mechanism has had to pay a certain price to survive in the midst of a nation-state with a commercialized economy. The durability and legitimacy of the patron-client tie was best served when all of a client's dependencies were focused on a single patron. But, as Godfrey and Monica Wilson have shown, this situation is less and less likely since the process of modernization tends to create multiple dependencies—each of less intensity—rather than concentrating dependence on one person.⁴⁸ The slowly weakening comprehensiveness of the link is, ultimately, what undermines its sanctity and legitimacy for the client.

2. The Dynamics of the Transformation. The engine behind the shift in patron-client ties was largely provided by the penetration of the local arena by an intrusive national economy and rational political system. This penetration wrought two major changes that transformed patron-client links: a) during the colonial period, especially, it impaired the effectiveness of local redistributive pressures and b) partic-

⁴⁸G. and M. Wilson, *The Analysis of Social Change*, pp. 23, 40.

ularly after independence it "nationalized" access to patronly resources, thus creating new bases of patronage and devaluating old ones.

Traditional peasant societies, operating in an economy of great scarcity in which one family's gain is another's loss, have generally developed a variety of social control mechanisms that guarantee a measure of security to each family and temper the centrifugal forces generated by the struggle for subsistence.⁴⁹ These mechanisms commonly involve forcing anyone who has accumulated considerable wealth to redistribute a portion of it. A wealthy man is pressed to assume expensive ceremonial offices, to make large religious contributions, to give loans and donations, and so forth. He trades his wealth for prestige, and, by so providing for at least the minimum well-being of others, he becomes a legitimate patron with a personal entourage of those obligated to him.

The central fact about these redistributive mechanisms, however, is that they operate by virtue of a local power situation. That is, the wealthy man in a peasant village can seldom rely on outside force or law to protect him; instead, his wealth and position are ultimately validated by the legitimacy he acquires in the local community. Unless a wealthy individual can persuade most of the community that his wealth is no threat to them or can win enough personal allies to sustain his position, he is in danger. Colonialism, however, broke the relative autonomy of the local arena and hence weakened many of the community's redistributive pressures. Supported in effect by the power of the colonial regime to enforce its notion of law, the patron could increasingly ignore local levelling pressures. If he lost much of the social approval he previously enjoyed, he had gained an outside ally with the power to guarantee his local position. The colonial power situation thus offered the *older* patron new leverage in the local arena—leverage which was further strengthened by the growing complexity of colonial society. As Blau has explained,

Social approval has a less pervasive significance as a restraining force in complex societies than in simpler ones, because the multiplicity of groups

and the possible mobility between them in complex societies allows deviants of nearly all sorts to escape from the impact of community disapproval by finding a sub-group of likeminded persons....⁵⁰

Absentee landlords, the new urban wealthy, and minority communities (who were relatively impervious to local social approval so long as they had colonial backing) were *new* elements in colonial society which could escape patronly obligations. The colonial system thus tended to allow existing patrons greater latitude for exploitation while producing a class of wealthy nonpatrons.

If the intrusion of external power could strengthen the hand of an existing patron, it could also create a resource base for the rise of new patrons. The activities of the colonial regime included the hiring, firing, and promotion of public employees, the dispensing of contracts, and the granting of licenses and permits, all of which could be used to create a personal following. With independence, not only did local leaders take over responsibility for all these decisions, but the scope of government activity and regulation was generally expanded into new areas such as community development. The survival or demise of a local patron often depended, as Geertz has shown, on how successful he was in tapping these new bases of power.⁵¹

Except for the rare local patrons—especially in indirectly ruled areas—who were able to monopolize these external resources, the new situation produced more competition and mobility among patrons. Many potential clients quickly discovered that their needs were best served by a patron who had access to the institutions which controlled the use of these external resources. In any local context this shift could be measured by the rise of new patrons who were wholly or partly based in these new structures. Studying the incorporation of Sardinia into the Italian nation-state in the 20th century, Alex Weingrod has documented the growing importance of such externally based patrons.⁵² The proportion of outsiders asked to be godparents, for example, increased dramatically from 1920 to 1960, and patrons with links to the ruling party and the state bureaucracy had increased their followings at the expense of traditional

⁴⁹ For a description of such mechanisms see Clifford Geertz, *Agricultural Involution* (Berkeley: University of California Press, 1963); George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist* 67, 2 (April, 1965), pp. 293-315; Swift, *Malay Peasant Society* . . . , and Mary B. Hollnsteiner, "Social Control and Filipino Personality," *Symposium on the Filipino Personality* (Malcat: Psychological Association of the Philippines, 1965), p. 24.

⁵⁰ Blau, *Exchange and Power in Social Life*, p. 114.

⁵¹ Geertz shows how local leaders often managed to become agents of the local sugar mills—buying crops, renting land, and recruiting labor and thereby enlarging their power in the community. *The Social History* . . . , p. 57.

⁵² Alex Weingrod, "Patrons, Patronage, and Political Parties," pp. 388, 397.

landholders. A similar process has occurred in Southeast Asia as the integration of villages into a national economy and political system tended to produce a number of more specialized local patrons who often became factional leaders.

Most of the transformations in patron-client bonds that we have been discussing apply with greatest force to the directly ruled, lowland areas of Southeast Asia where the colonial impact was both swift and far-reaching, and where colonial officials more thoroughly replaced indigenous leaders. In the indirectly ruled areas—such as highland Burma, the Unfederated States of Malaya, most of Indonesia's Outer Islands, Cambodia, Laos (and perhaps Thailand belongs here as a limiting case of indirect colonial influence)—these generalizations must be qualified. To ease the financial and administrative burden of colonial rule in these areas, the colonizers generally kept local rulers in place and used them as agents. Since these were by and large peripheral areas of marginal commercial interest, the pace of economic change tended to be slower as well.

The effects of this policy on patrons, in contrast to the directly ruled regions, were twofold. First, local patron/leaders tended to be strengthened by colonial backing and the new powers given to them. What had probably been a fairly unstable and minimal chieftaincy now became a local regime stabilized and extended by the colonial power. Secondly, the sanction of colonial authority permitted many such leaders to broaden the resource base of their authority.⁵³ It is true of course that a local patron's new source of strength entailed some threat to his legitimacy, but since the colonial regime demanded little beyond the maintenance of law and order in those areas, it was seldom crippling. On the other hand, the anointed patron now had the means to eclipse his rival patrons. He not only had his traditional authority and the discretionary administrative powers given him by the colonial regime, but he could use his power to purchase land, control local trade, and act as the commissioned agent of private firms. Frequently, then, the local ruler gained a new lease on political life as the dominant local figure owing to his wealth, his administrative power, and a measure of tradi-

tional legitimacy. Given the slower pace of economic change and state penetration in these regions, the local leader had to contend less with new competitors who flourished amidst such changes. The strength many patrons achieved under indirect rule is nowhere more apparent than in postcolonial elections in which they often could deliver most of their region's vote.

3. Electoral Politics and Patron-Client Ties. Most Southeast Asian states have had functioning electoral systems at one time since their independence. Although only the Philippines retains a parliamentary system, the electoral studies that do exist can tell us much about the effects of party competition on patron-client bonds and, beyond that, highlight some of the unstable features of patron-client democracies.

The dynamics of electoral competition transformed patron-client relations in at least four important ways: (1) it improved the client's bargaining position with a patron by adding to his resources; (2) it promoted the vertical integration of patron-client structures from the hamlet level to the central government; (3) it led to the creation of new patron-client pyramids and the politicization of old ones; and (4) it contributed to the survival of opposition patron-client pyramids at the local level.

First, with popular elections the client gained a new political resource, since the mere giving or withholding of his vote affected the fortunes of aspirants for office. Nor were voters slow to realize that this resource could be turned to good account. Even someone with no other services of value to offer a patron found that the votes of his immediate family were often sufficient to secure the continuous assistance of a local politician. This pattern could be found throughout Southeast Asia in electoral situations but is most striking in the Philippines, where most patron-client ties are centered around landholding and elections. The Filipino politician, as Wurfel points out, does favors *individually* rather than collectively because he wishes to create a personal obligation of clientship.⁵⁴ The voter, for his part, asks that his patron/politician favor him because of a personal obligation to reciprocate.

In one sense, popular elections can be seen as a reestablishment of the redistributive mechanisms of the traditional setting. Once again a patron's position becomes somewhat more dependent on the social approval of his community—a social approval that is now

⁵³ Similar assessments of the effects of indirect rule can be found in M. G. Swift, pp. 148–149; Harry J. Benda and John Bastin, *A History of Modern Southeast Asia* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), pp. 75–122. The best accounts of the pattern, however, come from India. Cf. Bailey, *Politics and Social Change*, Chapter 4, and Paul R. Brass, *Factional Politics in an Indian State* (Los Angeles and Berkeley: University of California Press, 1965), Chapter 4.

⁵⁴ David Wurfel, "The Philippines," in *Comparative Studies in Political Finance: A Symposium, Journal of Politics*, 25 (November, 1963), 757–773.

backed by the power to defeat him or his candidate at the polls. Unable to depend on outright coercion, and faced with competitors, the electoral patron knows he must (unless his local economic power is decisive) generally offer his clients better terms than his rivals if he hopes to maintain his local power.

Second, nationwide elections make it necessary for a national party to establish a network of links extending down to the local level. For the most part a party does this by taking advantage of existing patron-client clusters and incorporating them into its structure. The competitive struggle of Indonesian parties to forge such links in Java during the 1950s is apparent in the accounts Feith and Geertz give of electoral campaigns.⁵⁵ They agree that effective campaigning in the village took the form of activating and politicizing preexisting personal links rather than mass meetings or policy stands. The campaign was, as Feith says

... a race for a foothold in these villages ... a foothold involving allegiance of as many as possible of their influential people. Here the first step was to secure the support of those whose authority was accepted by the village prominent. Thus the parties struggled with one another for influence with the bupatis, the wedanas, and the tjamat ... with the local military commanders and the heads of local offices of Religion, Information, and Mass Education, kijaijis, ulamas, heads of clans, old guerrilla leaders. . . .⁵⁶

As elsewhere in Southeast Asia, a party succeeded best at the polls by securing the adhesion of the important local patrons, who would deliver their clients as a matter of course. Working on voters individually or by class affiliation made little sense when most of the electorate was divided into patron-client clusters. The affiliation of patrons was often gained by making them candidates, by promising them jobs or other patronage, or even by cash payments. It is clear, however, that, in comparison with those parties who had to create new links to village leaders, parties such as the Nahdatul Ulama which could rely on bonds that antedated the election, were in a stronger position. Nash's account of the 1960 election in upper Burma reveals a similar pattern of patron mobilization. When a local patron was approached to join U Nu's faction of the AFPFL on the promise of later patronage, he was able to get thirty-nine others—his relatives and those who owed him money or for whom he had done fa-

vors, i.e., his clients—to join as well.⁵⁷

The nature of the new exchange relationship that gives vitality to this patron-client pyramid is similar in most electoral systems. The local patrons and their clients provide votes at election time, hopefully carrying the village, while the party undertakes to help its local adherents (through their patron) with jobs, help in dealing with the bureaucracy, providing public works, and so forth. Since the winning party can generally offer more support to local allies than the opposition can, local patrons are likely to display a "bandwagon effect," switching allegiance to a probable winner. In addition, the party's need for a powerful local base is likely to lead to a certain localization of power. In return for delivering local votes for its list, the party is likely to give its local patron a wide discretion in administrative and development decisions affecting the locality. Thus many local patrons are able to entrench themselves further as dominant figures.

A third consequence of elections for the patron-client structure is to promote the expansion of patron-client ties and the politicization of existing bonds. Knowing that an electoral victory is important, a local patron with a modest following will probably try to obligate more clients to him in order to strengthen his electoral position. Patrons who have previously been politically inactive would "immediately convert their private power such as control over sharecroppers, debtors kinsmen, neighbors, etc., into public political power in the form of votes."⁵⁸ Given these tendencies, the patron-client structures in a given community are most evident immediately before an election, especially a hotly contested one, when the contestants attempt to activate any links that might advance their cause.

A final point about the impact of elections on patron-client structures is that they tend to heighten factionalism and unless one cohesive party completely dominates, to promote the survival of local opposition factions. In most traditional settings, patron rivalry was largely limited to the local arena so as not to invite external intervention. An electoral system, by contrast, creates rival national or regional parties which need allies at the local level. A weak faction that might previously have been forced to compose its differences with a dominant faction, can now appeal for external support. Many of these external allies are able to provide their local adherents with patronage, cash,

⁵⁵ Feith, *The Indonesian Elections of 1955*; Geertz, *The Social History of an Indonesian Town*.

⁵⁶ Feith, p. 79.

⁵⁷ Manning Nash, "Party Building in Upper Burma," *Asian Survey*, 3 (April, 1963), pp. 196-202.

⁵⁸ Nicholas, "Factions . . .," p. 45.

or other favors so as to maintain a local foothold. The net effect of electoral competition is thus to exacerbate many of the latent factional differences among patron-client clusters and occasionally to buttress weak patrons whose position would otherwise have disintegrated.

The effects we have attributed to elections can be compared to the situation in Thailand, where elections have only rarely been any more than a device to legitimate self-selected rulers. There the local client's vote is not important enough to materially improve his bargaining position with a patron, and the vertical integration of patron-client clusters had not gone very far beyond the central institutions of the bureaucracy and armed forces. In Thai villages unlike the electoral settings of Java in the 1950s, or the Philippines, many patron-client clusters are of purely local significance and are not highly politicized. Local factional conflict, as a result, is much less striking in Thailand than where competitive elections have helped to subsidize it.

To this point we have focused on the general influence of elections on patron-client structures. Depending on the region and the party in question, however, there has been a noteworthy variation in the connection between party and patron-client structures. The essential distinction is one between a party that has created its own network of patron-client linkages from the center and a party that relies on preexisting patron-client bonds and merely incorporates them into its organization. This corresponds to René Lemarchand's distinction between types of party machines in Africa.

In situations where micro-level clientelistic structures provide the essential linkages between the party machine and the masses . . . the machine is superimposed upon, and in some ways tributary of, the clientelistic subsystem. A distinction must therefore be drawn between the more orthodox types of machine . . . in which patronage becomes the essential source of cohesion, and what one might call the neo-traditional machine, in which exchange processes between the center and periphery are mediated by, and contingent upon, the operation of traditional forms of clientelism.²⁹

As Lemarchand adds, the "orthodox" machine is more dependent on material inducements since its linkages are of more recent origin and hence more instrumental. "Neo-traditional" machines, by contrast, can rely somewhat more heavily on established patterns of deference, though they must bargain with nearly autono-

mous local patrons. This distinction has validity in Southeast Asia as well, both in accounting for the different styles of politics in the indirectly ruled, more traditional areas as compared to the directly ruled, heavily commercialized areas and also in explaining the structural differences between the more traditionalist and compared to more modern parties. In the indirectly ruled areas, political parties confronted fairly stable constellations of local patron-client power which would have been difficult to destroy. It was simpler to come to terms with local leaders rather than to try to circumvent them, even though this accommodation tended to divide the party into a coalition of political fiefdoms. In directly ruled areas where competition among patrons and heavily instrumental ties were more common, a party had a greater opportunity to create new linkages but at perhaps greater expense in favors and patronage. Throughout Southeast Asia all parties had to adapt themselves to these differences in social structure in different regions of the country: the PNI of Indonesia operated differently in Central Java than in the Outer Islands; U Nu's AFPFL faction could not win the support of the hill tribes in the same way they won the vote of the lowland Burmese, and the Nationalists in the Philippines campaigned differently in central Luzon than in Mindanao.

There were also systematic differences between the neo-traditionalist and modernist parties within a given area. In Java, for example, the Nahdatul Ulama incorporated more traditional patron-client ties than did the PNI, which could rely more on its access to material rewards and local administration. On the East Coast of Malaya, the Islamic PMIP appealed most to the more traditional peasantry while the ruling UMNO concentrated on the town population and those dependent on federal funds. One would also guess that the Ba Mwe faction of the AFPFL had a more instrumental base in towns and the modern sector, while U Nu's faction was more frequently based on existing patron-client links in the countryside. Simply knowing the kind of patron-client bonds a party had created or incorporated could reveal a great deal about the party's cohesiveness, the nature of its local base, and the extent of its reliance on material inducements.

4. The Inflationary Character of "Patron-Client Democracy." The introduction of competitive elections in Southeast Asia increased the pressures on regimes for the downward distribution of tangible benefits. In return for votes flowing up the vertical chain of patron-client struc-

²⁹ René Lemarchand, "Political Clientelism and Ethnicity: Competing Solidarities in Nation-Building," *American Political Science Review*, 66 (March, 1972).

tures, each patron depended upon the downward distribution of patronage in the form of administrative favors, land grants, public employment, and so on, in order to keep his own pyramid of followers intact. Elections, by themselves, had shifted the balance of exchange so that it favored the client somewhat more than before. The consequence of this shift in exchange terms was a greater flow of material benefits toward the base of the patron-client network.

The strength of the downward distributive pressures generated by electoral procedures in Southeast Asia depended primarily on four variables which are stated below in contrasting terms.

(See table at bottom of page.)

Each of the above variables related to the strength of incentives impelling a party to maximize its clientele and the degree to which that clientele will depend on concrete material incentives rather than ties of affection or deference. The first and obvious requirement for distributive pressures is that elections be important in the selection of an elite. Secondly, a shaky regime or party will be in a less advantageous position to resist client demands than a strong one, since an election is an all or nothing affair and uncertainty over the outcome will raise costs; when the race is close, the party in its recruitment efforts knows that the marginal value of the extra dollar, patronage job, or development grant is all the greater. That is one reason why distributive pressures were greater in 1955 in Indonesia when an election fraught with uncertainty would determine which parties would form a coalition, than in Malaysia in 1964 when the question was not whether the Alliance would win but whether or not it would take two thirds of the seats. The importance of social change (item three) is based on the observation that patron-client ties in less traditional areas typically require the patron to deliver more in the way of instrumental, material rewards. The maintenance of a loyal patron-client network in a traditional area where def-

erence is strong, will, I assume, cost a party somewhat less in material rewards and favors than will a network of the same size in a built-up area where traditional patron-client bonds have eroded. Finally, a neo-traditionalist party such as UMNO in Malaysia can, in part, rely upon the traditional legitimacy of many of its leaders while a party of "new men" such as the PNI in Indonesia or the AFPFL in Burma has to rely more often on highly instrumental ties. Thus a weak party led by "new men" and relying on votes from among an uprooted population is likely to develop a patchwork patron-client structure that is very expensive to maintain. It is indicative of just how much financial backing such structures require that only a ruling party with access to the public till can generally afford the construction costs.⁶⁰

The distributive pressures experienced by such regimes manifest themselves in familiar ways. Government budgets, and of course deficits, swell quickly with expenditures on education, growing public employment, community development projects, agricultural loans, and so forth. Particularly since votes in Southeast Asia are to be found in the countryside, one would expect that regimes with strong electoral pressures would spend more in the grass-roots rural areas than would regimes without such pressures. Given such pressures, local expenditure is also arranged as much as possible so that benefits can be distributed individually since that arrangement is more appropriate to patron-client exchange patterns. Even with pork-barrel programs a local party leader will claim personal responsibility for the gift and personally help distribute whatever employment or subcontracting it includes. The capacity of the regime to keep its network intact and win

⁶⁰ Adrian C. Mayer seems to have this distinction in mind, in his study of an Indian town comparing the "hard" campaign of the Jan Sangh, which relied on durable social ties and tried to prevent defections, and the local Congress Party, which ran a "soft" campaign of short-term links by promising favors and benefits to intermediaries. See Mayer, "The Significance of Quasi-Groups in the Study of Complex Societies," p. 106.

Distributive Pressures of Elections

A. Strongest when

1. Elections determine powerholders
2. Regime is weak, unstable regime
3. Socioeconomic change is extensive (direct rule, lowland areas)
4. Party is modernist, secular.

Examples of Strong Distributive Pressure (A): Indonesia (until 1955), Burma (until 1959 at least), the Philippines.

Examples of Weaker Distributive Pressures (B): Thailand, and perhaps Malaysia (until 1967).

B. Weakest when

1. Elections have marginal significance
2. Regime is strong, stable regime
3. Socioeconomic change is less extensive (indirect rule, highland areas)
4. Party is traditionalist, religious

elections depends on its capacity to provide rewards for the lower tiers of its structure at a constant or even expanding rate.

A regime that is dependent on its particularistic distributive capacity is also unlikely to solve its financial dilemmas either by structural reform or by tapping new sources of revenue. Most conceivable structural reforms, such as land redistribution, would strike at the resource base of many patrons and are thus unacceptable to parties whose *policy* interests coincide with the desires of its dominant patrons. Such regimes also have a most difficult time raising revenue from internal taxation. A rise in direct taxation would threaten their base of support; and, in fact, they are notorious for the under-collection of revenues due them, since favors to their clients often take the form of either leaving them off local tax rolls or ignoring debts they owe the government. The Burmese peasants connected to U Nu's faction of the AFPFL, for example, were almost universally in default on agricultural loans they had received as party supporters. They assumed the loan was a gift for clientship and knew that a government dependent on their votes could scarcely press matters.

If this analysis is correct, regimes under intense distributive pressures will characteristically resort to budget deficits, especially in election years, to finance their networks of adherents. Their reliance on heavily instrumental and highly monetized patron-client ties will also make it difficult for them to avoid running down their foreign exchange reserves to maintain their strength at the polls. The division of expenditures within the budgets of such regimes should also reveal a heavy emphasis on distributive expenses at the local level. Empirical studies of budget distribution, budget deficits, and foreign exchange expenditures over time in parliamentary Burma, Indonesia, and

the Philippines, when compared with similar statistics for nonparliamentary periods in these same countries, or say, with statistics from Thailand and Malaysia should confirm this prediction.⁶¹

Democratic regimes which must cater to the strong distributive pressures generated by their electoral clientele are thus particularly vulnerable to the vagaries of world prices for primary products on which their budgets depend. As long as the economy expanded and world prices were buoyant, they could afford the costs in public jobs, pork-barrel projects and loan programs to solidify and expand their huge patron-client network. But a stagnating economy or declining world prices threatened the entire structure they had pieced together, since it relied so heavily on material inducements and relatively little on affective ties. In this context it may be that the collapse of Korean war-boom prices for primary exports was the crucial blow to democracy in Indonesia and Burma. The Philippines may narrowly have escaped a similar fate by virtue of their longer and more legitimate democratic tradition, as well as by their not having suffered as proportionately large a loss in foreign exchange. Malaysia was less vulnerable, since she had just become independent and her strong government faced only moderate distributive pressures, while the Thai military elite was even less reliant on its distributive performance. The political stability or instability of parliamentary forms in these nations in the late 1950s was thus strongly affected by the strength of distributive pressures fostered by these political systems in the mid 1950s.

⁶¹ Many of these data are not in a form that permits easy comparisons. Although budget deficit and foreign exchange figures seem to fit this pattern, statistical confirmation will have to await further research.

On Anarchism and the Real World: William Godwin and Radical England

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The rhetoric of anarchism is very much with us today, be it from Robert Paul Wolff in the academy or Jerry Rubin in the streets.¹ In its radical defense of freedom, this anarchism repudiates politics and the use of political means to achieve that freedom. This is the very essence of anarchist thought, and, indeed, its most salient distinction from other revolutionary doctrines. Freedom, the anarchist holds, cannot be realized through political methods and certainly not through popular or mass action. What replaces politics for the anarchist is either education or theater. If the former, then the path to utopia is conceived of as slow and laborious, successful only in the distant future. If the latter, then the new order of freedom is forever around the corner, instantly achieved through repetitive defiant deeds and romantic acts of self-assertion even unto death and annihilation. The one form of anarchist thought is passive, resigned, and critical of premature and precipitous action; the other is activist and precipitous in the extreme. Despite their differences, however, both forms of anarchist thought share a most important belief, a common conviction of superiority. Above the mediocre, the petty, the base, the dull, and the deceived, stands the anarchist of either stripe, convinced of his superiority of intellect or feeling. No surprise, then, that as political thought anarchism has traditionally been the expression of an intellectual or artistic elite.² Like Abbie Hoffmann, the anarchist has always stood apart from the "Pig Nation," which he sees so mindlessly unaware of its misery and mediocrity.³

¹ Robert Paul Wolff, *In Defense of Anarchism* (New York: Harper & Row, 1970) and Jerry Rubin, *Do It!* (New York: Simon & Schuster, 1970).

² The history of anarchism both as movement and set of ideas has been ably presented by James Joll, *The Anarchists* (New York: Grosset and Dunlop, 1964) and George Woodcock, *Anarchism: A History of Libertarian Ideas and Movements* (New York: Meridian, World Publishers, 1962). Particularly useful as a contemporary indictment of anarchism as both elitist and counterproductive to the potential for change and reform in the real world is Benjamin R. Barber's excellent *Superman and Common Men: Freedom, Anarchy and the Revolution* (New York: Praeger, 1971), a position which this paper shares and hopefully corroborates.

³ Abbie Hoffmann, *Woodstock Nation* (New York: Random House, 1969) and *Revolution for the Hell of it*. (New York: Dial Press, 1968).

There is, one must insist, nothing new in this. For Emma Goldman, an earlier American anarchist, this elitism was itself the very essence of anarchism. Writing of Nietzsche, she insists, "(he) was not a social theorist, but a poet and innovator. His aristocracy was neither of birth nor of purse; it was of the spirit. In that respect Nietzsche was an anarchist, and all true anarchists were aristocrats."⁴

The masses, more often than not, have seen the anarchist for what he is, an arrogant, albeit sensitive, snob; and they have rewarded him accordingly. Herbert Read, England's most distinguished anarchist of this century, sadly laments this fate of the artist-anarchist at the hands of public opinion. He must "accept this thankless task: to stand apart yet to mediate." Society will never understand or love him, he must "drink with Socrates, the deadly cup."⁵ But there may well be another dimension to the popular repudiation of the anarchist beyond mere distaste for those who stand apart. It could be informed by an astute realization that the purity and/or insanity of the anarchist often serves the conservative interests of the powers that be.

All of this rings true for the founder of modern anarchist political thought. Indeed, William Godwin (1756-1836) gives to anarchist thought its peculiar ambiance and its enduring characteristics. A radical theorist, Godwin despised politics and the pursuit of change through political activity. Less interested in the-
atrics than in rational education and enlightenment, he still speaks for a literary and intellectual elite. As such he was repudiated by the leaders of the popular movements in his day. Enshrined in the sacred texts of anarchist tradition is Godwin's plea of 1793 in his *Political Justice* for "the dissolution of political government, of that brute engine, which has been the only perennial cause of the vices of mankind."⁶ Libertarian though this may sound, Godwin,

⁴ Emma Goldman, *Living My Life*, 2 vols. (New York, A. A. Knopf, 1931) I, 194.

⁵ Herbert Read, *To Hell with Culture* (New York: Schocken, 1964) p. 9.

⁶ William Godwin, *Enquiry Concerning Political Justice and Its Influence on Morals and Happiness* (Toronto, Univ. of Toronto Press, 1946) 11, 212. All future references to this work will appear with volume and page numbers in the text of this paper.

despite his philosophical commitment to freedom, was by no means a friend of reform. This divergence between the radical theorist and the conservative practitioner is evident in his views on political action and revolution found in *Political Justice* and in his public performance two years later when he sided with Pitt in the repression of the English Jacobins.

To facilitate an understanding of Godwin's anarchism and the extent to which it will mold the later tradition, the discussion here will be organized around five philosophical and political sections: (1) Godwin's destructive attack on government and institutions; (2) his positive vision of the anarchist society to replace "the brute engine government;" (3) his discussion of how to achieve this anarchist utopia, or, more importantly, how not to achieve it; (4) doctrine meeting practice, the clash between Godwin and the English Jacobins in 1795-1796; (5) speculations on the reasons for Godwin's break with the radicals and the significance of this for the future development of radical and anarchist thought. One final comment by way of introduction: Behind much of this paper are events and a scenario that will seem familiar to many readers; the 1790s were in England a time not unlike our own day. The response described here has continued to recur, so that even today we see the wasteful confrontation of elitist radicals, disdainful of politics, and popular radicals seeking political change.

The "Enthanasia of Government": Godwin's Attack on Law, Power, and the State

It is worthwhile at the outset to schematize the development of Godwin's argument. Two stages of destruction are followed by one of visionary reconstruction. The first negative stage involves an assault on the liberal tradition, carried out primarily by invoking Rousseau. Then follows the attack on law and political authority in the name of the liberal values of private judgment and individuality. There is, to be sure, some tension, incompatibility, and even contradiction, between these two destructive aspects of the argument, some of which remains and cannot be reasoned away. But much of this tension is resolved in the positive vision of anarchist society. The synthesizing agent is the principle of sincerity. In bare outline, then, these are the stages in Godwin's philosophy of anarchism. One can turn now in greater detail to each stage in the argument.

Godwin begins *Political Justice* with a root and branch attack on the Lockean natural rights tradition. His main charge is that it is too egotistical, too concerned with the individu-

al's right to do as he wills. Sacrificed in this world view, according to Godwin, are duty, justice, and concern for the common good. Writers like Locke, he contends, in their "unprofitable disquisitions . . . respecting the probable origin of government," and in investigating rights, have failed to ask what form of government was the most conducive to public welfare (I, 123). Godwin repudiates any notion of contract based on some original promise. He does this by simply repeating the arguments of Hume (I, 188; 194-95; 219). What irritates him even more is the basic liberal notion of rights, for as far as Godwin is concerned rights "are all of them superceded and rendered null by the superior claims of justice." Man has no permissive grant to do as he wishes based on some supposed ethic of self-justification. He is bound by justice to do his duty, to employ his talents, his understanding, his strength, and his time, in the production of the greatest quantity of general good. One's duty is to see that his every act is "bound to . . . the general weal, that is, for the benefit of the individuals of whom the whole is composed" (I, 166; 135; 13).

Godwin is perfectly willing to dispense with the most fundamental of liberal rights. Man, he states, has no right even to his life, "when his duty calls him to resign it." Neighbors are duty bound to deprive one another of liberty and even life, he suggests, if to do so is absolutely necessary to the prevention of greater evil. Godwin sees such calculations of duty by and large as easy. It is, after all, blatantly clear that should one's mother and Archbishop Fenelon be trapped together in a burning house the latter should be saved. Not only has Godwin thus dealt with Locke and Hobbes, he also repudiates what will be one of Mill's major liberal contentions. Men have a duty to amend the errors of anyone they may see or know, suggests Godwin. "It is absurd to suppose that certain points are especially within my province, and therefore [that] he may not afford me, invited or uninvited, his assistance in arriving at a right decision" (I, 167; 162). In his assault on the liberal obsession with private rights Godwin reads very much like contemporary Straussian critics:

According to the usual sentiment, every club assembling for any civil purpose, every congregation of religionists assembling for the worship of God, has a right to establish any provisions or ceremonies, no matter how ridiculous or detestable, provided they do not interfere with the freedom of others. Reason lies prostrate at their feet; they have a right to trample upon and insult her as they please . . . but as it has been before shown that it

cannot be their duty to do anything detrimental to the general happiness so it appears with equal evidence that they cannot have a right to do so. There cannot be a more absurd proposition, than that which affirms the right of doing wrong (I, 165-66).¹

Like Rousseau, his exemplar in much of this criticism, Godwin is basically a *philosophe*; and the Enlightenment thought he espouses in essence revives Platonic and Stoic conceptions of right reason, truth, and justice. Indeed, at this point there is every good reason to expect Godwin to continue his Rousseauian assault on liberal government by repudiating its negativism, its failure positively to promote virtue, the good life, or at least the general good. All he has written to this point in the argument tends in that direction. If not an enlightened despot then at least a democratic sovereign finding duty and right reason via the law of the general will seems in order. But Godwin follows neither of the two alternatives taken by the French *philosophes*; he sets out on his own path to anarchism. Is it the duty of governments to watch over the manners of the people, he asks? Ought it to superintend the sentiments of the people and "encourage such as are favorable to virtue, and to check in the bud such as may lead to disorder and corruption?" "Surely," he states ironically, "government can do some positive good" (II, 214-15). But no, alas it cannot, he proclaims. Helvetius and Rousseau are both rejected, and in an abrupt about face there is the Godwin one expected.

Government is, in all cases, an evil; it ought to be introduced as sparingly as possible. Man is a species of being, whose excellence depends on his individuality; and who can be neither great nor wise, but in proportion as he is independent (II, 214-15).

This turn in the argument introduces Godwin's second destructive stage, his assault on law and political authority in the name of private judgment and individuality. The mood shifts decisively and one finds the traditional liberal preoccupation with individual freedom pushed to extremes—to anarchist extremes. But the antiliberal Godwin, the critic of rights and the defender of duty and a higher public good, will eventually return. He will eventually proclaim a vision of an ideal society, where not governments but friends and neighbors will lead men to truth and virtue. But in this early stage of the argument he is concerned with the

defense of private judgment and the need to replace the brute engine, government.

Godwin argues, then, that self-determination and independence are basic to the nature of man. Free man consults his own reason and draws his own conclusions. He then will conscientiously conform to whatever are his own ideas of propriety. The free man is one who exercises his own judgment and, "stands by himself, and rests upon his own understanding." The fundamental principle of politics, writes Godwin, is the universal exercise of private judgment, a doctrine "unspeakably beautiful." Each man seeks truth and right through his own rational faculties. The only principle which legitimately imposes upon him any specific conduct is the conviction of his individual understanding (I, 168; 182; 181). We are unmistakably in the camp of individualistic liberalism. It is only necessary for Godwin now to push this liberalism to the finality of anarchism. This he does by criticizing first governmental authority, then law, and finally punishment.

Anything that moves man to action other than his own private judgment, Godwin contends, is by definition force or coercion. Government, which sets up other men as permanent arbiters over the actions of individuals, is the ultimate determinant of most men's destinies. As such "government is nothing but regulated force; force is its appropriate claim upon your attention" (I, 230). But Godwin's rejection of government is much more complex than this often quoted passage. Government is only the third of three possible forms of authority. In the first, the authority of reason, the individual obeys simply himself. Reason, indeed, represents the absence of government. The second form of authority is confidence in and deference to some esteemed figure and his decisions. As Godwin rightly points out, this is the original meaning of the word authority; it represents "compliance which is the offspring of respect" (I, 231). Godwin regards only the third form of authority as genuinely political; this is governmental authority, to which compliance is based not upon esteem and respect but upon force and power. Self-government, the first form of authority, is obviously the ideal. Of the two forms of external authority, the second is clearly preferable to the third, according to Godwin, especially when the individual has reason to believe that another person knows better than he what is proper to do. If this is the case, then one ought to conform to such direction. The door is opened here, we must note now and return to later, for an elitist appreciation of enlightened tutelage, an idea not at all unconge-

¹ Compare, for example the sentiments expressed in this passage with those of Walter Berns in his *Freedom, Virtue and the First Amendment* (Baton Rouge: Louisiana State Univ. Press, 1957.)

nial to the eighteenth century *philosophe*. But, nothing, Godwin writes, can justify the third form of authority. It is totally contrary to reason and justice to surrender one's private judgment, i.e., to obey another man because he is superior in station "or because of concurrence of circumstances has produced him a share in the legislative and executive government of our country." Men need not, however, take arms to overthrow this brute engine, that Godwin calls government. Such forms of government are doomed anyway, writes Godwin, for all government will ultimately wither away. People obey government only because they are ignorant. When knowledge and virtue increase, men will discover that there is "no mystery in government which uninitiated mortals must not presume to penetrate." At some point they no longer will need nor will they tolerate authority of governments, the third form of authority. Formal governments will be abandoned and only the authority of the truly esteemed and wise will operate. But as knowledge and virtue further increase men will be themselves wise and defer to the authority of no one but themselves. With the inexorable passage of time, then, and with the progressive dissemination of knowledge and wisdom all forms of government, formal and informal, will slowly be put to rest.

In other words, government cannot proceed but upon confidence, as confidence on the other hand cannot exist without ignorance. The true supporters of government are the weak and uninformed, and not the wise. In proportion as weakness and ignorance shall diminish, the basis of government will also decay. This, however, is an event which ought not to be contemplated with alarm. A catastrophe of this description would be the true euthanasia of government (II, 214-215).

Having disposed of governmental authority, Godwin turns next on law. None of Rousseau's rhapsodic appreciation of law is found here.⁸ Godwin's central point is that the diversity of human experience defies generalization, one important form of which is abstract law. No action of any one man is the same as another's. Law is either stretched on a procrustean bed to fit new cases, or else a new general law is made. In either case, Godwin argues, instead of being certain and eliminating ambiguity, law is cloaked in uncertainty. Few can thus know what to expect at the hands of the law. Moreover, it "fixes the human mind in a stagnant condition," and substitutes permanence for pro-

gress (II, 400; 403). Because of these shortcomings law must be abolished and replaced by what Godwin calls situational wisdom. Reason, he holds, must be applied anew to each incident, and all the many and unique factors of each case must be heard and understood. Only this conduct befits a rational people.

The final step in the destructive phase of Godwin's anarchism is his attack on the doctrine of punishment. It is essential for his anarchism that he question the legitimacy and the utility of punishment, since, as he himself concedes, the most important justification for government is that in order to prevent the hostile actions of one member of society upon another there must be the sanction of criminal punishment. Punishment is unjust, however, Godwin contends, because it assumes free will when in fact necessity and circumstance propel the criminal to his action. In a famous passage Godwin suggests that the assassin can no more prevent the murder he commits than can his dagger (II, 324). Moreover, he adds, punishment is a useless method of dealing with a wrongdoer. Like all forms of political coercion it is a mere recourse to force that has no intrinsic capacity to convince. "It is no argument . . . it begins with violently alienating the mind from the truth with which we wish it to be impressed." One ought to reason with a criminal and convince him of his errors, for as Godwin firmly believes, no man can reject truth and choose falsehood when the evidence is clearly and fairly presented (II, 237; 225).

So much, then, for the destructive phase of Godwin's anarchism. Government, law, and coercion are pushed aside by self-determined, wise men who use their own private judgment and their own reason. These men are independent and autonomous. "Each man should be wise enough to govern himself, without the intervention of any compulsory restraint." So keen is Godwin on protecting this individual autonomy that he pushes his anarchic-individualist ideal to the elimination of all forms of cooperation. "Everything that is usually understood by the term cooperation," he writes, "is in some degree, an evil" (II, 246; 501). And that degree is the extent to which cooperation inhibits a man's individuality and prevents his thinking for himself. Meals in common and communal labor are suspect. Men may now have to work together in factories, but technology, will soon, he predicts, make the most extensive operations within the reach of one man. Theatrical and musical performances will go, too. Autonomous men may very well cease to "repeat words and ideas that are not

⁸ See for example Rousseau's praise of law "the celestial voice," in his *Discourse on Political Economy* (New York: E. P. Dutton & Co., 1955), p. 294.

their own," or music composed by others. Though they may find pleasure in one another's company, solitude will be the greatest delight of autonomous men: they "ought to be able to do without one another." And so one comes to Godwin's famous views on marriage, which he sees as both unjust and monopolistic. Even more harmful is its unnecessary emphasis on cooperation and its infringement on personal independence (II, 503; 504; 505; 508ff.).

In these prophetic passages of *Political Justice* one finds the quintessential statement of Godwin's anarchic individualism—the ideal of independent, self-determined, non-cooperative man. This is the often parodied Godwin that has amused his detractors ranging from the staff of the *Anti-Jacobin Review* of his day to the Victorian Leslie Stephen. But Godwin's anarchism, and anarchist thought after him, does not end with this longing for individual privacy and autonomy. It moves on to a second stage, more positive and less liberal—the vision of an ideal order, in which emphasis shifts dramatically to values of community, cooperation, and solidarity, all achieved through the coercion of virtuous and duty-seeking neighbors.

"Political Simplicity": The Politics of Utopian Anarchism

Godwin's vision of anarchist society is structured around three basic principles: "political simplicity," "public inspection," and "positive sincerity." One must begin with "political simplicity," for the two other principles presume its prior existence. The practice of sincerity and the performance of public inspection require the geopolitical reorganization of society. The masses of mankind have been duped, claims Godwin, "by the mysterious complicated nature of the social system" (II, 208); the social order must therefore be simplified. Simplicity of political arrangement would liberate men and could be achieved through localism and federalism. If nations were broken down into smaller autonomous units, parishes that regulated their own affairs, they would cease their tyranny:

The ideas of a great empire and legislative unity, are plainly the barbarous remains of the days of military heroism. In proportion as political power is brought home to the citizens and simplified into something of the nature of parish regulation; the danger of misunderstanding and rivalry will be nearly annihilated. In proportion as the science of government is divested of its present mysterious appearances, social truth will become obvious, and the districts pliant and flexible to the dictates of reason (II, 293).

A general assembly of these parishes would be a rare and extraordinary event. Normally, representatives of the parishes would assemble but once a year to adjust their few differences. This assembly would pass no laws, take no votes. To do so would be an affront to reason and justice, "the deciding upon truth by the casting out of numbers" (Rousseau is not totally forgotten). As part of the generally nostalgic flavor of Godwin's ideal order, this yearly assembly would merely perform the medieval task of hearing complaints and representations from constituents (II, 205; 207).

Within the parish no political institutions would be necessary. Occasional disputes would be settled by calling a jury, but these juries would have no coercive power. They would only invite and reason. They would persuade, not dictate. Political authority in the parish would be replaced by public persuasion. The entire system is made to work by the power "of public inspection." In a simple and small unit the "observant eye of public judgment" would prompt each individual to seek the common good. Godwin was not the first, nor the last, to suggest that men are less moral under the cover of a large society and more virtuous in the face-to-face contacts of simpler and more local surroundings. Extensive governments require coercion and force; smaller units allow the sway of opinion. "The inspection of every man over the conduct of his neighbors, when unstained with caprice, would constitute a censorship of the most irresistible nature." Law and government are replaced, then, in Godwin's anarchist utopia, by the watchful and censoring eye of public opinion. The disapprobation of one's neighbors is enough, he holds, to make everyone shy away from vice. (II, 211; 231; 274).

Public inspection and censorship would work, however, only if neighbors tell the truth to one another; i.e., only if they are sincere. Godwin's preoccupation with sincerity, his quaint insistence that one must not have his butler lie when one is in but not eager to see company, is more than simply the eccentricity of a bookish crank. It is, in fact, a principle basic to his vision of a new order, for public persuasion can only work if each citizen is his neighbor's "ingenious censor" and if they proclaim one another's "virtues, good deeds, meanness and follies." Only through such just and impartial evaluation can vice presumably be discouraged and virtue encouraged. In the telling of these truths, neighbors should not be restrained by private protections like libel laws. The occasional false charge or mistaken censor,

Godwin insists, is a small price to pay for the advantages of unlimited and sincere criticism (I, 329; II, 270ff.). It is in light of this important political purpose, then, that one is to understand Godwin's claim that sincerity is "the most powerful engine of human improvement." But sincerity does more: it helps bring together the antiliberal and the liberal aspects of his anarchism. Liberated from governmental coercion, men in Godwin's parishes will freely exercise their private judgments, but they will not leave each other alone. They will not till their gardens in passive liberal enjoyment of private rights. They will recognize their fundamentally nonliberal duty, "the most indispensable business of man, to study and promote his neighbors welfare" (I, 340; 339). Their exercise of private judgment will be in the service of others. It will pursue a higher will, a higher realm of truth. This pursuit will take the form not of legislative enactment of a general will but rather of positive, constructive, and sincere neighborly advice and criticism. There is, according to Godwin, no plurality of ideals. The nature of man and the nature of his good life are not relative. "There is but one perfection to man; one thing most honorable . . . all else is deviation and error; a disease to be cured, not to be encouraged." That single and overriding ideal is "benevolence," or as he calls it elsewhere, "disinterested transport." The goal of man's social existence is "to make others free, virtuous and wise." Godwin is convinced that "there is but one best mode of social existence," which can be deduced from the principles of human nature for there must be one code of truth on the subject of our reciprocal duties (I, 240; 314). It is as if Rousseau's legislator and general will were replaced by what Godwin calls:

Putting ourselves in the place of an impartial spectator, of an angelic nature,—beholding us from an elevated station, and uninfluenced by our prejudices, conceiving what would be his estimate of the intrinsic circumstances of our neighbor and acting accordingly (I, 133).

Each individual is obliged to see that his neighbor lives up to the uniform code of truths which teach benevolence and public spirit. To do this, he must, however, break the cold reserve that keeps men at a distance from one another. Men must end the charade in which they talk to one another without disclosing their feelings or opinions. They must sincerely and openly speak to each other about their moral character, "how they ought to be employed, and how to be improved" (I, 294).

Sincerity is the key, then, to understanding

the relationship of the diverse parts of Godwin's argument, as well as being the essential value in his simple, face-to-face, nonpolitical society.⁹ Laws would be unnecessary, for example, in this vision of political simplicity. The inhabitants of a small parish seeing that laws were irrational would judge each case not by written axioms but by the circumstances and demands of the particular case, what Godwin calls the "spontaneous justice" of the parish. But even this would become unnecessary at some point, for eventually crime would be totally eliminated. Public disapproval would turn all knaves honest. Who would commit a crime when faced by "the sober condemnation of every spectator?" Even the "legal" crime and murder of governments would come to an end in the new order, for they were indelibly associated with the false notions of extensive territory and dreams of glory, empire, and national greatness (II, 294; 410; 275; 361).

The new anarchist order would see the decline of crime for reasons much more basic than simply the public disapproval of knaves. The very motivation to crime would dry up. For, according to Godwin, its most important source was economic inequality. If this were changed, crime would cease. Godwin is dramatically outspoken in his condemnation of inequality. Few English Jacobins of the 1790s would outdo his indignation at social injustice; perhaps only his wife, Mary Wollstonecraft, and Tom Paine could match him on this score. The rich, Godwin claims, were "in all countries directly or indirectly the legislators of the state." Their laws invariably favored their interests over those of the poor and "reduced oppression into a system" (I, 18; 19). While the rich, for example, could associate for monopoly and gain, the law of Parliament prevented workmen from combining in unions. The effect of this inequality on the poor was disastrous. To be born poor, Godwin writes, "may be said under another name to be born a slave." Any potential for distinction and intellectual achievement in the lower orders was dashed by "the present ordering of society . . . the great slaughterhouse of genius and of mind."¹⁰

The great ally of the rich in this oppression

⁹ Godwin's concern with sincerity is also important in understanding his link to Romanticism. Here and in his discussion of openness, spontaneity, and values of the heart (II, 280; I, 335–340) one sees a Godwin far different from the stern rationalist he has usually been depicted as, and much closer to the romantic so evident in his novels.

¹⁰ William Godwin, *The Enquirer* (London, 1797), pp. 62, 17.

and brutalization of the poor was, according to Godwin, modern technology; thus he sets the pattern for subsequent generations of anarchists in the condemnation of the machine age. Like many of them, however, he also relished the progressive potential of technology. Would not, for example, the drudgery and oppression of communal factory work be ended with the invention of one great machine operated by a solitary individual? But his response in this, his antitechnological, mood was in fact a rather acute anticipation of later notions of alienation, for the end product of the factory system, he argued, was the estrangement of man from himself, his transformation from worker to machine.

A mechanic becomes a sort of machine; his limbs and articulations are converted, as it were, into wood and wires. Tamed, lowered, torpidified into this character, he may be said perhaps to be content.¹¹

The most grievous political result of this constant and unmerciful exercise of superiority by the rich was, according to Godwin, that it produced crime, which in turn could be used as justification for punishment and, therefore, as rationale for government. The exploitation by the rich led the poor to seek reprisals. The poor see "the state of society as a state of war." In his novel *Caleb Williams*, Godwin has his pathetic hero, driven to crime, exclaim in his defense:

Oh poverty! Thou art indeed omnipotent! Thou grindest us into desperation; thou confoundest all our boasted and most deep rooted principles; thou fillest us to the very brim with malice and revenge, and render us capable of acts of unknown horror.¹²

The inequality and injustice of this economic system would disappear in Godwin's simple anarchist society. The factory, taxation of the poor, legislation by the rich, oppression and misery leading to class crime—all these would be replaced simply and magically in the transition to a frugal agrarian economy. Once again Godwin anticipated generations of future anarchists. Taxation would be unnecessary in the parish, he holds, since there would be no foreign wars, no domestic office holders. The usual army of government functionaries, clerks, collectors, etc. would not be needed. But Godwin felt that the right to private property, on the other hand, was still legitimate, and was in fact, essential for the exercise of private judgment

and independence. Too great and too unequal an accumulation of property was, to be sure, unjust, but the small and simple parish society would restrain this accumulation by "the good sense of the community, and the inspection of all exercised upon all." Eventually the parish would see an equality of holdings with no man having for his personal use more than his necessities required. Man in his parish society would work only in simple manual industry and agriculture. No one would accumulate unearned property produced by the labor of others; "the mathematician, the poet, and the philosopher, will derive a new stock of cheerfulness and energy from the recurring labor that makes them feel they are man" (II, 315, 433; 412; 480; 482). No one in Godwin's utopia would manufacture luxuries, however, an unnecessary chore that occupied many men in modern nations. Whereas the object of modern society was to multiply labor, Godwin's parish economy would simplify it, would produce "a state of the most rigid simplicity." In the England of the 1790s, one in twenty worked on agriculture, he conjectured; but in his utopia all men would work in the fields at some time. All other jobs would be eliminated. The job of supplying the community with the absolute necessities of life would be parceled out to all men and thus occupy one-twentieth of every man's time. Each member of the community would perform manual labor about half an hour a day; the rest of the day he would enjoy life, learning, and nature in his idyllic rural paradise (II, 480). More probably, I suspect, he would spend much of his free time inspecting and giving sincere advice to his neighbors.

Much of the imagery of Godwin's visionary parish society was familiar to Englishmen of his day. The rural nostalgia and the longing for the simplicity of a frugal economy and a face-to-face society were themes often found in earlier opposition writings under Walpole, both from Tories, like Bolingbroke and Gay, and radicals like Trenchard and Gordon. These themes are also writ large in the Toryism of Goldsmith's *Deserted Village* and the radicalism of Southey's *Wat Tyler*.¹³ Godwin, in his novel *Imogen*, written some nine years earlier, had himself depicted an idyllic community of Druids with no government, and no ranks of distinction. His sincere and simple protagonists, Imogen and Edwin, roundly overcame the villain Roderick. But the vision of *Political Justice*

¹¹ William Godwin, *Fleetwood* (London, 1805), I, 277.

¹² William Godwin, *Caleb Williams*, (London, 1794), II, 30.

¹³ See Isaac Kramnick, *Bolingbroke and His Circle, the Politics of Nostalgia in the Age of Walpole* (Cambridge: Harvard Univ. Press, 1968) and Bernard Bailyn, *The Ideological Origins of The American Revolution* (Cambridge: Harvard Univ. Press, 1968).

differs from other nostalgic pastorals and from the spate of English imitations of Rousseau in its transcendence of mere social and economic nostalgia and its thoroughgoing political radicalism—in short, in its complete dismissal of the political.¹⁴

Even here, however, one wonders if perhaps Godwin didn't have a model for his vision. Where else, for example, might he find a vision of a community free of law, free of coercion, free of institutions, free of power, indeed free of politics? Where else might he find a community of spontaneous solidarity and zealous regard for one another's moral well being? The answer may well be that Godwin, the former dissenting minister, could never quite free himself from the anarchic vision of the primitive Christian community—but that is another story. Suffice it to point out here that Godwin held no illusion that if the government of Britain were dissolved in 1793 his simple parish society would immediately flourish. The citizenry were still unregenerate, and not fit for this dispensation; it would come only in due time. Before one speculates on this eventuality, one must make clear how Godwin felt this dispensation would *not* come about. The radical ideal of anarchic political simplicity, he was certain, could never be brought about by radical political agitation or reform.

"Tumult and Violence": Godwin's Critique of Radical Politics

In the preface to the first edition of *Political Justice* Godwin credits many of his insights to the recent experiments in America and France. He was persuaded "of the desirableness of a government in the utmost degree simple" by the ideas suggested in the French Revolution (I, x). That event, he confesses, more than anything else, prompted his writing the *Enquiry*. Like Price's Sermon at Old Jewry, Paine's *Rights of Man*, Mackintosh's *Vindiciae Gallicae*, and Wollstonecraft's *Vindication of the Rights of Women*, Godwin's *Political Justice* was part of the literary outburst produced among English Jacobins by the French Revolution. But Godwin's response has very much the flavor of another written in those years, Burke's *Reflections*. Godwin, the philosophical radical, would have no truck with any political

action designed to encourage or hasten the achievement of anarchism. The Revolution in France, to be sure, had awakened Godwin's love for freedom, but it had also irredeemably frightened the calm philosophical skeptic in him. Thus in *Political Justice* Godwin specifically repudiates reform politics.

Tumult and violence, the phrase he habitually used to describe the hurly-burly of radical politics, could never bring about an ideal order, he argued. Only when the great majority of society were persuaded, would "the chains fall off of themselves." Any effort to introduce political change—no matter how excellent theoretically—that did not consider the state of the public mind would be both absurd and injurious. Men ought to speculate on the ideal form of political society, Godwin felt, but they should be on guard against precipitate measures. Ideals could be securely established only when there was a general preference in their favor. Philosophers might well know that one form of government is most desirable, but inferior forms of government must still be tolerated since immediate efforts to remove them would introduce something far worse, chaos and turbulence. All the wheels, he writes, must move together lest the fragile structure of society break down:

The interest of the human species requires a gradual, but uninterrupted change. He who should make these principles the regulators of his conduct, would not rashly insist upon instant abolition of all existing abuses . . . Truth, however unreserved be the mode of its enunciation, will be sufficiently gradual in its progress. It will be fully comprehended, only by slow degrees, by its most assiduous votaries; and the degrees will be still more temperate, by which it will pervade so considerable a portion of the community, as to render them mature for a change of their common institutions . . . we shall have many reforms, but no revolutions . . . Revolutions are the produce of passion, not of sober and tranquil reason (I, 99 242ff.).

Political change will come not through political action but through knowledge. New or reformed laws or policies would change nothing if opinion and understanding remained the same. The proper task for the friends of humanity was not to introduce new practices in politics, but "enquiry, communication, and discussion." Godwin assailed Rousseau on this very point. He felt that Rousseau's recourse to a civic religion in the effort to reorient the public mentality enlisted a new authority over a people not yet capable of perceiving the wisdom of the new order. Duping men to receive a system whose reasonableness is not a part of their understanding is, Godwin suggests, "a

¹⁴ William Godwin, *Imogen, A Pastoral Romance* (London, 1784, reprinted 1963, New York Public Library, edited by J. W. Marken). For the English Rousseauians see J. A. Warner, "The Reaction in Eighteenth Century England to Rousseau's *Two Discourses*" in *P.M.L.A.* 38 (1933) and Michael Sadler, *Thomas Day, An English Disciple of Rousseau* (Cambridge: Cambridge Univ. Press, 1928).

questionable method for rendering them sober, judicious, reasonable, and happy." Burke, too, is criticized on this score for his suggestion that men cherish their prejudices. Lulling men not to use their own understanding is wrong when used to uphold the established order as well as when used to change it (II, 128; 130; 140).

The major thrust of Godwin's caution, however, is directed against the "domestic spoilers" who preach immediate dissolution of all governments. Godwin contends that if this should occur in 1793, in a period when political truth had not yet permeated the understandings of all, an anarchy of barbarism and violence would follow, not the reasoned anarchy of political simplicity, positive sincerity, and public censorship. The resultant insecurity and disorder would be far worse than the preceding despotism. Particularly vulnerable were not the poor and the exploited, but men the very likes of Godwin, "the wisest, the brightest, the most generous and bold." In a state of barbaric and violent anarchy one "must bid farewell, to the patient lubrications of the philosopher, and the labor of the midnight oil." True anarchy would not result from headlong and impatient action:

In order to anarchy being rendered a seed of future justice, reflection and enquiry must have gone before; the regions of philosophy must have been penetrated, and political truth have opened her school to mankind (II, 366; 368; 371).

Godwin is so outspoken in his opposition to any form of resistance or revolution that he even rejects the argument from Locke so often cited by English radicals. Nations have no right, Godwin answers, to shake off governments simply because of an alleged break with a social contract or usurpation of traditional rights. Since government is founded on opinion, he argues, even bad governments must have come to be accepted by whatever be the current level of popular understanding. If the people are unenlightened and unprepared for a state of freedom, then resistance is inappropriate, and if political knowledge has spread among them, then their resistance is unnecessary, for subjection of the people even by foreign forces will be futile and short-lived. The theme is constantly reiterated: "if the nation be not right for a state of freedom" no resistance is justified (I, 252ff; 302).

Godwin is careful in *Political Justice* to remind his readers that "the great cause of humanity" has two enemies, the Burkean friends of prejudice and antiquity, on the one hand, and the "friends of innovation" on the other. Having warned of two enemies he concentrates,

however, on only one, those "impatient of suspense, inclined violently to interrupt the calm, incessant, rapid, and auspicious progress which thought and reflection appear to be making in the world." He even uses the misgivings of the one enemy to combat the other. Like Burke, Godwin keenly saw the paradoxes inherent in the Jacobin-Revolutionary mentality. Resisting tyranny, the revolutionaries became tyrants themselves, scrutinizing men's thoughts and punishing their opinions. "To dragoon men into adoption of what we think right," Godwin writes, "is an intolerable tyranny" (I, 256; 257).

Much like an orthodox Marxist, Godwin, in the next stage of his attack on radical reform, cautions against rushing the inevitable developments of progressive change. "There is a condition of political society best adapted to every different stage of individual improvement." Everything is in order, and succeeds at its appointed time. History must unfold in its predetermined stages, one cannot impatiently hurry it to its conclusion. The historical passage is long and arduous; and if we aspire to the final result, we must submit to the portion of misery and vice, which necessarily fills the span in between," he writes. One such evil is the unfair distribution of wealth. This, too, cannot be changed by premature violence or even legislative action; it can only come from a "revolution of opinions." Men should be careful lest they rip down too hastily the institutions of property and its inequalities which are so woven into the social fabric. If private property were suddenly abolished, or if even the principles of feudal rights and privileges of rank were instantly dissolved, thousands of men, indeed whole classes, would be reduced to immediate wretchedness, Godwin contends, and all manner of calamity, convulsion, and upheaval would ensue more dreadful than the evil remedied. It is Godwin the proto-Marxist historicist again who writes, "the inequalities of property perhaps constitute a state, through which it would be necessary for us to pass" (II, 372; 441; 418; I, 273, 259). It is more than simply anachronistic, however, to label Godwin thus. His historical caution on this score is, after all, informed by fundamental bourgeois convictions.

Without permitting to every man, to a considerable degree, the exercise of his own discretion, there can be no independence, no improvement, no virtue and no happiness. This is a privilege in the highest degree sacred; for its maintenance no exertions and sacrifices can be too great. Thus deep is the foundation of the doctrine of property. It is, in the last resort, the palladium of all that ought

to be dear to us, and must ever be approached but with awe and veneration (II, 450).

Burke might well agree with Godwin's criticism of the politics of resistance, revolution, and redistribution, but Burke would never agree with Godwin's more basic and unique assault on political associations, parties, and political life in general. Political parties, associations—any form of cooperative political endeavor—all fall before Godwin's ruthless philosophical bias. Sounding much like a contemporary student of group politics, Godwin insists that in any such collective gathering the intemperate and artful push aside the "prudent, the sober, the skeptical, and the contemplative." Truth could never emerge from the superficial convictions of committed partisans; it would come only from rigorous and laborious enquiry. Party, Godwin argues, puts a premium not on thinking for one's self but on identifying one's creed with that of one's associates. There is no encouragement to enquiry when all the diversity of opinion has been reduced to the position of one common mass. Political meetings encourage harangue and declamation, he contends, which lead only to passion and not argument. Orators and party leaders work on the passions of their followers, playing on hopes and fears. "Truth can scarcely be acquired in crowded halls and amidst noisy debates" (I, 290).

Burke need not have feared, different as their opinions on party were, for in these passages Godwin was not criticizing the Rockingham Party nor the Whigs, old or new. His clearly intended victims were the radicals, those who thought that politics and not philosophy would usher in the new political age.

The Philosopher and the Agitator: Godwin's Attack on John Thelwall

This theoretical repudiation of activism found in the bible of anarchism, Godwin's *Political Justice*, was soon tested in the arena of Jacobin politics. In 1795 and 1796 Godwin's philosophical anarchism and the politics of radical agitation met head on. The second party to the confrontation was the London Corresponding Society and its leader John Thelwall.

The London Corresponding Society, founded in 1792 by a shoemaker, Thomas Hardy, hardly seems to merit Burke's assessment as "the mother of all mischief."¹⁵ Its membership, according to France Place, an early official of the Society, was about two thousand; more generous estimates place it around twenty thou-

sand. The social types attracted by the Society were artisans; mainly hatters, bakers, grocers, booksellers, and shoemakers—"the thinking part of the working people"—as Place characterized them.¹⁶ The division meetings of the Society were devoted to political discussion and communication with other radical societies, often with the two very large ones in Sheffield and Norwich. Pamphlets and broadsides were periodically published proclaiming the Society's positions on issues of the day. The ideology of the group was simple and straightforward and was shared by most of the Jacobin groups of the 1790s. It stood for three principles: annual parliaments, universal suffrage, and parliamentary reform. Despite a liberal dose of Paine's ahistorical and nonconstitutional argument from nature and natural rights, most of the Society's rhetoric was still couched in terms of recapturing ancient lost English rights. Communications and pamphlets were as often signed "Anglo-Saxon" as "citizen." The ancient constitution, freed from its "Norman Yoke" was authority enough on which to base true English liberty in the three areas of reform. There were in the Society a very few who, according to Place, "were desirers of confusion, and for all sorts of absurd and violent measures." Some members, for example, accepted Paine's Republicanism, but even they assumed that it would be achieved after the House of Commons was reformed and the King and Lords set aside peacefully.¹⁷ The L.C.S. (as it was known in its day) was, in short, an association of sober shopkeepers, craftsmen, and journeymen. Some indication of the extent of its threat to the social order can be gathered from Place's evaluation years later of his association with the Society. It had a profound moral impact on its artisan membership, he wrote. Instead of wasting time in the public house, they were encouraged to read books, to think for and respect themselves. In addition, Place wrote, it encouraged them to educate their children. Lest this general assessment be doubted Place related how in 1822 at an anniversary dinner for Hardy he saw twenty-four men whom he knew

¹⁶ Add. Mss. 35, 143 f/9. All references to Additional Mss. refer to the manuscript collection of the British Museum. For further details on the Society see C. B. Cone, *The English Jacobins* (New York: Scribner's, 1968); Philip Anthony Brown, *The French Revolution in English History* (London: G. Lockwood and Son, 1918); W. P. Hall, *British Radicalism 1791-1797* (New York: Columbia Univ. Press, 1912); G. S. Veitch, *The Genesis of Parliamentary Reform* (London: Constable & Co., 1913); and E. P. Thompson's *The Making of the English Working Class* (New York: Random House, 1963).

¹⁷ Add. Mss. 35, 143 f/90. (Place Papers)

¹⁵ Thomas Hardy, *Memoirs* (London, 1832), p. 109.

at the Society in the middle of the 1790s. "Of the twenty-four, twenty were then journeymen or shopmen; they were now all in business and all flourishing men."¹⁸

How surprising then that in May, 1794, Pitt's government should try the leaders of the L.C.S. for treason! The great treason trial of that year brought to an end the first stage of Pitt's repression of the radical Jacobin community. It had begun over a year earlier with his decision to ban Paine's *Rights of Man*, and with Paine's subsequent flight and trial *in absentia*. Anti-Jacobin societies quickly spread across England and Scotland, as did John Reeves's Church and King Associations,¹⁹ groups heavily assisted by Pitt's Government. Moreover, in 1793, the French King was killed, and England went to war. The blissful dawn that had so moved Wordsworth had turned to dusk. The campaign of repression was so widespread that few were surprised in May of 1794 when Pitt accused the Corresponding Society and other leading radicals of plotting:

To assemble a pretended convention of the people, for the purpose of assuming the character of a general representation of the nation, superseding the representative capacity of the House and arrogating the legislative power of the country at large.²⁰

The Tory newspapers echoed Pitt's charge. "The whole system of insurrection lay in the monstrous doctrine of the Rights of Man, and the Corresponding Society composed of the meanest and most despicable of people."²¹ Godwin knew most of the accused. Three of them—Jerrald, Thelwall, and Horne Tooke—were good friends. One defendant, Thomas Holcroft, the writer, was Godwin's close friend. Nevertheless, Godwin had little love for the Society. It stood condemned of all he had written about political associations. Moreover, in *Political Justice* he had even criticized those who argued for change in the name of antiquity, having heaped ridicule on "those governed by the dicta of their remotest ancestors." In his book he had criticized the narrowness of groups like the L.C.S., groups that opposed "a specific tax perhaps, or some temporary grievance" (I, 245; 255). He had, after all, pro-

posed a truly radical restructuring of society, albeit not readily attained. Friendship and the conviction that Pitt's charges were based not on actions but on publications and speeches won out, however, and Godwin anonymously published an attack on the court. Shortly thereafter, the defendants were found innocent by the jury, and at least one of them, Horne Tooke, credited this to Godwin's *Cursory Strictures on the Charge Delivered by Lord Chief Justice Eyre to the Grand Jury*. Soon, however, the basic differences between Godwin and the L.C.S. would surface, and true to his own creed of outspoken and sincere censorship no amount of friendship would quiet Godwin and prevent the confrontation within the Jacobin family.

The essential ingredient for the evolution of the controversy was the emergence after the trial of one of its defendants, John Thelwall, as the leading figure in the L.C.S. He gave to the Society a new and decidedly more radical face. Poet, playwright, pamphleteer, and politician, Thelwall cut a fascinating figure in the 1790s. Friend and confidant of Coleridge, author of Rousseauian pastorals, publisher of a weekly radical newspaper, and ever present agitator at public meetings and lecture halls, Thelwall may well deserve a place in history as the first great leader of the English working class.²² His ideas are not our central concern here; in this story they are less important than his actions. Suffice it to say, however, that he was an intellectual disciple of Godwin's. In his *Tribune* articles and in his very popular *Rights of Man* (1796) he, too, was a perfectionist, who wrote on necessity, the fraud of punishment, and hypocrisy of patriotism, marriage, and gratitude, and one who recognized the obligations of sincerity and truth over friendship. He refused, however, to follow Godwin into the utopia of "political simplicity." Much more concerned with economic and social questions, he wrote primarily of specific reforms required to improve the lot of the working man. As one historian puts it he took "Jacobinism to the borders of Socialism." He also took it "to the borders of revolutionism," which is much more our concern.²³ Thelwall roused the L.C.S. from the depths of its despair and responded to Pitt and the legions of Church and King. His tactic was twofold: first, he gave rousing lectures with great rhe-

¹⁸ Add. Mss. f/93. E. P. Thompson suggests, it should be noted, that Place may be glossing over its real radical thrust to fit his own later, more moderate position (p. 153).

¹⁹ The full name of Reeves's group was "The Association for the Protection of Property Against Republicans and Levellers."

²⁰ Brown, *French Revolution*, p. 112.

²¹ *New Annual Register*, 1794, pp. 190-1.

²² For Thelwall, see C. Cestre, *John Thelwall, A Pioneer of Democracy in England* (London, 1906); B. S. Allen, "William Godwin's Influence upon John Thelwall" in *P.M.L.A.* 37 (1932); and Thompson, *Making of the English Working Class*, esp. pp. 157-161.

²³ Thompson, p. 160.

torical flourish, lectures which soon became the source of inspiration for scores of militant Jacobins. Much more courageous was his second tactic, the mass meeting; he organized and spoke to meetings at least three of which, in 1795 alone, drew more than 100,000 people to London.²⁴ Whatever his cause—universal suffrage, annual parliaments, shorter working hours, or redistribution of wealth—he brought to it an intensity and activism that pushed the L.C.S. from moderation to agitation and active intervention in the street politics of London. As an activist he was disdainful of philosophical and literary fellow-travelers like Godwin who refused to participate in Jacobin agitation.

Moderation! Moderation! A compromise between right and wrong! I detest it . . . for what is moderation of principle, but a compromise between right and wrong; an attempt to find out some path of expediency, without going to the first principles of justice. Such attempts must always be delusive to the individual and fatal to mankind. If there is anything sacred, it is principle! Let every man investigate seriously and solemnly the truth and propriety of the principles he adopts, but having adopted, let him pursue them into practice: let him tread on the path which they dictate.²⁵

The change in direction Thelwall brought to the once sober L.C.S. did not go unchallenged within the organization, and within its own meetings, quietly and beneath the surface there occurred the first skirmish between his tactics and Godwin's principles. A serious split developed in the society over the issue of large mass meetings, a split between Thelwall's activist followers and the disciples of Godwin who saw the group as merely engaged in educational work disseminating political wisdom and truth through publication and discussion. The Godwinians lost, as evidenced by the resignation of twenty-one members, "who deplored the increase of factious spirit, the preference given to measures the most inconsiderate and violent." The General Committee of the society answered with an attack on "those many weak minded persons who have attempted to propagate an opinion, that all endeavors to promote the cause of liberty in Britain by means of popular associations must necessarily prove fruitless." Under Thelwall's leadership, Place informs us, many in the society came to believe that repeated mass meetings and agitation would provoke a crisis that would "force the House of Commons to consent to radical re-

form in the state of representation."²⁶ Place, a good Godwinian, resigned, however, rather than agree to such coercive tactics.

This disagreement took place solely within the radical family, but the next and more devastating confrontation took place out of doors for all to see. It came to a head in late 1795 and early 1796 with the introduction into Parliament of Pitt's notorious Anti-Sedition Acts. Pitt's response to L.C.S. agitation was not the reform of Parliament but instead the introduction of two laws abrogating freedom of speech, freedom of assembly, and freedom of the press. Fearful for its survival, Pitt's Government was lashing back. Food riots had spread through England in the summer and autumn of 1795, inflammatory tracts appeared at the same time with titles like *King Killing*, *the Reign of the English Robespierre*, or *the Happy Reign of George the Last*. En route to Parliament, the King was hissed and booed by some two hundred thousand Londoners thronging the streets. His carriage window was broken by a stone, and he is alleged to have gasped, "My Lord, I, I, I've been shot at." For many, the nightmare world of Versailles seemed to have arrived at St. James.²⁷ The Anti-Sedition Acts were Pitt's response to all of this. The Foxite opposition fought the bills unsuccessfully, and upon their passage *habeas corpus* was suspended for eight years. The two acts having been passed, Pitt's repression succeeded. The L.C.S. and the English Jacobins were stilled, only to revive in the short-lived naval mutinies of 1797.

Where did Godwin, the anarchist and philosopher of freedom and truth, stand in all of this? As a careful reading of *Political Justice* might have predicted, he was defending Pitt and attacking the L.C.S. and most specifically his one time friend, John Thelwall. His position is found in his pamphlet, *Considerations on Lord Grenville's and Mr. Pitt's Bills Concerning Treasonable and Seditious Practices and Unlawful Assemblies*, published anonymously by "a lover of order." (Did he find it so hard to betray a friend that he violated his own canon of candor?) The pamphlet begins with a general lecture on the principle of philosophical anarchy, as differentiated from false anarchy, offered by one "untainted with the headlong rage of faction." Governing men in a "petty

²⁶ Add. Mss. 27, 815 f/165-6; f/184. (Letters of L.C.S. 1795-6). Add. Mss. 35, 143 f/15.

²⁷ (Anon.) *Truth and Treason! or a Narrative of the Royal Procession* (London, 1795); See also J. H. Rose, *William Pitt and the Great War* (London, G. Bell and Sons, 1911) pp. 282-88; and Thompson, pp. 141-143.

²⁴ Thompson, p. 141ff.

²⁵ *The Tribune*, No IX, Saturday, 9 May 1795. Thelwall's weekly newspaper was reprinted in collected volumes in 1796. (Vol. I, 214).

and limited circle," he contends, is easy and can be done not by institutions but by reason alone. All men will "exercise an inspection over all," and no deeds can be concealed from the general censure or applause. But in nations with millions of men, "there is no eye penetrating enough to detect every mischief in its commencement." Governments are needed in such states to keep order and provide security. Shifting to the language of Burke, Godwin describes the fragility and complexity of society and government.

He that deliberately views the machine of human society, will even in his speculations approach it with awe. He will recollect with alarm, that in this scene "fools rush in, where angels fear to tread." The fabric that we contemplate is sort of a fairy edifice, and though it consist of innumerable parts, and hide its head among the clouds, the hand of a child almost, if suffered with neglect, may shake it into ruins.²⁸

There are, to be sure, abuses in societies; there are, for example, large numbers of poor and very few rich. But these abuses, Godwin quickly adds, are woven into the fabric of society and must be painstakingly corrected with careful judgment and deliberation lest one destroy faster than one creates, and rear up only false anarchy and barbarism. "These universal principles of political science," Godwin writes, "are necessary as premises" to the examination of Pitt's legislation. They convince one that government has as its major responsibility the protection of order and security, "the blessings we already possess, from the rashness of presumptuous experiment." The major threat to this security, Godwin insists, is the L.C.S. Like the Jacobin party in France, it is filled with impetuous and ardent zealots. It threatens civil order by "the immense multitudes it collects" who wreak disorder in the city streets. Its speeches and resolutions lack all temperance. Having done with the L.C.S., Godwin turns next on its leader Thelwall, whose lectures and public addresses violate all reasonable tenets of political life. The public mind ought to be enlightened, Godwin insists, and a uniformity of understanding reached such that no minister or monopolist would ever be powerful enough to withstand it. Could such a transformation occur in Thelwall's crowded, noisy meetings? No, he answers, and he condemns Thelwall:

Oh reform! Genial and benignant power! How often has thy name been polluted by profane and unhallowed lips! How often has thy standard been

unfurled by demagogues, and by assassins been drenched and disfigured with human gore!²⁹

Thelwall, Godwin continues, is a dangerous demagogue. "An impatient and headlong reformer," he is in no position to weigh his words with proper deliberation and purvey the truth while prompted by the demoralizing stimulus of the clamorous applause of large, excited audiences. He appeals to their passions and not reason, and if as "saving clauses," he urges the practice of universal benevolence and utters warnings against violence, he is like "Lord George Gordon preaching peace to the rioters in Westminster Hall," or "Iago adjuring Othello not to dishonor himself by giving harbour to a thought of jealousy."³⁰

A shocked Thelwall lamented in his biweekly issues of the *Tribune* that even Godwin, his friend for so long, should have turned on him when all the forces of Pitt's repression sought to shut him up.³¹ But Thelwall should have known that candor and sincerity were more important values for Godwin than friendship and that true to his own teaching, Godwin was obliged to censor his neighbor, to show his shortcomings, and thus to contribute to the formation of a more virtuous character.

The Elite and the Masses: Godwin the Anarchist—Some Concluding Comments

What most alarmed Godwin about the L.C.S. was the absence at its meetings of "persons of eminence, distinction, and importance in the country," who could temper the enthusiasm of those "not much in the habits of regular thinking." In addition, Godwin charged that Thelwall, the popular leader, was "not calmed and consecrated by the mild spirit of philosophy."³² As in his attack on reformers in general, the intellectual and elitist disdain shown here was by no means a novel departure in Godwin's public writings. *Political Justice* had been just as outspoken on this score. There, for example, Godwin described the greater part of people as "mere parrots" who mouthed arguments of which they understood little or nothing. Burke's depiction of the democratic masses constantly setting their minds on every "floating fashion and fancy," was no more arrogant than Godwin's castigating a constantly

²⁸ *Considerations* . . . the reader will have noted in this paper the incredible number of parallels found in these events with those in contemporary America, including, as we shall see, the tension between radical intellectuals and radical street (movement) politicians.

²⁹ *Considerations* . . . p. 22.

³¹ *The Tribune*, Vol. II, vii-xiv; Vol. III, 101-103.

³² *Considerations* . . . pp. 17, 19, 20.

²⁸ Godwin *Considerations* . . . (London, 1796) p. 4; 2-3.

changing public opinion by writing "... what is it they desire? They know not. It would probably be easy to show that what they professed to desire is little better than what they hate" (I, 255). One obvious answer, then, is given to the question raised at the end of Part II in this paper. "Political simplicity" cannot be realized by popular agitation led by radical reformers. Nor, for that matter, would widespread popular education suffice. Unlike Tom Paine and Mary Wollstonecraft, both of whom advocated a government-run school system, Godwin saw schemes of national education as only further examples of coercive interference with the individual's unfettered exercise of private judgment (II, 297-304).³³

If the age of felicity could not be entered through the schoolhouse, nor through street pressure on a weak Parliament, how would it be accomplished? How would the simple and sincere social order come about? Enlightened despotism, according to Godwin, was clearly not the answer. Power and force could never be the allies of truth and reason; nor for that matter could monarchs ever be capable of encouraging progress. The same could be said for the clergy and aristocracy. As for the middle class of newly enriched commoners, they too were ruled out as too selfish to be champions of reform. The lawyers who favored the rich over the poor and discouraged candor were also incapable of this task (II, 50-90, I, 41; 18-19; II, 457ff.; II, 390; 423; 407). The only social group capable of the noble mission of moving society to its blissful future was for Godwin, as for legions of future anarchists, the literary and intellectual elite. At bottom, Godwin harbors the one basic prejudice shared by all the enlightenment philosophers, save Rousseau—the unreflecting faith in the power of reflective man. "*Political Justice*," he informs the reader in his preface, is "an appeal to men of study and reflection." If only a small number of "liberally educated and reflecting members" would serve as the "people's guides and instructors," then, Godwin holds, "the business would be done." The "elevated system of the preceptor" would push aside "the groveling views of the great mass of mankind." These few preceptors "having stored their minds with reading and reflection" would communicate their wisdom to all, and through *them*, not through na-

tional schooling, enlightenment would filter down to all mankind (I, xi; 104; 50; 295-6). The social and political mission of the intellectual elite is most succinctly described by Godwin in his *Enquirer* of 1797.

Men of genius must rise up . . . to analyze the machine of society, to demonstrate how the parts are connected together, . . . and point out the defects and the remedy. It is thus only that important reforms can be produced. . . . He who is a friend to general happiness, will neglect no chance of producing in his pupil or his child, one of the long-looked for saviours of the human race.³⁴

There is no doubt who these men of genius were. They could be found in the literary and philosophical circle that always had Godwin at its center, the circle of Holcroft and Godwin's wife, Mary Wollstonecraft, and that of Coleridge, Hazlitt, and Godwin's son-in-law, Shelley. Writing to a friend, Godwin unabashedly described himself and his correspondent as "of course among the few enlightened." His close friend Hazlitt wrote that Godwin "has the happiness to think an author the greatest character in the world."³⁵ His friend Coleridge, however, soon to leave the fold of eighteenth-century rationalism, did not even in 1795 share its and Godwin's high hopes for the intellectual elite. In two sentences he repudiates the intellectualism of Godwin in particular and the *philosophes* in general.

The annals of the French Revolution prove that the knowledge of the few cannot counteract the ignorance of the many . . . the light of philosophy, when it is confined to a small minority, points out the possessors as the victims rather than the illuminators of the multitude.³⁶

In his writings and in his actions Godwin exemplifies the characteristic intellectualism of the Enlightenment. How fitting, then, that in 1795 he should have locked horns with the democrat Thelwall, for in this obscure confrontation an important turning point is marked in the evolution of modern political attitudes. The philosophical and theoretical radicalism of the eighteenth century, the work largely of speculative intellectuals colored by elitist assumptions, meets head on the practical radicalism of the Nineteenth century, the work mostly of political agitators and reformers with democratic popular sympathies. The days would soon be

³³ For Paine's views see *Rights of Man*, Part II. (London, Penguin Books, 1970). For Wollstonecraft's views see *The Vindication of the Rights of Woman* (London, 1792), p. 185. See also the useful book *Education and Enlightenment in the Works of William Godwin* by B. R. Polin (New York, 1962), especially Ch. IV.

³⁴ *The Enquirer*, pp. 10-11.

³⁵ William Hazlitt, *The Spirit of the Age* (London J. M. Dent & Sons, 1964), p. 202; C. Kegan-Paul, *William Godwin: His Friends & Contemporaries* (London, 1876), II, 195.

³⁶ S. T. Coleridge, *Essays on His Times* (London 1850), I, 7-8.

gone when radicals would write, as Godwin did, that men of character would emerge in times of crises and the "soil in which such men are to be matured, is less that of action, than of enquiry and instruction" (I, 298). Interestingly enough, the symbolic aspects of this transition were captured best by Thelwall, the victim of Godwin's intellectual snobbery. To Godwin's charge that in the politics of agitation and reform, one "bids farewell to the patient lubrications of the philosopher, and the labor of the midnight oil" (II, 368), Thelwall responded through his *Tribune* that, on the contrary:

It is in the mixed and crowded audiences in theaters and halls of assembly that the real lover of his species must principally expect to inspire that generous sympathy, that social ardor without which a nation is but a populous wilderness and the philosopher himself only a walking index of obsolete laws and dead-letter institutes.³⁷

This division within the Jacobin camp in 1795 sets the pattern for the evolution of nineteenth-century radicalism and has its counterpart even to this very day. Most friends of humanity react to the ills of society with a political response and like Thelwall seek political goals through political action. A smaller number, however, still cling to Godwin's philosophical response and flee from politics to the vision of a nonpolitical society achieved through the nonpolitical means of education and enlightenment.

One should also note the significance of this split for the general development of anarchist thought, which is where this discussion of Godwin began. Many anarchists would leave Godwin the *philosophe* behind. Much of nineteenth- and twentieth-century anarchism is, of course, an outright repudiation of the Enlightenment rationalism that the founder of anarchism so typified. Ineed, for many more recent anarchists, politics is in fact equated with reason and because of this to be superseded

with and by apolitical action. In the same vein, it could be argued that Thelwall's street politics was in fact a more accurate anticipation of the future anarchist style. But, these points notwithstanding, Godwin stands as the father of modern anarchist thought in a sense more profound than simply being the first to write of society without government.

He gives to anarchism qualities that endure despite the philosophical, methodological, and cultural gap that may separate him and the doctrine's later proponents. As it was in his presentation, the doctrine after him is forever torn between the liberal values of individuality, independence, autonomy, privacy, and self-determination, on the one hand, and the nonliberal values of community, solidarity, and the encouragement of virtue through social pressure, on the other. So, too, later anarchists waver, like Godwin, between a progressive, futuristic orientation with assumptions of perfectibility and endless innovation and improvement, and a nostalgic yearning for a simple, agrarian, and preindustrial existence. Most decisively, however, he has stamped anarchism with its elitism, its abiding convictions that if only all men were as wise or as sensitive as the anarchist then governments would be superfluous, and that until this was the case government represented pure coercion. The artist-anarchist's role meanwhile is to emancipate the ignorant "great mass of mankind," the misguided "Pig Nation," whether slowly and imperceptibly through reason and enlightenment or hastily with the dramatic flourish of an exquisite act. Finally, Godwin in these years may well have marked anarchism with another indelible stamp, its ultimate service to the status quo (although latter-day anarchists may have been and are less willing in this service than he). To the extent that change in political systems—either incremental or revolutionary—has and must come from political activity, there is little to fear from those who seek change but who eschew politics, be it for philosophic enlightenment or for theater.

³⁷ *The Tribune*, II, xiv.

Congress and the Defense Budget: 1960-1970*

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Introduction

Increasing dissatisfaction with our defense policies and growing demands to reorder our national priorities have led to renewed interest in the role of Congress in the shaping of national security policy. What some critics regard as the unresponsiveness of the executive has stimulated hopes that the Congress might play a greater part in directing our defense effort. The conclusion which emerges from a survey of the scholarly literature, however, is that Congress has performed neither an active nor a policy-oriented role in the past and that the prospects for the future are likewise dim. Senator Proxmire's description of Congress as a "pushover for the Pentagon"¹ echoes the conclusions of scholars who have found that Congress does not address the strategic issues nor debate national security objectives.²

Congressional influence in the shaping of defense policies could assume at least two forms: (1) Congress simply could make nontrivial dollar changes in the President's budget request, and (2) these budget changes might be attributable to Congressional policy concerns and program preferences rather than to other motives, especially a desire simply to reduce the level of defense spending. The first condition relates to the *fiscal impact* of Congressional power over defense appropriations. The second condition

relates to Congressional *motives* for making changes in the President's budget.

Conceptually, the two elements are largely independent of each other. Congress could have a substantial impact on defense spending by imposing large across-the-board reductions. This would have important but largely unpredictable consequences for policy. Alternatively, Congress might make selective changes in the budget in order to encourage or to inhibit particular defense policies. These changes would not necessarily be reflected in large dollar changes in the total budget.

Whoever seeks to increase Congressional involvement in defense policy must be cognizant of both aspects. But Congressional motivation is the higher hurdle for those who hope for increased Congressional participation. If Congress already views its control over appropriations as a means by which to influence defense strategy and programs, the goal of Congressional activism in national security policy is more than half realized: Congress's image of its function is already established, and it need only be persuaded to become more involved or to pursue alternative objectives.

A conventional perspective on the last decade might suggest that Congress has been deficient on both counts, that it has sought to serve fiscal motives but has achieved only a small fiscal impact. Studies of the budgetary process which concentrate on nondefense spending conclude that the Congress's orientation to appropriations is essentially "non-programmatic"³ and lead to the inference that Congress is disinclined to use its power over the defense budget as an instrument to shape programs and policies. Generalizing from these studies, we might expect that Congressional changes in the President's defense budget stemmed primarily from *fiscal* motivations, i.e., the desire to save money *per se*, to eliminate waste and inefficiencies.⁴ From

* This study was undertaken while the author was a Research Fellow in Foreign Policy Studies at the Brookings Institution. The advice and comments of John Ferejohn, Leslie H. Gelb, Morton Halperin, Kerry L. Jones, Craig Liske, and John Manley are gratefully acknowledged.

¹ Senator William Proxmire, *Report From Wasteland: America's Military Industrial Complex*. (New York: Praeger, 1970), p. 122.

² The literature is surprisingly silent on this aspect of Congressional behavior during the 1960s, although it is a fairly common description of Congress during the preceding decade. See, for example, Raymon H. Dawson, "Congressional Innovation and Intervention in Defense Policy," *American Political Science Review*, 56 (March, 1962), 44; and Lewis Anthony Dexter, "Congressmen and the Making of Military Policy," in Robert L. Peabody and Nelson W. Polsby, editors, *New Perspectives on the House of Representatives* (Chicago: Rand McNally, 1963), p. 306. For a contrary view of the same period, see Samuel P. Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1959), p. 409; "... Congress . . . gives considered and effective attention to the major issues of military policy involved in the budget."

³ Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), p. 136. Cf. Allen Shick, "Systems Politics and Systems Budgeting," *Public Administration Review*, 29 (March-April, 1969), 139.

⁴ Cf. Morris Janowitz, *The Professional Soldier* (New York: The Free Press, 1960), p. 354. "Fiscal" motivations, as the term is used here, combines Goss's categories of "procedural" and "fiscal" Congressional influence. See Carol F. Goss, "Congress and Weapons Procurement," paper prepared for delivery at the Sixty-sixth Annual Meeting of the American

fiscal 1960 through fiscal 1970, however, Congressional changes in the President's budget request for defense rarely exceeded 2 per cent.

The conventional wisdom which holds that Congress has been unwilling to address the defense budget in policy terms tends to be based on a superficial reading of highly aggregated data.⁵ An exploration within the defense budget, specifically, of the separate appropriation titles and individual budget line-items, suggests that Congress has had more impact in some areas than others, and, more importantly, that Congressional budget activity has been concerned with more than reducing the level of expenditures.

Both cross-sectional and longitudinal variations in Congressional budget activity support this view. Congressional changes in the President's defense budget have been concentrated in two areas—Procurement, and Research and Development—rather than equally distributed among all the line-items. Congressional activity across time has varied directly with the intensity of national debate about our defense policies. Neither fact convincingly disproves the notion that Congress's approach to the defense budget has been simple budget cutting; these may merely be the areas and times when the defense budget has been most accessible to fiscal concerns. These findings do, however, raise some doubts about the adequacy of the "non-programmatic" explanation, because they are precisely the results which would be expected if Congress did address the budget in policy terms and did use the power to appropriate as a tool to influence the shape of defense programs.

The purpose of this paper is twofold. First, it demonstrates that the distribution of Congress's changes in the President's defense budget is consistent with the hypothesis that Congress has a *programmatic* as well as a fiscal orientation toward defense spending; that is, Congressional judgments regarding specific defense programs and their relation to national security, rather than an exclusive concern with reducing spending and eliminating waste, explain Congress's scrutiny of the defense budget. Second, it seeks to illuminate the roles and relative in-

fluence of the House and Senate in the defense appropriations process and to highlight some of the problems of interpretation which are relevant to the broader study of the budgetary process.

The Data Base

The study examined four of the stages through which the defense appropriations⁶ pass every year: the President's budget request to the Congress, House floor action, Senate floor action, and the conference report.⁷ Four of the five appropriations titles were included: Personnel; Operations and Maintenance (O&M); Procurement; and Research, Development, Testing, and Evaluation (RDT&E). Military construction, which is dealt with separately in Congress, was not included. Twenty-eight individual line-items included within these four appropriation titles were selected for study.⁸ Since the budget format has been more or less stable since fiscal 1960, the eleven years from fiscal 1960 through fiscal 1970 define the period of the study. Budget *amendments* were included in the data; budget *supplementals* were excluded.

Before analyzing these data, one should note that aggregating the defense budget by line-item presents important drawbacks for a study of Congressional activity regarding specific national security programs. This is because the funds for particular programs may be distributed among several line-items, and each line-item may include money for several programs. Thus, funds which support the fleet of Polaris submarines may be included under the line-item for "Navy Shipbuilding and Conversion" (Procurement), as well as "Navy O&M," while the single line-item "Navy Shipbuilding and Conversion" covers the procurement of de-

⁵The exclusion from this study of defense *authorizations* seemed permissible primarily because, for the vast majority of the cases considered, neither language nor dollar amounts of authorization acts limited the appropriations actions. Cf. Jack D. Fleer, "Congressional Committees and the Making of Foreign Policy: Authorizations and Appropriations for Major Weapons Systems in the Legislative Process," unpublished Ph.D. Dissertation, University of North Carolina, 1964; and Herbert W. Stephens, "The Role of a Legislative Committee in the Appropriations Process: A Study Focused on the House Armed Services Committee," unpublished Ph.D. Dissertation, Florida State University, 1967.

⁷Although the data in this study are based on floor action, appropriations bills reported out of committee very rarely are changed on the floor. Neither the relationship between the appropriations committees and their parent bodies nor the opportunity for influence inherent in that relationship is addressed in this study.

⁸See Appendix for a list of these line items.

Political Science Association, Los Angeles, September 8-12, 1970. It should be noted that another source of Congressional activity—the desire to advance a political career—is not addressed in this paper.

⁶For a more general discussion of shortcomings of aggregate data, see Joseph La Palombara, "Macro-theories and Micropolitics in Comparative Politics: A Widening Chasm," *Comparative Politics*, 1 (October, 1968), 52-78.

stroyers and aircraft carriers as well as submarines.

These line-item data are employed in this analysis because they represent the only form of disaggregation of the total defense budget which is publicly available and the form in which Congress considers the defense budget and reports its appropriations actions. (Congress also receives detailed information regarding specific weapons systems and projects, but these data cannot be combined to yield the total budget.) Moreover, the different appropriations titles roughly describe the path which weapons systems follow from drawing board to field deployment. New weapons are conceived, designed, developed and tested with funds budgeted under RDT&E. They are produced in quantity with funds from the Procurement budget. The manning and servicing of weapons in the inventory are paid for with funds from the O&M and Personnel budgets.

The Dollar Impact of Congressional Budget Activity

The dollar magnitudes of Congressional changes in the President's budget requests for defense have not been large. Table 1 indicates the relative scale of Congressional activity. From fiscal 1960 through fiscal 1970, five annual appropriations involved changes of less than 1 per cent, seven of less than 2 per cent, and nine appropriations out of eleven varied from the President's request by less than 4 per cent. As one standard of comparison, Richard Fenno considered changes of less than 5 per cent to be "marginal" in his survey of nonmilitary appropriations from 1947 to 1962.⁹ Thus, when compared to changes in the nondefense budget, Congressional alterations in the President's budget requests for defense have been small.

Congressional budget changes also have been much smaller than the reductions made by the administration in the budget estimates submitted by the military services. Although Congressional changes rarely exceeded 4 per cent, the administration regularly reduced the services' estimates by from 10 to 20 per cent.

When compared either to Congressional changes in the nondefense budget or to Presidential reductions in the services' budgets, the dollar impact of Congressional changes in the

Table 1. Percentage Change in Budget Requests

Fiscal Year	President's Change in Services' Budget Requests	Congressional Change in President's Budget Requests
	(%)	(%)
1960	-19.98	-0.06
1961	-10.42	+1.68
1962	- 8.07	+0.58
1963	- 9.78	+0.48
1964	-19.70	-3.66
1965	-28.09	-1.51
1966	-20.10	-0.18
1967	-21.59	+0.70
1968	-22.91	-2.30
1969	-24.09	-1.75
1970	-24.69	-7.49

Source: Department of Defense, Office of the Assistant Secretary of Defense (Comptroller).

defense budget has been modest. Congress's power to appropriate funds did not yield large changes in the levels of defense spending during the 1960s. Other dimensions of Congressional activity should be evaluated within this context.

The dollar magnitude of Congressional changes in level of *total* defense spending obscures variations which occur within the budget. Table 2 shows Congressional changes in the defense budget by each appropriation title. Congress almost always has made larger changes in Procurement and RDT&E than in the other two budget areas. As a result, computation of budget changes based on the total defense budget consistently understates the size of the change in the former two appropriations titles and exaggerates the changes in Personnel and O&M. While the dollar magnitude of Congressional changes has been small, it has been somewhat greater for those budget categories which contain funds for the development and production of weapons systems.

The Focus of Congressional Attention

Two measures of Congressional activity were employed: (1) the presence or absence of changes in the President's budget request, by line-item, and (2) the size or frequency of Congressional changes in the line-items. Both measures indicate that Congressional activity has been concentrated in particular budget categories.

Table 3 classifies the 308 budget outcomes under consideration according to whether the conference report was equal to either the House or Senate, equal to neither ("Compre-

⁹ Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), p. 353. Cf. Bruce M. Russett, *What Price Vigilance?* (New Haven: Yale University Press, 1970), pp. 26, 187; and Proxmire, *Report from Wasteland* . . . pp. 91, 110-11.

TABLE 2. Congressional Changes in President's Budget Requests, by Appropriation Title

	President's Budget Request (in Millions)	Congressional Change (%)
FY1960		
Personnel	11,625	+0.11
O&M	10,512	-0.65
Procurement	13,348	-0.09
RDT&E	3,772	+1.19
Total	39,257	-0.06
FY1961		
Personnel	11,813	+0.18
O&M	10,527	-0.59
Procurement	13,085	+3.32
RDT&E	3,910	+6.85
Total	39,335	+1.68
FY1962		
Personnel	12,746	-0.46
O&M	11,793	-0.53
Procurement	16,860	-1.10
RDT&E	4,790	+9.48
Total	46,189	+0.58
FY1963		
Personnel	13,050	-1.12
O&M	11,569	-0.03
Procurement	16,445	+1.23
RDT&E	6,843	+2.62
Total	47,907	+0.48
FY1964		
Personnel	13,235	-2.90
O&M	11,792	-0.66
Procurement	16,725	-6.09
RDT&E	7,262	-4.31
Total	49,014	-3.66
FY1965		
Personnel	14,597	-0.20
O&M	12,396	-0.67
Procurement	13,756	-2.43
RDT&E	6,722	-4.06
Total	47,471	-1.51
FY1966		
Personnel	14,560	+0.27
O&M	12,472	+0.17
Procurement	11,412	-0.07
RDT&E	6,709	-2.07
Total	46,852	-0.18
FY1967		
Personnel	18,676	+0.29
O&M	15,675	+0.18
Procurement	16,408	+1.43
RDT&E	6,905	+1.14
Total	57,664	+0.70

Table 2 (Continued)

	President's Budget Request (in Millions)	Congressional Change (%)
FY1968		
Personnel	22,001	-1.00
O&M	19,136	-1.46
Procurement	22,917	-4.00
RDT&E	7,273	-2.25
Total	71,584	-2.30
FY1969		
Personnel	23,014	-1.92
O&M	22,787	-4.49
Procurement	23,254	-14.07
RDT&E	8,006	-5.68
Total	77,074	-6.75
FY1970		
Personnel	24,377	-3.31
O&M	21,792	-4.28
Procurement	20,887	-14.58
RDT&E	8,222	-10.37
Total	75,278	-7.49

Source: Department of Defense, Office of the Assistant Secretary of Defense (Comptroller).

mise"), equal to both the House and Senate but not equal to the President's request ("Same"), or equal to the President's request in all three Congressional budget stages ("No Change"). In 22 per cent of the budget outcomes, neither house of Congress made any change (either increase or decrease) in the President's request; about a quarter of the President's budget requests were, if not unexamined by Congress, at least untouched by either house.

Table 4 suggests that Congressional inaction has been concentrated¹⁰ in the Personnel cate-

¹⁰ Since each appropriation title includes a different number of line-items, a measure of relative concentration of "No Change" outcomes (Table 4) is obtained by taking (1) the incidence of "No Changes" in Personnel as a percentage of the total number of "No Change" outcomes, (2) the number of Personnel budget decisions as a percentage of the total number of budget decisions, and (3) dividing the former by the latter. If the incidence of "No Change" outcomes were distributed in the same proportion as the total line-items, the relative concentration would equal 1.00. Index values greater than 1.00 indicate a greater than expected incidence of "No Change," and values less than 1.00 indicate a fewer than expected number of cases. Thus, 13 per cent of "No Change" outcomes occurred in Procurement and only 6 per cent in RDT&E but, owing to the greater number of line-items included under Procurement, the relative concentration of "No Change" indicated in Table 4 is about equal for both categories (.45 vs. .43).

Table 3. Distribution of Appropriation Outcomes, by Appropriation Title, FY1960-FY1970

Appropriation Outcome	Per-sonnel	O&M	Procure-ment	RDT&E	Total
	(%)	(%)	(%)	(%)	(%)
House	05	08	07	05	06
Senate	26	18	17	14	20
Compromise	02	38	42	43	27
Same	29	17	24	30	25
No Change	38	20	10	09	22
Total	100% (110)	100% (66)	100% (88)	100% (44)	100% (308)

gory. If Congressional activity were equally distributed among the appropriation line-items, the index of relative concentration would equal 1.00 for all categories. The high concentration (1.72) of "No Change" outcomes in the Personnel category indicates that the President's budget requests for these items were quite likely to be left untouched by Congress. By contrast, the low concentration of "No Change" outcomes in Procurement (0.45) and RDT&E (0.43) shows that the President's requests in these areas were very likely to be changed by Congress. Congressional attention and activity have been concentrated in the latter categories at the expense of the former.¹¹ The concentration of "No Change" outcomes (0.90) was also relatively high in the O&M category. Although the evidence is not nearly as strong as the case of Personnel, it nevertheless seems reasonable to suggest that budget decisions relating to Operations and Maintenance have commanded relatively little Congressional attention.

The Personnel category constitutes about 30 per cent of annual Department of Defense expenditures and O&M contributes 27 per cent. (Procurement represents about 32 per cent and RDT&E the remaining 11 per cent.) Thus, more than half the budget seems to receive a relatively low level of Congressional attention, as measured by the likelihood of changing the President's budget requests. This helps to explain why the total DOD budget has shown such small Congressional changes in the executive budget, viz., even relatively large changes in Procurement and RDT&E are

¹¹ If "No Change" is an indication of minimal Congressional interest, "Compromise" suggests the reverse case in which both the House and the Senate were sufficiently attentive to make changes in the President's request, and were sufficiently committed to their respective positions so that neither house yielded completely to the other in the conference.

Table 4. Relative Concentration of "No Change" Outcomes, FY 1960-FY 1970

Personnel	O&M	Procurement	RDT&E
1.72	.90	.45	.43

<1.00 = less than expected concentration.

>1.00 = greater than expected concentration.

likely to be blunted and obscured by that half of the total budget which changes hardly at all.

Thus far, our measures of Congressional activity have been restricted to the most extreme cases—the presence or absence of Congressional changes in the President's budget. A more complete picture may be obtained by a measure which is able to distinguish budget categories in which there were small or infrequent changes in the President's budget from those in which there were large or frequent changes.

The absolute value of the difference between the President's budget request and the Congressional appropriations (computed as a percentage of the President's request) is one measure of the distribution of Congressional activity. The mean annual absolute difference between the President's request and Congress's appropriations supplies a useful summary index of Congressional activity for the period under consideration. If Congress made no changes in a particular budget category in the eleven year period, the index of activity = 0.00. Large or frequent Congressional changes in the President's requests (either increases or decreases) will yield larger values for the index.

The level of Congressional changes for each of the four budget categories and for the total DOD budget (included as a benchmark) is reported in Table 5. These results confirm the observations based on the distribution of "No Change" outcomes reported above. The mean annual absolute difference for the total defense budget is 2.308. The Personnel budget category has the smallest values (1.069), indicating minimal Congressional activity. Procurement and RDT&E rank highest (4.401 and 4.547 respectively), suggesting that Congressional attention and activity have been focused on matters relating to the procurement of weapons systems. O&M (1.246) is closer to Personnel than to either of the other two appropriation titles.

One may conclude from these results that Congressional activity, and inferentially, Congressional interest, is concentrated on Procurement and RDT&E matters at the expense of Personnel and O&M. Congress does not ordinarily impose "across-the-board" cuts on the

Table 5. Congressional Percentage Changes in President's Budget Requests, Mean Annual Absolute Difference

	Personnel	O&M	Procurement	RDT&E	Total DOD
Mean Absolute Difference	1.069	1.246	4.401	4.547	2.308

defense budget. The total DOD budget appears to emerge from the Congressional budget stages virtually unscathed largely because more than half of that total is virtually untouched as it passes through Congress.

Congressional Activity: Fiscal or Programmatic?

Given the size and complexity of the defense budget, and the limited resources available to the Congress to monitor it, it is not surprising that Congress has chosen to focus its resources on selected areas rather than risk dissipating its energies by surveying the entire budget in equal, if not great, detail.¹² The question remains, however, why the Congress appears to have consistently concentrated on Procurement and RDT & E at the expense of Personnel and O&M. The following speculations are offered to guide to further research on the question.

Wildavsky and Fenno in their research on the nondefense budget have suggested that Congress is inclined to emphasize the fiscal, or dollar-level, aspects of the budget at the expense of a program orientation.¹³ Congress, it is implied, is less concerned with what is obtained than with what it will cost. It does not seem likely, however, that Congress is focusing its attention on Procurement and RDT & E merely in order to maximize the short-term economic impact of defense expenditures; a purely fiscal orientation would lead it to other areas of the budget.

In order to evaluate the expected short-term fiscal impact of Congressional appropriations, it is necessary to distinguish between those *appropriations* and the *expenditures* which are allowed by them. Congress appropriates in terms of "new obligational authority" (NOA)—permission to incur additional financial liabilities up to the limit set by the appropriation. Expenditures permitted by that NOA need not occur in the same year in which funds are appropriated. Nearly all funds budgeted for Personnel are expended in the same year in which they are appropriated. By contrast, only about sixty cents of every Procurement dollar is spent in the same year as obligated, the balance being

expended in succeeding years.¹⁴ Thus, if Congress were seeking to have an impact on the level of defense *expenditures* in the next fiscal year, it would be adjusting the Personnel budget rather than the Procurement budget. In fact, if influencing the short-term level of federal expenditures by changing the defense budget were a serious objective of Congress, the distribution of its activity would be almost precisely the *opposite* of that which is observed.¹⁵ Congress has evidenced concern about both the price and the *product*.

Several factors direct Congressional attention to Procurement and RDT&E. Fenno and Wildavsky suggest that Congress seeks to compensate for its limited resources for coping with the size and complexity of executive budget requests by focusing primarily on *changes* made in the previous year's budget and *changes* proposed in the present year's request.¹⁶ The Procurement and RDT&E categories typically contain the largest number of recent and proposed program changes. On the basis of studies of the nondefense budget, we would expect Congressional scrutiny to be directed to these budget categories.

Congress's alleged inclination to take incremental, remedial, undramatic budget actions may also be a factor in directing its attention to Procurement and Research and Development. These budget categories can be disaggregated into individual programs with relative ease. Congress may make decisions about, and changes in, these individual weapons systems, one by one, with a sense that the decisions are more or less independent of one another.¹⁷

It must be emphasized, however, that Con-

¹⁴ Cf. *Congressional Quarterly Almanac*, 20, 1964, 154-55.

¹⁵ This is supported by the experience of Projects 693 and 703, Pentagon budget drills to hold expenditures within Congressional and Presidential budget ceilings. In both cases, the largest reductions occurred in Personnel and O&M. I am indebted to Charles Shirkey and Arnold Kuzmack for this information as well as other assistance.

¹⁶ Fenno, *Power of the Purse*, pp. 318, 333; and Wildavsky, *Politics of the Budgetary Process*, p. 59.

¹⁷ Cf. David H. Davis, "The Price of Power," *Public Policy*, 18 (Spring, 1970), 377. Jernberg has found that certain committees' line of questioning indicates a programmatic orientation. See James E. Jernberg, "Information Change and Congressional Behavior," *Journal of Politics*, 31 (1969), 722-40.

¹² See, for example, Fenno, *Power of the Purse*, p. 332.

¹³ Wildavsky, *Politics of the Budgetary Process*, p. 60, Fenno, p. 316.

gressional activity cannot be explained simply as responses to year-to-year changes made by the President in the executive budget. A model of "simple incrementalism" might predict that changes in the line-items in the President's budget at time t (as compared with appropriations at time $t-1$) would stimulate Congressional changes in those line-items during the Congressional phase of the appropriations process.

The "simple incrementalism" model is suggested by the following description drawn from Wildavsky's study of the budgetary process:

Budgeting is incremental, not comprehensive . . . [T]he men who make the budget are concerned with relatively small increments to an existing base. . . . As Representative Norrel declared in testifying before the House Rules Committee, "If you will read the hearings of the subcommittees you will find that most of our time is spent in talking about the changes in the bill which we will have next year from the one we had this year, the reductions made, and the increases made. That which is not changed has very little, if anything, said about it."¹⁸

According to this model, the previous year's appropriations constitute a budgetary "base," and budgetary review is devoted primarily to deviations (increases or decreases) from that base. The "simple incrementalism" model assumes that maintaining stability in funding levels is a primary Congressional objective, and to that end, Congress will seek to minimize deviations from the budgetary base.

This line of argument permits either of two conclusions. First, deviations from the base may merely attract Congressional attention and stimulate Congressional activity (whatever the motivation of the activity). That is, the locus of Congressional activity is largely dependent upon Presidential initiative. If this were the case, between-year changes in the President's budget would be expected to be highly correlated with within-year changes by Congress.

A second and obviously related conclusion is that Congress might be motivated by the objective of maintaining stable levels of funding by minimizing deviations from the budgetary base. If this were the case, annual changes in the President's budget would be negatively correlated with Congressional budget changes.

Neither prediction is supported by the data. There is virtually no relation between the size of the changes made by Congress in the Presi-

dent's budget line-items and year-to-year changes in the President's budget requests ($r = -0.006$). Congress does not simply react to year-to-year changes in the executive budget. Its attention is attracted by features other than annual variations in the President's requests, and its objectives are not restricted to maintaining a stable level of funding.

The machinations of interested parties in the executive branch help to explain Congress's concentration on RDT&E and Procurement. The military services, particularly the Navy and the Air Force, have their greatest stake in these two areas. Both the claims for increased budget shares and the rhetoric of interservice rivalries (especially competition over roles and missions) are carried on in terms of present and future weapons systems. The civilians in the executive branch who are concerned with the defense budget are required to share the services' interest and similarly to concentrate their resources. Both the civilians and the military may be expected to transfer this focus of concern to the Congress as Congress acts on the budget decisions of the President. Appeals, requests, and demands for Congressional changes in the President's budget are most likely to be in terms of weapons systems, and therefore under the Procurement and RDT&E appropriation titles.

The importance of these categories to the executive is suggestive of another, and more fundamental, reason why Congress distributes its energies as it has. Just as struggles within the executive branch over the content and direction of defense policy are manifest as conflicts over weapons systems, Congress may believe that decisions relating to present and future weapons systems, reflected in Procurement and RDT&E budget actions, are at the very heart of national security policy, and constitute that part of the budget on which Congressional efforts to influence national security policy are most economically expended. As Charles Hitch, former Pentagon Comptroller, has observed: "These [weapons systems] choices have become . . . the key decisions around which much else of the defense program revolves."¹⁹ If Congress were seeking to influence defense policy and, to that end, had a programmatic orientation toward budget decisions, the distribution of Congressional activity would parallel the actual behavior described above.²⁰

¹⁸ Wildavsky, pp. 15, 59; and Otto A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), 529-30. It should be noted that the "simple incrementalism" model, although related to the models evaluated by the latter authors, is not explicitly tested by them.

¹⁹ Charles J. Hitch, *Decision-Making for Defense* (Berkeley: University of California Press, 1965), p. 23.

²⁰ Congress's interest in influencing the content of national security policy by means of its control of the pursestrings explains in part the reassertion by the Armed Services Committees of their right to

Table 6. Correlation(r) of Between-Year Budget Changes and Within-Year Budget Changes, FY 1960–FY 1970, by Appropriation Title

Personnel	−0.337
O&M	−0.212
Procurement	0.089
RDT&E	0.101

This suggests that the “simple incrementalism” model might better describe budget changes in those areas—Personnel and O&M—which provide less leverage for influencing defense policy. The relationship between annual changes in the President’s budget and within-year Congressional changes should yield greater negative correlations for these appropriations titles than for Procurement and RDT&E.

The results reported in Table 6 lend some support to this hypothesis. Although none of the correlations is large, the rank order and signs are in the predicted direction. There is an inverse relationship between Presidential changes and Congressional alterations for the Personnel ($r = -0.337$) and O&M ($r = -0.212$) line-items as the “simple incrementalism” model would predict. The smaller and positive correlations for Procurement ($r = 0.089$) and RDT&E ($r = 0.101$) are contrary to the predictions of “simple incrementalism.”

Nonprogrammatic factors such as focusing on changes and making incremental alterations which, according to the literature, account for Congressional attention to Procurement and RDT&E, may simply reinforce Congress’s independent inclination to influence the direction and content of national security policy. As Fenno notes (regarding nonmilitary budgets):

A program established last year or proposed for the coming year will receive careful scrutiny; and it is here that the *program preferences* of a key Committee member can work themselves most directly and decisively into legislative policy-making.²¹

authorize on an annual basis the major portions of the Procurement and RDT&E budgets. See: Dawson, “Congressional Innovation . . .,” p. 43. For similar motivations influencing legislation regarding Pentagon reorganization plans, see John C. Ries, *The Management of Defense* (Baltimore: Johns Hopkins University Press, 1964), pp. 95–96, 104, and 141–142.

²¹ Fenno, *Power of the Purse*, p. 318. For material on the Appropriations Committee’s interest in policy see: Goss, “Congress and Weapons Procurement,” p. 7; Dexter, “Congressmen and the Making of Military Policy,” pp. 316–17; and Bernard K. Gordon, “The Military Budget: Congressional Phase,” *Journal of Politics*, 23 (November, 1961), 693–94.

An examination of what Dexter calls the “rhetoric of justification”²² supports the conclusion that Congress has made changes in the Procurement and RDT&E budgets on the basis of its understanding of the requirements of national security. Congressional increases in the President’s budget frequently are explained in these terms. In its report on the FY 1960 budget, the House Appropriations Committee took note of the need for limited war forces:

We must . . . maintain a capability of coping quickly with any incidents which may develop as an additional assurance against a small affair growing into a much larger one.²³

Believing that the Eisenhower administration had not adequately recognized the necessity of a strong limited war capability, the House increased the President’s request for Army Procurement by 20 per cent.

The same House Report also noted that the mobility of forces-in-being was a more important contribution to the limited-war capability than was the ability to mobilize large reserves:

It is not so much a matter of mobilizing immediately available manpower to handle a limited war as it is the capability of the necessary manpower to get quickly to the spot, adequately equipped and supported.²⁴

In support of this strategic choice, the House created a new budget line-item, “Airlift Modernization,” and appropriated more than \$300 million for it in FY 1961. Similarly, the Senate Appropriations Committee exceeded the Administration request for a new manned bomber because it believed that ICBM’s were not sufficiently reliable.

We view the B-70 cutback and stretchout (planned by the Administration) with grave concern and serious reservations. . . . We question whether this nation can afford to place full dependence on an operationally untried and untested missile force . . .²⁵

Budget reductions also have been explained in terms of the Congress’s understanding of the requirements of national security. Senator Richard Russell articulated his uneasiness about procuring the fast deployment logistics (FDL) ships requested by the Johnson Admin-

²² Dexter, p. 306.

²³ U.S. House of Representatives, Committee on Appropriations. *Report on Department of Defense Appropriations Bill for Fiscal Year 1960*. 86th Congress, 1st Session, Report No. 408, p. 7.

²⁴ U.S. House of Representatives, Committee on Appropriations. *Report on Department of Defense Appropriations Bill for Fiscal Year 1960*. 86th Congress, 1st Session, Report No. 408, p. 7.

²⁵ Quoted in *Congressional Quarterly Almanac*, 17 (1961), 146.

istration: "If we build anything like this, we are just going to be handed more and more of this business of fighting everybody's wars everywhere."²⁶ The belief that an increased ability to intervene would result in greater U.S. intervention abroad contributed to Congress's regular refusal to fund the FDL's.

It should be noted, without judging the sincerity of conviction underlying the rhetoric, that Congress has publicly explained individual budget actions affecting particular weapons systems programs in terms of their relation to national security.

The distribution of Congressional activity among budget categories indicates a programmatic orientation toward the defense budget. That orientation also should be reflected in Congressional responses to perceived changes in the international environment. Variations in the level of Congressional budget activity during the past eleven years should be associated with prominent events relevant to U.S. defense policy.

The mean absolute difference of the 28 line-items for each year is illustrated in Figure 1. As can be seen, Congressional activity was relatively high in both the beginning and the end of the period while the middle years were characterized by somewhat fewer or smaller changes by Congress in the President's budget request.

Explanations for the two dramatic changes which are observed may be divided into two general classes: (1) efforts to modify policies in response to perceived changes in the international environment, and (2) considerations of domestic politics reflected in Presidential elections. Unfortunately, not only is there a substantial interrelationship between the two—perceived changes in the international situation usually are reflected in their consequences for the domestic political scene—but changes of both sorts occurred nearly simultaneously during the time period under study. Nevertheless, the observed trend is consistent with the "programmatic" explanation suggested above.

The relatively high level of Congressional activity in the beginning of the period occurred during the last two Eisenhower defense budgets of fiscal 1960 and fiscal 1961. It was a period of the "missile gap" and the leaking of the Gaither Committee Report, when the doctrine of massive retaliation was losing its domestic credibility, and the nation appeared to be militarily unprepared. Congressional activity,

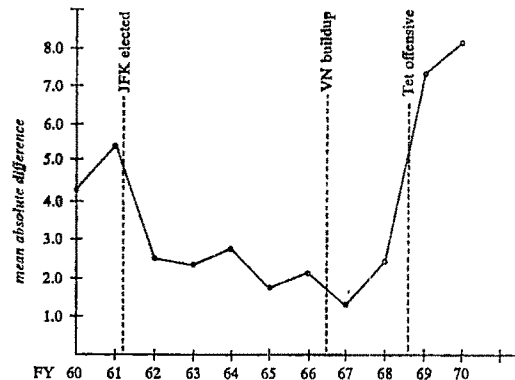


Figure 1. Level of Congressional Activity

which mostly took the form of increases in the President's budget requests, may have been a response to these changing perceptions of the security of the United States. As Goss concluded:

[Congressional] increases to weapons procurement requests have been associated with the perception of an increased threat and therefore have been justified on the grounds of needs of national security.²⁷

The downturn in Congressional activism or the defense budget coincided with the first Kennedy budget (actually an amended Eisenhower budget). Perhaps the newly elected Democratic Congress was simply granting the requests of the narrowly elected Democratic President. But it also is true that the first JFK budget was highly responsive to the perceived shortcomings of the existing defense posture; substantial increases in *both* nuclear strategic and conventional forces were requested of the Congress. There was little left for Congress to change, and Congressional activism accordingly declined. The several international crises which punctuated these years also may have stimulated a "rally 'round the flag" attitude in Congress, making it reluctant to challenge the President on defense matters.

The level of Congressional activism declined further with Johnson's and the Congressional Democrats' landslide victory in 1964. There was, in any case, little indication of Congressional dissatisfaction with the President's defense budget requests: The budget was high but relatively stable, our superiority over the Soviets seemed assured, and, with the signing of the Limited Test Ban Treaty and reciprocal reductions in defense spending, the era of *détente* ap-

²⁶ U.S. Senate, Committee on Appropriations, *Hearings on Department of Defense Appropriations for Fiscal Year 1968*, Part 1. 90th Congress, 1st Session, 5(1968) 7, 359–60.

²⁷ Goss, "Congress and Weapons Procurement," p. 13.

Table 7. Distribution of "No Change" Outcomes, by Fiscal Year

Fiscal Year	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970
%	10	06	12	06	10	12	19	13	09	01	01 = 100%
N	(7)	(4)	(8)	(4)	(7)	(8)	(13)	(9)	(6)	(1)	(1) = (68)

peared to be dawning. In its review of the FY 1965 defense budget, the House Appropriations Committee observed: "This year, more so than in many previous years, the Committee has adopted to a large degree the program presented in the budget. . ."²⁸

Congressional activism declined to its lowest level in the budgets immediately following the large-scale buildup of U.S. forces in Vietnam. The small and infrequent Congressional changes in the defense budget during this period may have reflected its confidence in the conduct of the war. The dramatic increase in Congressional activity (which reflected sharp reductions in the President's budget) occurred in the first defense budget to follow the Tet offensive of January, 1968. Explanations for this change in Congress's budget activity seem to be uncomplicated by partisan considerations: The 1968 elections were still several months away, Johnson had already removed himself, and the level of Congressional activism persisted after the election into the first Nixon defense budget.

These variations in budgetary activism may reflect Congress's changing assessment of the President's handling of national security. It is as if when Congress's confidence in the President is high, it makes few and minor changes in his budget. Larger and more frequent changes in the President's budget requests may signal Congress's dissatisfaction with his performance and its determination to take a more active role in defense matters.

The distribution of "No Change" outcomes, seen in Table 7, supports this general picture. The incidences of "No Change" are concentrated in the middle of the period when, it appears, Congress was prepared to support the President in his conduct of defense in general, and the war in Vietnam in particular. Of the "No Change" outcomes, 44 per cent are concentrated within the 27 per cent of the period represented by the Vietnam buildup (fiscal 1965-1967).

Congressional confidence in the President was waning in Eisenhower's last year in office, and under Johnson, was severely jolted by the

experience of Tet. As can be seen in Table 7, Congress made frequent changes in the President's request during those years. Only 8 per cent of the "No Change" outcomes occurred during that 27 per cent of the period (FY 1961, 1969, 1970). In fact, only one minor line-item in twenty-eight was left unchanged in fiscal 1969 (Navy Reserve Personnel) or fiscal 1970 (Marine Corps Reserve Personnel).

The trend of Congressional activity over time supports the view that budget actions are inspired by program preferences. While Congress occasionally has indulged in across-the-board reductions to "encourage more efficient management," most of its decisions are based on judgments relating weapons systems to its perception of the nation's defense requirements. Thus, many of the changes made in the last Eisenhower budgets were designed to achieve greater "balance" in our force structure to enable the U.S. to fight non-nuclear wars. In fact, Congressional budget increases earmarked for Army modernization and airlift may be viewed as anticipating, by two years, JFK's efforts to move to a strategy of "graduated response."

Likewise, Congress's refusal to fund the fast deployment logistic ship program (FDL's), was publicly justified in terms of relating weapons programs to national defense policy. Critics may not applaud Congress's policy choices, but it does seem that Congress has, in the recent past, made such deliberate program decisions.

The House and Senate in Defense Appropriations

If it is true that Congress can and has made budget decisions based on program preferences, it becomes important to identify the relative importance of the two Congressional actors in the appropriations process, and to locate particular points of leverage available to an extra-Congressional participant seeking to influence the budgetary outcome.

Since the appropriations agreed to in conference and enacted into law are the culmination of Congress's role in the budget process, it might seem reasonable to judge which house is more dominant by comparing how frequently

²⁸ Quoted in *Congressional Quarterly Almanac*, 20 (1964), 156.

the conference report is equal to the Senate appropriations with how often it is equal to the House figure.²⁹ As can be seen in Table 1, the conference report is equal to the appropriations voted by the Senate about three and a half times as often as it is equal to the funds voted by the House.³⁰ On this basis, the locus of power and focus of effort seem obvious.³¹

However, as Fenno has cautioned: "... dominance in the conference committee must never be confused with dominance in the appropriations process as a whole."³² It must be remembered that matters are raised in conference if and only if the Senate has made changes in the House version of the bill; issues to be resolved in the conference may be no indication of the extent to which the President's request has been changed.³³

An alternative approach to this problem is to begin by comparing the number of individual changes³⁴ the House makes in the President's request with the number of Senate alterations of the House version. Changes made by both

houses were examined for a subset of the data comprising the period FY 1966 through FY 1970.

Most of the changes which appear in the final Congressional version of the Defense appropriations bill were made by the House. The Senate made 161 individual changes in the House appropriation bills during the fiscal years 1966 through 1970. The House, however, made 344 separate changes in the President's budget *which were accepted by the Senate* during its consideration of the defense appropriations. That is, for every change made by the Senate in the House bill which required consideration by the conference, *two* House changes, accepted by the Senate, became part of the final appropriation without ever entering into the conference bargaining process. Only about 30 per cent of all Congressional changes were reviewed in conference. Thus, based on the number of changes in the president's request the House plays the larger role in Congress.

Part of the explanation for this finding lies in the differences between the self-defined roles of the House and Senate in the budget process. As Fenno has noted in his study of domestic spending, the Senate Appropriations Committee does not scrutinize the entire budget request, as does the House. With its more limited resources, the Senate tends to concentrate on changes made by the House and primarily on those House actions that are appealed by the agency concerned.³⁵ "When an agency does not appeal a House cut, Senate appropriations committee members will take no further notice of it."³⁶

In nearly every case in which the Senate did not respond to a House change, the DOD previously indicated acceptance of that change and requested of the Senate the amount appropriated by the House: Of the 344 House actions accepted by the Senate, 306 were instances in which there was no DOD reclama (appeal). Although most of the changes in the appropriations bill are made by the House, the House's role is, in one sense, influence by default. It may be accurate to say that the House dominates the Congressional appropriations process (since most changes in the President's request occur in the House), but this does not mean that the House prevails in its struggle with the Senate. The fact is that there are minimal opportunities for differences. The incidence of DOD reclaims, and the factors leading to the executive branch's decision to appeal a particular House change, emerge as central elements

²⁹ For a similar procedure, applied to nondefense spending, see Fenno, *Power of the Purse*, pp. 661-670.

³⁰ This is somewhat greater Senate dominance than Fenno observed for nondefense spending (p. 663).

³¹ Cf. David J. Vogler, "Patterns of One House Dominance on Congressional Conference Committees," *Midwest Journal of Political Science*, 14 (May, 1970), 303-320.

³² Fenno, p. 663.

³³ There is another problem involved with the casual comparison of conference outcomes with House and Senate dollar amounts. Consider the following hypothetical example: (1) the House makes four individual cuts in a particular line-item; (2) the Senate makes no changes in any of the four decisions taken by the House, i.e., accepts the House's four cuts, but it reduces the President's request for another individual component of the same line-item; and (3) only the Senate's change is considered in conference (since that is the only difference between the bills of the two houses), and the House yields to the Senate. The result would be that the Senate appropriated less money than the House, and the appropriation to emerge from the conference would be exactly equal to the Senate's, i.e., it would be scored as a "straight Senate victory." It seems inappropriate, however, to talk about a Senate "victory" when there seems to have been little if any conflict between the two houses. The Senate accepted all of the changes proposed by the House, and the House, in turn, accepted the change proposed by the Senate. While, strictly speaking, the Senate did dominate the conference—the House did yield to the Senate—it cannot be said that the Senate dominated the Congressional appropriations process.

³⁴ Each of the 28 line-items considered is in reality a composite of several individual decisions. Thus, Congressional changes in budget requests for new destroyers, aircraft carriers, and attack submarines, appear, in aggregate, as a Congressional change in the line-item "Shipbuilding and Conversion, Navy," which in turn appears under the Procurement Appropriations title.

³⁵ Fenno, p. 534.

³⁶ Fenno, p. 569.

in any explanation of Congress's treatment of the defense budget.

The most obvious fact is that the Defense Department does not appeal the majority of House changes. The general reluctance to appeal may be due in part to a desire to minimize friction with the Congress and a belief that challenging all or most House changes, as a matter of course and regardless of importance, will probably antagonize the Senate and will certainly antagonize the House. There is probably a general rule to restrict *reclamas* to cases of serious damage to very important budget items.³⁷

All budget reductions are not equally detrimental to the interests of the military services (where the initiative to appeal usually resides). Although the House was very likely to cut most of the line-items in the President's request during the 1960s, these actions were not necessarily antagonistic to the priorities of the services. The services did not always strongly resist cuts in their budgets, e.g., conventionally powered guided missile destroyers and fast deployment logistic ships. Moreover, such reduction in low-priority items (low from the perspective of the service) frequently obscured increases over the President's budget in particularly esteemed service programs, e.g., nuclear powered guided missile frigates and "quiet" submarines. Establishing which of the houses is the more generous patron of the services cannot be done simply by comparing the size, in dollars, of Congressional budget changes.

There is undoubtedly also, the simple rule to appeal only when you think it will do some good. Since most House changes were *decreases* in the President's request, a *reclama* to the House decision represented an appeal to the Senate to restore the cut. The Senate usually could be depended upon to increase the House appropriation through FY 1966. Beginning in FY 1967, however, the Senate took a more critical attitude toward defense expenditures

³⁷ One reader of an earlier version suggested that the House Committee decides on the dollar amount of the contemplated reduction and then informally negotiates with the military services about which parts of the budget will absorb the cuts. Once an agreement has been reached, appeals to the Senate to restore the cuts would represent a default on the bargain; hence the low incidence of *reclamas*. Implicit in this line of argument is the hypothesis that Congress has no independent policy preferences but merely acts as a conduit for another's priorities, in this case the military services. The strong version of the hypothesis, viz., services distribute budget reductions among themselves in proportions equal to their shares of the defense budget, was not supported by the record of the past eighteen years. The general notion, however, is consistent with the data and certainly warrants further investigation.

and the House figure exceeded the Senate appropriation for several of the line-items. By FY 1969, the House was voting larger appropriations than the Senate for most line-items. In view of this change in the temper of the Senate, *reclamas* may have decreased in number simply because they were perceived to be futile exercises.³⁸ (The recent inclination of the Senate to vote *less* money than the House may require a serious modification of any models of the budgetary process which assume that the Senate restores House cuts.)

It is clear that the House is responsible for a larger *number* of changes in the President's request than is the Senate. But this is not necessarily to say that the House has made the most *important* changes. It can be argued that the issues that must be resolved in the conference committee are primarily those in which the actors are most interested and to which they are most intensely committed. Identifying the house which was more successful in conference may indicate which house was more influential when it mattered. We therefore return to our comparison of the conference appropriations with the House and Senate versions.

As was noted above, the dollar amount recommended in the conference report was equal to the Senate appropriations about three and a half times as often as to the House figure. It may be argued, however, that occurrences of such "straight" House and Senate "victories" are more indicative of weakness of commitment by one house or the other, rather than of relative political power and domination. "Compromises" may signal an intensity of commitment in each house sufficient to require bargaining and reconciliation in the conference.

Cases of compromise shown in Table 3 were allocated to either the House or Senate according to which house's appropriation bill was closer (in dollar amount) to the appropriations recommended in the conference report.³⁹ These allocated compromises combined with the straight House and Senate victories are shown in Table 8. When compromises are included, the gap between the houses narrows to the point where the conference report is closer to

³⁸ If this is true, then the data subset for the period FY 1966-FY 1970 may be unrepresentative of the total time period. A less rigorous analysis of individual budget decisions for the earlier period, however, yields the same general picture.

³⁹ Comparing dollar amounts is a crude and may be a misleading indicator of a victory in the conference committee: "If, for example, one group is willing to give up an item costing \$50 million to get some especially valued item costing \$10 million, then dollars and cents measures will be inadequate to define winning" (Fenno, p. 661).

Table 8. Distribution of Appropriation Outcomes, with "Compromises" Divided between House and Senate

Conference Closer to:	Personnel	O&M	Procurement	RDT&E	Total
House	06%	16%	28%	12%	15%
Senate	27%	46%	38%	48%	37%
	(109*)	(63*)	(87*)	(42*)	(301*)

* In seven cases, the conference report was equidistant from the House and Senate appropriations.

the Senate figure a little more than twice as often as the House figure.

We have already seen, moreover, that certain budget categories appear to attract more Congressional interest than others. Based on the high concentration of "No Change" outcomes and the infrequent occurrence of compromises, the Personnel category was judged to attract rather low attention in Congress. Yet nearly half of the Senate's straight victories concerned Personnel matters (See Table 3). Thus, the appearance of overwhelming Senate domination is largely the result of almost complete control of that budget category which experiences the fewest and smallest Congressional changes.

Although Senate victories outnumber House victories in all four budget categories, the gap between the two houses narrows considerably in the case of the Procurement budget, the category in which all of the participants appear to have the greatest stakes. Moreover, House victories were heavily concentrated in Procurement (index of concentration = 1.83). Thus the House appears to concentrate its efforts and successes where it matters most to most of the actors.⁴⁰ Conversely, the preponderance of Senate victories in conference is, to some extent, inversely related to the importance of the budget category.

In view of this analysis, the finding that Senate victories substantially outnumber House victories must be qualified. The Senate largely controls those parts of the budget which are least controversial. House victories are concentrated in the two areas which the participants appear to consider more important, and are nearly equal to the Senate in the most important appropriations title, viz., Procurement.

The 161 individual differences reconciled in conference between FY 1966 and FY 1970

were analyzed in order to probe the relative influence of each house in the conference committee. The most striking feature of these 161 differences is the surprisingly small number of "split-the-difference" compromises which occurred. Only twenty of the individual decisions represented such compromises. In 80 per cent of the cases, either the exact House figure or the exact Senate figure was the appropriation which emerged from the conference.⁴¹ It appears from these figures that *logrolling* is the dominant form of conflict resolution in the conference.⁴² The House either accepts the Senate position, or the Senate accepts the House position; very rarely do they fix on a figure which is neither. Conflict is resolved by trading concessions on separate individual decisions. At the level of the individual budget decisions, there are very few compromises and a great preponderance of straight victories. What appear to be "split-the-difference" compromises at the level of the line-items are usually the sum of the House victories and Senate victories on the component decisions of that line-item.

Analysis in terms of House or Senate "victories" connotes an atmosphere of struggle between the houses over particular items. Fenno's description of the conference highlights the participants' expectations of conflict.⁴³ But the preponderance of logrolling solutions suggests that conferences on defense appropriations may be less stressful, more friendly encounters than one might expect.⁴⁴

⁴⁰ In thirteen cases, the Senate accepted a House change and then added to it, the sum of the two actions being the appropriation reported by the conference. Thus, the House may have cut \$100 million from the President's request and the Senate an additional \$50 million from the House bill. The final appropriation would represent a decrease of \$150 million from the President's budget request. Although the case could be made that such outcomes ought to count as Senate victories, they were classified separately in this analysis in the "both" category.

⁴¹ See Wildavsky, *Politics of the Budgetary Process*, p. 61: "One way to secure agreement is to swap items in dispute."

⁴² Fenno, p. 625.

⁴³ Cf. Jeffrey L. Pressman, *House vs. Senate: Conflict in the Appropriations Process* (New Haven: Yale University Press, 1966), p. 61.

⁴⁰ It seems reasonable to credit to the House those of the outcomes labeled "same" which were accepted by the Senate, even though the Defense Department appealed them. If this is done, House "victories" are concentrated in the two most "important" budget categories. Moreover, the gap between Senate victories and House victories is substantially narrowed both in Procurement and RDT&E.

The Senate's record of victory in the conference committee is impressive. Appropriations reported out of the conference were closer to the Senate figure because the House usually yielded to the Senate's position at the level of the individual budget actions. The House accepted the Senate figure 57 per cent of the time (or 65 per cent if cases in which the Senate added to a House change are included) out of the total 161 differences. The Senate accepted the House figure in only 23 per cent of the cases.

Although House victories and instances of compromise were concentrated in those cases in which the Defense Department made *reclamas*,⁴⁵ the Senate prevailed more often than the House in *every* category considered. Moreover, the Senate was inclined to act on its own initiative, without the stimulus of a Pentagon appeal: Fewer than half the Senate changes were preceded by a *reclama*. When the Senate changed the House version of the defense appropriations bill, those changes were very likely to appear in the public law submitted for the President's signature.

Although the House made many more changes in the administration bill, the Senate has not been a second-class participant; the latter usually has had its way in conference. Although a significant proportion of the Senate's victories were on items to which neither the House nor the executive branch were intensely committed, the Senate did prevail more often than the House in every category, from least to most important. Any confusion on the part of the would-be activist seeking points of leverage is justifiable. Nevertheless, the Senate's record of success in the past, combined with its recent history of economy and skepticism, support the observation that "Senate deviations from the norm seem to be the bellwether of developing trends."⁴⁶

Summary and Conclusion

The widely held notion that an uncritical Congress passively grants whatever appropriations are requested by the President and the Pentagon, does not do justice to the reality of the pattern of the Congressional changes in the

defense budget, nor does it illuminate the past and potential Congressional role in influencing the content of defense policy.

Defense budget requests in the 1960s appeared to emerge unscathed from the Congressional appropriations process primarily because Congress made small and infrequent changes in the Personnel and O&M titles which together constitute more than half the budget. Congress has focused its energies and concentrated its attention on Procurement and RDT&E—those parts of the budget primarily concerned with present and future weapons systems. This Congressional activity has been obscured in discussions based on total DOD annual budgets.

Two classes of speculations for the observed distribution of Congressional activity have been offered: budgetary and programmatic. It is the programmatic explanations that have the greatest potential significance for defense policy. Congress's changes in the defense budget can readily be explained in terms of its dissatisfaction with defense posture and policies, and its substantive efforts to improve them. During the 1960s, Congressional policy preferences (which prevailed more than once) were different from those of the President and often resembled those of the military services. Indeed, case studies would probably reveal that the military has been the source of Congress's defense policy objectives.⁴⁷ But the lesson for the future is that in voting defense appropriations, the Congress is concerned about more than *how much* money is spent. It also has evidenced a willingness and desire to influence the content of national security policy. Congress in the 1960s did not docilely accept and ratify the defense policy decisions of the executive. On the contrary, it played a relatively lively role in particular policies, an activism whose level seemed to be associated with a perceived inadequacy of presidential decisions and actions. Congress can influence in the future, as it has influenced in the past, the *content* of defense policy by means of its control of appropriations.

Most of the changes made in the President's budget request by the House were accepted by the Senate, were never discussed in the conference, and were retained in the final bill. This would make it appear that the defense priorities of the House are likely to be those which prevail in the final Congressional act. We have

⁴⁵ It would be inaccurate to posit any direct causal links between the presence or absence of a Defense Department *reclama* and the probability of a Senate victory in conference. The existence of a *reclama* is more likely to indicate that the matter under consideration is relatively important to the Pentagon; it probably is relatively important to the House as well. The presence of House victories and compromises probably indicates a greater intensity of commitment to its position and less willingness to yield to the Senate.

⁴⁶ Goss, p. 12.

⁴⁷ As the *Congressional Quarterly Almanac* observed in 1961: "The Senate Appropriations Committee's actions on long-range bombers matched almost exactly recommendations made to the Committee by Air Force Chief of Staff Curtis E. LeMay . . ." p. 146.

seen, however, that most such House actions (at least during the latter part of the decade) went unappealed by the Pentagon. The relative dominance of the House in the Congressional appropriations process becomes more difficult to judge in such circumstances. Moreover, when the Senate did make changes in the House bill, the House tended to yield to the Senate's position in the conference. In addition, the very recent change from the Senate's traditional role of dependably restoring House cuts to its new posture of being even more critical than the House of defense spending injects a further element of unpredictability into House-Senate relationships.

Congressional policy preferences, and rela-

tions among the houses in the course of the appropriations process, appear to be in a state of flux. Consequently, the record of the past decade may be a poor indicator of which house is likely to be more sympathetic to a reordering of defense priorities as well as which house is likely to be more influential in determining the content of final appropriations bill. The record of the 1960s does reliably indicate, however, that influencing the direction of defense policy is not a novel role for the Congress. The inclination and mechanism for Congressional intervention in matters of national security already are present. The direction and results of that inclination have yet to be determined.

Appendix

Line-items Included in Analysis

Line-items Excluded from Analysis

MILITARY PERSONNEL

Military Personnel, Army
Military Personnel, Navy
Military Personnel, Marine Corps
Military Personnel, Air Force
Reserve Personnel, Army
Reserve Personnel, Navy
Reserve Personnel, Marine Corps
Reserve Personnel, Air Force
National Guard Personnel, Army
National Guard Personnel, Air Force

Retired Pay, Defense

OPERATIONS AND MAINTENANCE

Operation and Maintenance, Army
Operation and Maintenance, Navy
Operation and Maintenance, Marine Corps
Operation and Maintenance, Air Force

Operation and Maintenance, Defense Agencies

Operation and Maintenance, Army National Guard
Operation and Maintenance, Air National Guard

National Board for the Promotion of Rifle Practice,
Army
Claims, Defense
Contingencies, Defense
Court of Military Appeals, Defense

PROCUREMENT

Procurement of Equipment and Missiles, Army
Procurement of Aircraft and Missiles, Navy
Shipbuilding and Conversion, Navy
Other Procurement, Navy
Procurement, Marine Corps
Aircraft Procurement, Air Force
Missile Procurement, Air Force
Other Procurement, Air Force

Procurement, Defense Agencies

RDT&E

RDT&E, Army
RDT&E, Navy
RDT&E, Air Force
RDT&E, Defense Agencies

Emergency Fund, Defense

A Theory of Political Ambition: Career Choices and the Role of Structural Incentives*

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Introduction

Political ambition lies at the heart of much of the political "lore" in the United States. As the politician rises to new positions of power and prominence, his motives for seeking political advancement become subjected to intense public scrutiny. Most politicians try to promote the fiction that their motives are unsoiled by private ambition, that all they wish is to serve the public and to pursue the "public good"; but in the end, almost all American politicians appear somewhat suspect to the public at large, suspect in ways one seldom associates with corporation executives or those in other careers.

With all the public concern about excessive ambition, however, political ambition has not been a primary research subject for recent students of politics. There is, of course, a tradition in the discipline of political science of studying famous politicians, frequently through case studies that employ psychoanalytic methods, and we have, in addition, some rather extensive empirical findings about the "social backgrounds" of political leaders, but both approaches have failed to lead to an accumulation of knowledge about political ambition.¹

The most significant exception to the general neglect of political ambition is the work of Joseph A. Schlesinger.² In his seminal study of political recruitment, Schlesinger suggests:

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¹ One of the best examples of the "case study" approach of famous politicians is Alexander L. George and Juliette C. George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: Doubleday, 1956). Studies of the social backgrounds of decision makers are quite common. See, for example, Donald R. Matthews, *The Social Background of Political Decision Makers* (New York: Doubleday, 1954); and W. Bell, R. J. Hill, and C. R. Wright, *Public Leadership* (San Francisco: Chandler, 1961).

² Joseph A. Schlesinger, *Ambition and Politics: Po-*

The central assumption of ambition theory is that a politician's behavior is a response to his office goals. Or, to put it another way, the politician as office-seeker engages in political acts and makes decisions appropriate to gaining office. . . . It makes little difference to the theory of ambition whether men adopt the ambitions suitable to the office or attain the office because of their ambitions. It is sufficient to conclude that governors of New York will behave as though they were Presidential candidates while governors of Mississippi or South Dakota will not.³

Following an extremely insightful, yet introductory, analysis of the importance of political ambition, he proceeds through a careful and extensive examination of the recruitment patterns among the various offices in the United States. At no point, however, does he really focus on the actual interaction between the structure of offices and the ambitions of individual politicians. He thus leaves us with a strong case for the importance of political ambition but with little evidence of how it develops.

His work, however, does suggest several ideas that may prove of great value in the search for "theories" of political ambition. One such idea is his contention that a man's political motives and desires are molded by the availability of political "opportunities," and that such opportunities are structurally determined.⁴

litical Careers in the United States (Chicago: Rand McNally, 1966). Several other exceptions to the general neglect of ambition include Heinz Eulau *et al.*, "Career Perspectives of American State Legislatures," *Political Decision-Makers: Recruitment and Performance*, Dwain Marvick, ed. (The Free Press of Glencoe, 1961); James David Barber, *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965); and Chong Lim Kim, "Political Attitudes of Defeated Candidates in An American State Election," *American Political Science Review*, Vol. 64 (September, 1970), pp. 879-887.

³ Schlesinger, pp. 6, 9.

⁴ Schlesinger finds considerable consistency in the career patterns that politicians follow from state to state. He finds that some offices are substantially more likely to promote political advancement than other offices, and he characterizes these differences as the "opportunity structure" of the political system. Although one cannot prove it with these data, I suspect that the ambitions of individuals are strongly shaped by the availability of opportunities, and that this effect is manifested in the probability estimates that politicians assign to various alternatives.

The critical element here is the idea that a political structure acts on the choices of individuals so as to systematically affect all political office-seekers. Schlesinger's primary concern is with what he terms the "opportunity structure," and if the "opportunity structure" does have such a profound effect on the calculations of politicians, it is possible that other structural variables exert a similar systematic effect on these calculations.⁵

A corollary of the "structural effects" idea is the suggestion that the office-holder responds primarily to the immediate forces in his political environment rather than to factors that occur in the more distant past of the political actor. The average politician can hardly afford to develop elaborate plans concerning his political life. Rather, he probably tends to make decisions on the basis of the costs, benefits, and probabilities that operate at the time of his decision. If this is the case, then one should pay much more attention to the immediate circumstances surrounding a politician at the time of a decision rather than some set of factors in his childhood or elsewhere.⁶

These two ideas suggest the basis for a theory of political ambition. The theory rests on the idea that office-seekers attempt to behave in a rational manner in selecting among alternative offices; that rather than being driven by excessive ambition, they tend to develop ambition slowly as a result of their changing circumstances. The theory is partial, of course, because it excludes many variables that undoubtedly affect the development of political ambition in individuals; particularly those factors which move an individual to seek his first political office. The theory does, however, focus on a set of calculations and processes that appear to be central to the development of ambition in the politician.

The Setting for the Study

This is a study of 435 city councilmen in the San Francisco Bay Area, and the focus of the study is on the decision of councilmen either to retire from office, to seek reelection, or to seek other political or governmental offices. The

⁵ While Schlesinger can chart recruitment patterns, his data do not permit an examination of ambition directly. The study of political ambition is the study of the motivations of politicians and hence requires data on individuals.

⁶ Such background factors are obviously important, but it remains to be seen whether their cumulative effect on a politician is very great. They are probably outweighed by more immediate concerns, particularly those related to the choices that are available at the time a potential candidate must make his decision.

study is based on data collected for the City Council Research Project, and it includes information gained through an extensive interview with the city councilmen and information on the political and demographic characteristics of the cities in which they hold office.⁷

All of the respondents in this study were officeholders at the time the research was conducted, and thus they had already exhibited a level of political involvement that is considerably higher than that of the average citizen. At the same time, I only have data on officeholders at one level of the political structure, the city council, and these city councilmen are probably not, on the whole, as ambitious as others who hold higher political offices. These considerations impose an obvious methodological problem: the data only reflect a restricted range of the variables under consideration. Restricting the range of one variable in an analysis normally has the effect of reducing the correlation between that variable and the other variables in the analysis. The implication here is that these tables will tend to underestimate the actual relationship one would find if the analysis was based on the full range variables. This point will be reiterated later because it poses some problems for interpreting these results.

The Rational Office-Seeker

The model employed in this analysis assumes that individuals attempt to behave in a rational manner, and that includes politicians who are in the process of seeking political advancement. The assumption of rationality, although generally accepted in economics, still provokes a strongly negative reaction among many political scientists, largely, it appears, because they do not understand the technical use of the term, as in game theory. The assumption of rationality in this context implies that the polit-

⁷ These data include open and closed items on the interview, census data, and election statistics gathered from each city. This research is, in several important respects, parallel with some findings reported earlier by this author; "A Theory of Professionalization in Politics," *American Political Science Review*, Vol. 64 (September, 1970), pp. 865-878. That article also employed a utility model, but the point of the article was to show that politicians tend to adopt attitudes that facilitate their ambitions, what we termed instrumental attitudes. The logic behind this phenomenon was similar to that reported here: some attitudes increase the probability of success in a career sequence, thus increasing the rate of return that an individual can expect from that sequence. In this article, however, our interest is the phenomenon of ambition itself, and our goal is to show how the political structure of a community can affect the level of ambition expressed by officeholders in the unit.

clan, when confronted with a decision, will examine the alternatives with which he is confronted, that he will evaluate those alternatives in terms of the likelihood of their occurrence and the value they hold for him, and that he will choose that alternative which yields for him the greatest expected value.⁸ In this formulation, one assumes that the individual is attempting to select the best alternative for himself, i.e., that alternative which maximizes his expected value, but he may not, of course, always be correct in his choice.

Politicians, like everyone else, make errors; errors that can derive from an incorrect estimate of the probabilities attached to various alternatives or from an incorrect evaluation of the costs and benefits attached to a given alternative. These errors arise because information is almost always limited and expensive, and the decision-maker must generally arrive at his decisions through the process of an educated "guess." Thus, politicians can err in their choices even though they are trying to behave rationally.

In outlining the calculations of the rational office-seeker, we must begin by summarizing the basic variables in the decisional context:

B = the benefit, in *utils*, that an individual will receive from achieving an office.⁹

P = the candidate's estimate of the probability that he can obtain an office should he attempt to seek it.

C = the cost required during a campaign to obtain an office.

$u(0)$ = the utility of an office for the individual prior to the election.

This calculus for the potential candidate is represented by the following statement:¹⁰

$$u(0) = (PB) - C$$

and the potential candidate will find it reason-

⁸ For a detailed explanation of the approach, see R. D. Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley and Sons, 1957).

⁹ The term "utile" refers to the abstract metric used in utility theory; and its value is specified in terms of the other abstract components of a theory, not in terms of some specific empirical measure such as dollars.

¹⁰ The form of this function is a simplified version of what is a much more complex set of calculations. One simplification is that we are treating the calculus as if (P) and (C) are independent variables, which they most certainly are not. On the whole, the more a candidate spends on his elections, the more likely he is to win, that is, it might be rational for a candidate to increase his investment because it increases his probability of success at the polls. For our purposes, however, the interdependence between these two factors is less important because the analysis here is primarily concerned with the structural conditions that affect the value of (C) so as to establish

able to seek office under the following conditions:

Condition I: The benefit of office-holding, (B), times the probability of obtaining the office, (P), is greater than the cost of obtaining that office, (C).

Condition II: The utility of office-holding, $u(0)$, is more valuable than the alternative ways the individual might invest his resources, i.e., $u(0) > u(A_1)$.

The first condition simply states that the expected utility of officeholding must be positive, while the second indicates that the utility of officeholding must be greater than the utility of the available alternatives. Both are quite obvious conditions, and they tell us little about the actual calculations of politicians. What is of interest here are the costs attached to seeking political offices, particularly those costs that are determined in part by the structural characteristics of the political system in which the individual seeks to run.

Campaign Costs and Political Structure

It is obvious that the rewards and costs of running for office in a city vary with a great many factors. Some factors, such as an individual's previous public prominence or an individual's need for public attention, are idiosyncratic, and their effect on the calculus of the individual is probably randomly distributed across cities. At issue here, however, are two factors that are structural in character—the size of a city and the degree to which a city's elections are competitive—and they probably have a systematic effect on the calculations of most of the individuals who seek public office in cities.¹¹ If their effect is systematic, i.e., they affect most individuals who seek public office, they may have a pervasive and critical impact on the social choice process that leads some men into the pursuit of public offices and leads other men to avoid public office.

In this regard the size of the electoral unit and the degree of electoral competition in that unit weigh heavily in determining the relative costs for candidates who seek office. Both the

some minimal value point below which (PB) cannot fall. The rather severe limitations in the type of data we have do not permit the more sophisticated and complicated analysis that would be required if we treated (P) and (C) as if they were interdependent.

¹¹ There are other structural characteristics which might similarly affect the recruitment of officeholders; nonpartisanship is one such characteristic, but it is not relevant here because all of the respondents were elected under nonpartisan forms of election. If some of the cities had had partisan elections, we would have had to take that factor into consideration.

Table 1. Relationship of Estimated Campaign Expenses to City Size, to Electoral Competitiveness and to Electoral Competitiveness Controlling City Size¹³

Estimation of Campaign Expenses	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
\$ 0-499	5	78		44	81	
\$ 500-999	16	16		23	8	
\$1000+	79	6		33	11	
Respondents	(105)	(273)	(378)	(223)	(123)	(346)

Estimate of Campaign Expenses	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
\$ 0-499	4	—		57	81	
\$ 500-999	12	—		22	13	
\$1000+	84	—		21	7	
Respondents	(83)	(14)*	(97)	(141)]	(119)**	(260)

* Too few cases.

** Rounding Error.

size of the community and the competitiveness of community elections should be related to the resources such as money, time, effort, etc., that candidates will have to spend on campaigning. This relationship can be partially substantiated with information gained from the interview with the city councilmen. In Table 1 both variables are correlated with the councilmen's estimates of the amount of money generally spent on campaigns in their cities. Both variables can be seen to be clearly related to the estimates of the amount of money that candidates for the councils spend on their campaigns.¹²

¹² The purpose of this table and the next is to establish that a strong relationship exists between the costs of running for office and the two structural variables. We have employed simple dichotomies and trichotomies because a more detailed analysis of this relationship is unfeasible for our purposes. There are two reasons for this. The more important of the two is that we cannot aggregate "costs" into a single index because they are not convertible into a common unit. In addition, it seems unwise to overestimate the accuracy of this kind of recall data.

¹³ In order to test this proposition we divided the cities into two categories, those above 30,000 in population and those below that number of people. The selection of 30,000 as a cutting point was an arbitrary decision: the only real concern was to choose a point that seemed large enough to separate out the cities in which the costs of running for office were significant, but small enough so that we had sufficient councilmen in the large cities for the analysis. The measure of competitiveness for the city was the closeness of the vote among the various candidates for office. This measure was obtained by calculating the mean deviation of the vote among all the candidates who received more than 15 per cent of the vote. The *mean*

The same pattern of relationships can be found when the "cost" variable becomes the councilman's estimate of the amount of campaigning that generally occurs in his city. These relationships are shown in Table 2.

Two things stand out about these tables. In the first place, both of the "costs of campaigning," money and effort, are strongly associated with city size and with the degree of competi-

deviation (or absolute deviation as it is sometimes called) is computed by determining the sum of the absolute deviations from the mean number of votes per candidate and then dividing that sum by the number of deviations. The mean deviation is a measure of dispersion around the mean, but it differs from the standard deviation in that it weights every deviation the same, rather than weighting extreme scores more heavily. In almost all cases, the measure of the closeness of the vote, the mean deviation, was determined from five elections in each community, and the average was taken and is used here.

These mean deviation scores for five elections in each city were averaged to obtain a measure of the closeness of the vote for the city during a ten-year period. The Councilmen were then classified according to whether the city in which they were elected was "high" or "low" on the measure of the closeness of the vote. Virtually all of the cities are, in effect, multiple member districts. There were, on the average, more than two candidates for every office, and there were almost always more than two councilmanic positions at stake.

The dividing point between the "high" and the "low" competitive was the average mean deviation of 13 per cent. The range for the cities was between a low of 8 per cent on the variable and a high of 18 per cent. This cutting point was selected because it stood at the midpoint of the range.

tion in community elections. In general, the candidates from the "large" and "competitive" cities had to allocate more resources to obtain their councilmanic positions. The second consideration, which will become more important later in this analysis, is that the effect of city size is noticeably greater than the effect of the closeness of the vote. When city size is controlled, the closeness of the vote is still significantly related to both dependent variables, but size is obviously the more "powerful" of the two variables.

For many candidates there may be other costs attached to running for office, costs such as anxiety, tension, etc.¹⁴ Time, money, and effort, however, are all resources that the prospective politician must allocate in order to obtain his office; and in the political choice process that confronts the prospective officeholder, the choice to spend these resources in office-seeking is also a choice not to spend the resources on other alternatives. For this reason they are an important component of the utility calculus of the potential office-holder when he must choose whether or not to seek political office.

¹⁴ Campaigning can be costly in a wide variety of ways. If a campaign is bitterly contested, the animosity, hostility, and rudeness of the campaign are all costs that the candidate and his family have to bear. It is difficult, however, to think of a way to aggregate costs such as these into a serious calculus of the "cost" of a campaign.

The Elements of a Theory of Career Choices
Electoral Costs as a Political Risk. Students of politics have generally examined the electoral process in communities primarily in terms of the competitive characteristics of that process; i.e., the relative probabilities that candidates or parties will emerge victorious. And, with the exception of Schlesinger's efforts, most research on the competitive process has ignored the election as a risk taking venture in which candidates are forced to wager a portion of their resources in the pursuit of office.¹⁵

The importance of "risk" derives from the condition that the magnitude of such risk is partly determined by the structural characteristics of the electoral system in which the candidate runs for office. For the city councilmen, risk enters into their choice calculus at two points, when they originally decide to seek the councilmanic position, and when they must decide whether or not to pursue any further political offices. In both instances they are confronted with the possibility of losing the election; and even if they are fairly certain of winning, the necessity to campaign is likely to force them to make investments in politics that could be made elsewhere. And, as our earlier data indicate, the costs of campaigning appear to be strongly affected both by the size of the city in which the election occurs and by the competitiveness of the local elections.

¹⁵ Schlesinger, *Ambition and Politics*, p. 10.

Table 2. Relationship of Campaigning to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

General Campaigning	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Considerable	72	37		56	28	
Moderate or Not						
Too Much	28	63		44	72	
Respondents	(110)	(259)	(369)	(233)	(124)	(357)
	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Considerable	78	—		42	28	
Moderate or Not						
Too Much	22	—		58	72	
Respondents	(85)	(8)*	(93)	(139)	(106)	(245)

Question: "In general, about how much campaigning is there in city elections here in (CITY)? Would you say it is considerable, moderate, or not too much?"

* Too few cases.

This information provides the basis for a general hypothesis relating risk to the two structural variables mentioned above.

Hypothesis I: The risk of running for office, (R), is an increasing function of the size of the political unit and of the degree of electoral competition in the political unit.¹⁶

Both parts of this hypothesis are essential for this analysis, but neither of them can be fully confirmed with the data available to us. The hypothesis implies that the costs of running for office are always greater than zero. This means that individuals would choose not to run for office unless the office was of some value to them, that is, if (PB) were greater than zero. These assumptions cannot be confirmed because I do not have data on the benefits that candidates receive from the act of running for an office; in other words, the benefits from campaigning that are independent of the outcome of the elections. The validity of the hypothesis will have to rest on whether or not the use of the hypothesis leads to predictions that can be confirmed with the data that are available.¹⁷

The magnitude of the risk for the politician depends on several factors. In the first place, the investment required for a campaign is likely to be substantial, and it constitutes only a portion of the investments required of the prospective candidate. In addition, however, the candidate may also have invested a substantial amount of time in another office or in other political activities, and prospective candidates may have to risk that investment in order to seek an office. And finally, any decision to seek an office involves an "opportunity cost"; the cost of foregoing other alternatives in order to seek a political office.

Although there are a variety of costs associated with holding office in these communities, the costs that result from elections constitute a

significant portion of the total investment required by the councilman. Most councilmen, for example, have not accumulated a large investment in a political career that must be "risked" to seek the councilmanic position, at least not anything like the investment "risked" by House of Representatives members when they run for the Senate.¹⁸ In addition, although the costs of being a councilman can be rather substantial, the councilmanic position is not a full-time job in any of the communities of this study. In most of the communities, the cost of holding office is minimal; but even if holding office involves some substantial investment, the magnitude of that investment is undoubtedly an increasing function of the size of the community, and the direction of that relationship only serves to reinforce the arguments in this analysis.

By assuming that the net investment of the councilmen is an increasing function of the size of his city and of the competitiveness of his city's elections, one can demonstrate that these investments dramatically affect the level of political commitment of those who seek public office. As the size of the investment required for an office increases (with city size and electoral competition), the less committed individuals should be more likely to drop out of the race for the office. Thus, we should find that the occupants of offices in the larger and more competitive cities are, in general, more committed to politics and to a political career than the occupants of offices in other communities. With these ideas in mind, we can outline the logic behind these effects.

$u(0) = PB - R$, where (R) represents the investment of the politician that will be lost if the politician loses his election. (2)

Proposition I: The greater the risk attached to a given alternative, (R), the greater that the *minimal value* of that alternative, (PB), must be for the individuals who pursue that alternative.

Proposition II: The lower the probability that an individual will succeed in achieving a given alternative, the higher must be the value of that alternative, (B), for some values of (P) and (B).

The first proposition restates a conclusion derived from the simple utility model outlined earlier; the value of an office, (B), must be greater than the risk of obtaining the office, (R); and as (R) increases in value, the minimal value of (PB) must increase correspondingly. The use of the term, *minimal value*, reflects the fact that (PB) can be and probably

¹⁶ The size of a city is also related to the degree of competition in city elections. Elections in the larger cities, on the whole, tend to be more closely contested than those in the smaller communities. For this reason city size must be controlled for when electoral competitiveness is employed as an independent variable. For empirical evidence of this relationship, see Gordon S. Black, *The Arena of Political Competition* (Bobbs-Merrill, forthcoming).

¹⁷ Some candidates undoubtedly do run for office, not because they expect to win but because it will benefit them in other ways. Local lawyers, for example, sometimes use office-seeking as a way of advertising, since advertising is normally forbidden under state law. The logic for such individuals is that they should attempt to minimize their total expenditures because they will benefit from all of the free publicity they derive from the election.

¹⁸ Few of the councilmen had to "risk" another office to run for the council, and the ones that did resigned from relatively minor offices.

often is very much greater than (R); and that while (R) establishes a lower boundary on (PB), it does not in any way determine what the upper boundary might be.

The second proposition also is a straight forward implication of our simple utility expressions that (P) and (B) are inversely related to each other in such a way that as either increases in value, the other will decrease in value (for some values of both variables). Because (PB) can be much greater than (R), however, (P) and (B) are not always fully interdependent; the degree of interdependence between (P) and (B) is a function of the ratio of (B) to (R).

If $(PB) > k$, where k represents some fixed value of (R), then

$$(B) = k/P \text{ as the } \lim_{(PB) \rightarrow k} (PB)/k = 1. \quad (3)$$

and

$$(B) \neq (k)/P \text{ as the } \lim_{(PB) \rightarrow \infty} (PB)/k = \infty$$

Expressions (2) and (3) indicate that as the ratio of $(PB)/R$ approaches one, (P) and (B) will increasingly become inversely related to each other; but as the ratio of $(PB)/(R)$ increases, the degree of interdependence between (P) and (B) will decline. In empirical terms, this means that in the situation where the value of the office to the candidate is very much greater than the risk attached to achieving the office, a decline in (P) will not produce a corresponding increase in (B). On the other hand, where (B) is nearly equal to (R), any decline in the value of either variable will be associated with an increase in the value of the other variable such that the condition, $(PB) > (R)$ is satisfied.

Hypothesis I suggested that the risk of running for office is an increasing function of both the size of the political unit and the degree of the competition in that unit's elections. By implication, then, the following must also be true as shown previously.

Proposition III: The greater the size of the political unit, the higher will be the minimal value of (PB).

Proposition IV: The higher the degree of electoral competition in a political unit (less whatever relationship exists between size and electoral competition), the higher will be the minimal value of (PB).

Propositions III and IV suggest that the net value of an office, (PB), is an increasing function of the degree of electoral competition in a unit after controlling for the relationship between size and electoral competition. These re-

lations do not predict all of the values of (PB), they only predict that (PB) cannot fall below some level determined by (R).

The Empirical Hypotheses. The remainder of the analysis will be concerned with the decisions confronted by these city councilmen at three levels: their original choice to seek office, their decision to attempt to retain their current office, and their decision to seek other political and governmental offices. These choices reflect two different levels of political commitment; the councilman's commitment to his current position which we will call *positional commitment*, and the councilman's commitment to seek other political and governmental positions, which we will call *progressive commitment*. The former is a reflection of the value the councilman has placed on achieving his present position and on staying in that office, while the latter reflects his generalized aspiration for other political or governmental offices.¹⁹

These two types of political commitment need not be highly correlated; that is to say, those who are progressively committed need not be committed to their current positions. A councilman may run for the council not because he was particularly concerned with local politics but because he wished to use the council as a springboard for other political offices. Likewise, a councilman who is highly committed to his position on the council may see that position as the limit of his political involvement.

One index of the councilman's commitment to his position is the strength of his original desire for the office. (Another, which is the subject of the next section, is the strength of his intention to seek reelection.) Because the respondents in this study are all incumbents, it follows axiomatically that the values they placed on their offices must have been greater than the risks they sustained to obtain those offices, i.e., $(PB) > (R)$, such that $u(O) > 0$. This is the case because $u(O)$ must be positive, that is to say, the councilmen must have expected to receive greater benefits from the office than the costs they sustained in seeking it. Knowing that (PB) must be positive, however, tells us little about the variation that exists in the values of (P) and (B). For such information we can refer to our earlier hypothesis that (R) is an increasing function of the size and electoral competitiveness of a community, a hypothesis that implies the following empirical hypotheses:

¹⁹ Schlesinger employs similar terms in his analysis, *Ambition and Politics*, pp. 10-11.

Hypothesis II: The councilman's desire for his office, (B), is positively related to the size of his city and to the degree of the competition in his city's election.

Hypothesis III: If the size of councilman's city is held constant, the councilman's desire for his office, (B), will still be related to the degree of the electoral competition in his city's elections, but the magnitude of that relationship will decline.

Hypothesis IV: If (R) is held constant, those councilmen for whom the outcome of the last election was uncertain will tend to express a greater desire for their office than those councilmen who felt they were certain to win. (See expression 3)

All four hypotheses follow directly from the cost/benefit model discussed earlier. Hypothesis II simply expresses the relationship between the value of an office and the risk attached to running, where risk is assumed to be an increasing function of both size and electoral competition. Hypothesis III indicates that electoral competition should have an effect on the value of the office for the councilmen that is independent of the effect of the size of the councilman's city. The last hypothesis expresses the inverse relationship that is expected between the preelection value of the office for the councilman and his estimated probability of winning in the election.

Before proceeding to a test of these hypotheses, we must make several points about the data employed here. In the first place, these empirical indicators are but crude reflections of the theoretical variables discussed in this research. For one thing, some of the indicators are based on recall data, and they are subject to all of the errors that are inherent in such data. In addition, the incumbency of all the respondents has the effect of skewing some indicators badly. (For example, the estimates of the likelihood that the councilmen might win their office originally are highly skewed toward certainty rather than uncertainty.) A final problem is that it is necessary to employ two surrogate variables as measures of electoral risk, size and the degree of electoral competition, because we lack complete (or additive) data on the costs of running for office.

These limitations in the data, limitations that are all too common in social science research, have caused me to follow two general rules in analyzing these data and testing the hypotheses. The first is that the method of analysis should be kept simple. (And simplicity is appropriate since we are not attempting to account for variance.) The second is that the theory in this research depends not on a single hypothesis but on a pattern of hypotheses, all of which must

be confirmed before we can have any confidence that our theory has merit. With these considerations in mind, we can examine hypotheses II and III (see Table 3).

Table 3 reflects the result of asking the councilmen to evaluate their desire to be elected during their last campaign, and it is used here as a measure of (B). In both analyses, the table shows that the hypotheses are confirmed; moreover, when city size is controlled, both city size and closeness of the vote in elections are cumulatively and positively related to the councilman's *desire to be elected* to his office.²⁰

Size, however, appears to be more strongly related than electoral competition to the councilman's desire to be elected. This finding was anticipated earlier when the variable of city size was observed to have a consistently stronger relationship to the two cost variables, money and effort.

The next hypothesis to be tested is the effect of the probability of winning on the councilman's original desire for the office he holds. Because the variable is highly skewed toward a high confidence of winning, the sample has been divided into two groups, one containing those councilmen who were nearly certain of winning and the other containing the remainder, who expressed varying degrees of uncertainty. In order to make this comparison, however, it is necessary to control as much as possible for the variable of electoral risk. This can be done by controlling for city size and for electoral competition.

As Table 4 indicates, in both the "small" and the "large" cities, the councilmen that were more certain of winning tend, on the whole, to place a lower value on the councilmanic position. The magnitude of this relationship appears to be greater in the large cities than in the "small" cities. We can now examine a similar table that uses electoral competition as the control variable.

The same pattern is apparent in Table 5 as in Table 4. Although the magnitude of the relationship is somewhat smaller than before, the councilmen who were more certain of winning tended to place a lower value on their office at the time of their last election than the councilmen who were less certain of winning. Although hypothesis IV is confirmed in both tables, this confirmation should be taken with caution because the number of cases in one col-

²⁰ I have employed the average closeness of vote because it gives a better measure of the general competitiveness of the community. Candidates probably form their expectations of what to expect in an election on the basis of what generally transpires in their city.

umn in each table is small. At the same time, the councilman's confidence of winning only measures a limited range of (P) because very few councilmen indicated that they thought they might lose. If the distribution of (P's) for the councilmen were less skewed, a greater effect from this variable could be expected.

Although the predictions from the model appear to be confirmed in each instance, the factor of risk probably does not *cause* candidates to place a high value on the office they seek to obtain. Inasmuch as these risk factors are structural characteristics of the system, they must be faced by all potential candidates when deciding whether to run for office. In a system characterized by a large initial risk in running for office, only the more committed candidates are likely to run for office, that is, those who place a high value on the office. The factors that cause individuals to place a high value on an office lie elsewhere; in the importance of the office, in the probability that the office will lead to other more important offices, and in the background of the individual. All the model predicts is that the higher the risk of running for once, the more committed will be those who run for the office. This prediction is important, however, because it suggests that the structural characteristics of political units determine, in part, the level of the commitment of the men who hold office in those units. This ap-

pears to be one of the ways that the electoral system operates as a screening device; i.e., it can, under certain conditions, screen out the less committed potential officeholders.

The Decision to Seek Reelection

Once the individual has achieved office, he is faced with a second choice in relation to that office; should he seek reelection? Although one might intuitively expect that those who place a higher value on the office are more likely to seek reelection, this would be the case only if the risk of running again were the same for all councilmen, which it is not.

As shown earlier, those who place a high value on their office tend to face a higher risk should they decide to seek reelection, while those that place a lower value on achieving office also tend to face a lower risk in running for reelection. The implication for the model is that each factor must be considered in relation to the other, that the decision to run again will be directly related to the difference between the value placed on the office and the risk attached to running for reelection.

What we should expect to find is that if (R) is held constant, $u(O)$ will be an increasing function of (PB). This means that when the two surrogate measures of risk are controlled, those councilmen who indicated they had "considerable" desire for their office will be more

Table 3. Relationship of Desire to be Elected to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

Desire to be Elected	Population			Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Considerable	57	26		42	19	
Some	22	12		15	14	
Little	21	62		43	67	
Respondents	(90)	(242)	(332)	(208)	(101)	(309)
Desire to be Elected	Large Population Average Closeness-of-Vote			Small Population Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Considerable	56	—		34	16	
Some	21	—		12	14	
Little	23	—		54	71*	
Respondents	(77)	(6)**	(83)	(131)	(95)	(226)

Question: "Which of the following statements most closely approximates your desire and efforts to be elected to the Council during your *last* campaign?"

* Rounding Error.

** Too few cases for interpretation.

Table 4. Relationship between Desire to be Elected and Confidence of Winning in Last Election (Controlling for Size of City)

Desire to be Elected	Large Population Confidence in Winning			Small Population Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Considerable	53	85		26	26	
Some	23	8		10	26	
Little	24	8*		64	49*	
Respondents	(68)	(13)	(81)	(190)	(39)	(229)

Question: "Which one of the following statements most accurately reflects your confidence in winning at the time of your *last* election? (I felt fairly certain of winning. I thought I could win, but I felt the election would be close. I thought the election could go either way. I expected to lose, but I thought if I worked, I could make it a close race. I felt fairly certain of losing.) The first two statements were grouped into the category of "certain" while all the rest were placed in the category of "uncertain."

* Rounding Error.

likely to seek reelection than those councilmen who expressed only "some" or "little" desire to be a councilman. Moreover, the relationship between the councilman's desire for his office and his intention to run again should be greater in the highest risk cities than in the lowest risk cities, because when (R) approaches zero, any positive value of (PB) will result in a decision to seek reelection.

Although the magnitudes of the differences in Table 6 are small, they tend to support the conclusion that when (R) is controlled (by controlling the size of the city and the degree of competition in elections), the original value placed on the councilmanic position by a councilman appears to be related to his intention to seek reelection. In the highest risk cities, the difference is 15 per cent; and in the intermediate risk cities, the difference is 11 per cent. The lowest risk cities do not follow this pattern, that is, the councilman's original desire for the office is unrelated to his intention to run again in the cities.

When these tables are viewed from another direction, however, the lowest risk cities do not constitute an exception to the prediction made above. An equivalent implication derivable from the model is that if (PB) is held constant, the risk of (R) running for office in a city, will be inversely related to the probability that a councilman will seek reelection. This proposition can be examined by comparing the councilmen who expressed "considerable" desire to be elected in both the "lowest" and "intermediate" risk cities and the councilmen who expressed only "some or little" desire to be elected in the same set of cities. The difference in the "some and little" categories is 18 per cent, whereas among the councilmen who indicated "considerable" desire to be elected the difference is only 16 per cent. This finding is consistent with the theoretical relationship between (PB) and (R). The higher the value placed on the office (PB), the more the risk of running for reelection (R) will have to increase before the risk of running for reelection is

Table 5. Relationship Between Desire to be Elected and Confidence of Winning in Last Election (Controlling for the Average Closeness of the Vote in City Elections)

Desire to be Elected	Closeness of the Vote					
	High			Low		
	Confidence in Winning			Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Considerable	41	47		18	25	
Some	14	21		12	25	
Little	45	32		70	50	
Respondents	(167)	(38)	(205)	(89)	(12)	(101)

greater than the value of holding the office, that is to say, for (R) to be greater than (PB) so that $u(O)$ is less than or equal to zero.

These data and the tables presented earlier suggest that the utility calculus outlined at the beginning of this analysis may prove a powerful tool in accounting for the career decisions that politicians must make. The real test of the model, however, is whether it permits us to predict the circumstances under which politicians are likely to develop that progressive commitment to politics, that we call political ambition.

A Theory of Political Ambition

The Search for Other Offices. Let us assume for a moment that a political system is composed of a set of positions that differ both in the benefits and the costs attached to those positions and in the investment required to achieve those positions; being a governor, for instance, tends to be more costly in terms of time, effort, and money than being a mayor of a city of moderate size. In order to induce men to seek more costly positions, those positions must be made relatively more rewarding than the less costly positions. This means that as the costs of an office increase, the benefits to be derived from that office must increase faster than costs, such that the net benefit from the

more costly position is greater than the net benefit from the less costly position. If this were not the case, rational men would not seek to advance to more costly positions.

The price that a politician is willing to pay for a position is the sum of the investments that he is willing to make in order to obtain the position. These investments are made in two general ways; by actually holding an office in a given career sequence and by competing in the periodic elections that politicians face in the course of a political career.

These investments constitute a measure of the political commitment of individuals. As the size of the investment required for an office increases, the less committed individuals are likely to drop out of the race for the office. My intention here is to show that with an extension of the calculus outlined earlier, one can predict the circumstances under which individuals are likely to aspire to higher levels of office.

One way to approach this question is to formalize the concept of a career sequence. Assume that a political system is composed of a set of positions, $(X_1, X_2, \dots, X_m)$, where X_i represents the i th position; and further assume that each of these positions has attached to it a set of costs, $((M_1, R_1), (M_2, R_2), \dots, (M_m, R_m))$, where M_i represents the cost of main-

Table 6. Relationship Between Decision to Seek Reelection and Original Desire for the Office
(Controlling for Size of City and Degree of Electoral Competition)

Large Population						
Intention to Run Again	Average Closeness-of-the-Vote					
	High			Low		
	Considerable %	Some & Little %	N	Considerable %	Some & Little %	N
Yes and Probably	73	58		—	—	
No	27	42		—	—	
Respondents	(33)	(26)	(59)	(6)*	(6)*	(12)
Small Population						
Intention to Run Again	Average Closeness-of-the-Vote					
	High			Low		
	Considerable %	Some & Little %	N	Considerable %	Some & Little %	N
Yes and Probably	67	56		73	74	
No	33	44		27	26	
Respondents	(39)	(72)	(111)	(15)	(72)	(87)

Question: "Do you expect to continue to run for the city council?"
* Too few cases to interpret

taining the i th position. Assume, in addition, that each potential candidate has a utility function such that $u(X_i) = (PB_i) - (R_i)$, where B_i is the value of the i th position and is composed of the sum of the benefits of holding office, (b_i) , minus the costs of holding office, (M_i) ; i.e., $B_i = (b_i - M_i)$.

The sum of the costs of holding office (M_i) contains within it a set of costs which are uniquely political. For example, a politician needs to learn how to operate in a political position, he needs to know to whom he must go in a constituency to obtain political support both for himself and for policies he wishes to pursue; and he needs to know which coalitions are possible and which are not. Finally, he must become familiar with the laws of his system, the rules of procedure in his political arena, and the informal norms that regulate the interpersonal relations among politicians.

Similarly, the risk of obtaining a position also contains a set of costs which are common to politics but which are less frequently found in other occupations. The potential candidate will have to become well enough known to his constituents so that they will support him at the polls. As before, he needs to know the structure of coalitions in his constituency, the groups from which he might obtain support, and those individuals who can provide him with financial support for his campaign. Finally, it would be useful to know how to run a campaign organization, how to handle himself in public, and what themes would be most useful to emphasize in seeking electoral support.

Most of these types of experience and knowledge can only be acquired in a single way, by investing in the political process in the area in which the candidate seeks to obtain a political position. At the same time, each investment that the individual makes in the political process in his political unit is an investment not only in the office he seeks or holds *but also potentially in the other offices or positions eventually open to the individual*. For example, the political novice who seeks a particular office will have to make a greater investment to obtain that office than the individual who has had long experience with the politics of the political unit. This phenomenon we will call an *investment transfer*, and it occupies a central place in this theory of ambition.

Investment Transferability. As a first approximation of the investment process for politicians, let us assume that politicians evaluate the alternatives that face them in terms of the relative *rates of return* they can expect from these alter-

natives,²¹ and further assume that their investment strategy will reflect their attempt to maximize their expected value from their net investments. Given the constraint that they can make a full investment of their resources, the attempt to maximize expected value is theoretically equivalent to the attempt to maximize one's average rate of return.

In the particular case at hand, all the individuals interviewed for this study were successful in obtaining a seat on the city councils in their respective cities. Since all of them could conceivably seek other offices, they each face three choices with regards to their future participation in politics: to decline to seek further office (at least in the short run), to seek re-election to the council, or to seek other political or governmental offices. They will choose that alternative with the highest expected rate of return, represented as follows:

Outside Alternative	Council Alternative	Other Political Alternative
$(P_o b_o)$	$(P_c b_c)$	$(P_i b_i)$
$L_o + R_o$	$L_c + R_o$	$L_i + R_i$

Only a portion of that which an individual invests in politics can be applied to a nonpolitical alternative; and as the individual continues to invest in a political office, the portion of his net investment that can be transferred to a nonpolitical alternative, relative to his total investment, probably declines. The principal effect of his investment in one office, to the extent that it is transferable to other positions, will be to reduce the size of the additional investment he must make in order to obtain those positions.

By reducing the magnitude of the investment necessary for other offices $(L_i + R_i)$, the individual is, in effect, increasing the rate-of-return he can expect from those other offices. This increases the attractiveness of other political offices relative to nonpolitical alternatives, and the magnitude of that increase should be a function of the cumulative investment the individual has made in politics.

This process by which the rates-of-return of various political and nonpolitical alternatives

²¹ The concept of rate of return as employed here includes the individual's estimate of the probabilities of succeeding at the various alternatives. We will not attempt to explore, however, the complex problem of the portion of their resources that councilmen will invest in politics because we lack data on the councilman's nonpolitical alternatives. This has no effect on the logic of the argument discussed next.

change as a result of an individual's political investments is part of the political choice process for politicians. In any career sequence, if this analysis is correct, as the individual increases his investment in the career sequence, the value of the next step upward or the next goal will also increase relative to alternatives outside the career sequence. In a sense, the individual's investments tend to pull him further and further into the sequence even though he may not have originally intended to follow the route on which he now finds himself.

This does not mean that his rate of investment must increase except insofar as the next position he might seek requires a higher level of investment, but his *cumulative investment* will increase because he continues to regularly invest in politics. It is this cumulative investment that makes nonprofessional alternatives outside the career sequence relatively less attractive. This proposition can be formalized as follows:

Proposition V: The greater the cumulative investment of the individual in political office-seeking, the greater will be the value placed on the offices to which the individual might aspire.

Of the variables mentioned earlier, size and competition both would seem to reflect part of the cumulative investment of these councilmen. Since the office of city councilman is nearly at the bottom of the office structure in this system, it seems likely that these individuals' electoral investments constitute a significant portion of their total investment in politics.

The essence of the question, however, is whether these two variables represent investments that are in fact transferable to other offices. For obvious reasons, city size probably reflects a set of investments that are transferable, both those investments made during elections and the investments made while holding office.²² The costs associated with electoral competition may, on the other hand, not be transferable to other offices. In fact, the closely

won campaign may prove detrimental to an individual's chances of seeking other offices, partly because of the antagonism generated during such campaigns and partly because a close election indicates that a sizeable portion of the electorate favors other candidates.

The variable of size operates in the expected manner; about 22 per cent more of the respondents in the "large" cities indicated they desired other offices than those in the "small" cities. At the same time, the closeness of the vote in elections is only slightly related to the councilman's ambitions when the variable of city size is employed as a control variable.

The councilman's confidence in winning also may affect his desire to seek other political or governmental positions, but the logic behind this relationship is somewhat different from that outlined above. One of Schlesinger's central points is that the probabilities of obtaining various offices are interdependent, so that success at obtaining one office alters, positively, the probability of obtaining other offices in a career sequence, i.e., $P_i = f(P_c)$.²³ If such probability estimates are interdependent, then the councilmen who were confident of winning their council seat might also be more likely to seek other offices. The reason for this is that any factor which increases the value of P_i also increases the rate of return that can be expected from the i th office.

Table 8 shows a rather strong and expected relationship between the councilman's confidence in winning his council seat and his aspiration to seek other offices. Councilmen who were *certain* of winning their councilmanic seats are also more likely to aspire to higher office than those who were uncertain of victory. Although this relationship is not a strict implication of our original model, it does fit the model if we accept the additional assumption that subjective estimates of achieving office by politicians are interdependent in the manner discussed earlier.

The final two variables in this analysis, the councilman's original desire for the councilmanic position and his intention to seek reelection, should both be related to the councilman's desire to seek other offices. The desire to

²² By introducing the concept of "transferability," we are opening up a relatively uncharted set of questions for analysis. Investments in public office, unlike those in stocks, are not readily translated into a common denominator such as money. The investments in politics involve rather amorphous quantities such as experience, knowledge, prestige, etc., and we know next to nothing about the general process through which such factors are aggregated by the individual. At the same time, however, the existence of an investment process (with some element of transferability) seems quite plausible and perhaps intuitively obvious. The data here seem to substantiate the existence of these phenomena, but we have only made a small and preliminary step toward delineating the full dimensions of the process.

²³ Throughout this paper we have conducted our analysis entirely with *subjective* probabilities, and we continue to do so here. Utility theory rests not on the real likelihood of a given outcome, but on the individual's estimate of what those probabilities are. An intriguing question, however, is the extent to which subjective estimates correspond to the objective events faced by candidates. I suspect, although this is simply conjecture, that candidates tend systematically to overestimate their electoral chances.

Table 7. Relationship of Political Ambition to City Size, to Electoral Competitiveness, and to Electoral Competitiveness Controlling for City Size

Desires Other Political Office	Population			Average Closeness-of-Vote		
	Large %	Small %	N	High %	Low %	N
Yes and Perhaps	55	33		41	34	
No	45	67		59	66	
Respondents	(102)	(294)	(396)	(247)	(149)	(396)

Desire Other Political Office	Large Population			Small Population		
	Average Closeness-of-Vote			Average Closeness-of-Vote		
	High %	Low %	N	High %	Low %	N
Yes and Perhaps	52	—		35	31	
No	47	—		65	69	
Respondents	(87)	(15)*	(102)	(160)	(134)	(294)

Question: "Are there any other political or governmental positions—local, state, or federal—which you would like to seek?"

* Too few cases.

seek reelection is also an expression of a willingness to continue to invest in politics, and this is consistent with the discussion earlier which suggested that a portion of such investment could be applied to other offices and would increase the attractiveness of such alternatives by increasing the rates-of-return from those alternatives.

We expect a relationship between the councilman's original desire for his office and his desire to seek other offices, because a great many councilmen value their office more than the minimal amount required by the propositions outlined earlier. I suggested that the campaign created a kind of investment barrier, but that many prospective councilmen could value their office considerably more than they actually have to pay for it. That excess evaluation probably represents a kind of generalized commitment to politics, and it might be expected to be

related to a higher level of value placed on offices up the career chain. Table 9 shows these two relationships. Both variables can be seen to be positively related to the councilman's desire to seek other offices.

What remains is to show that the three principal variables of this part of the analysis have a cumulative effect on the level of ambition demonstrated by the councilmen. Our argument has been from the beginning that such a cumulative effect should occur, that we should be able to demonstrate that each of the variables has an independent effect that is not canceled out by controlling for the other two variables. In Table 10 all three variables—the original risk of running for the council, the original value placed on the councilmanic position, and the councilman's intention to seek reelection—are correlated with his desire to seek other offices.

Table 8. Relationship of Political Ambition to Confidence in Winning in the Last Election (Controlling for City Size)

Desires Other Political Office	Large Population			Small Population		
	Confidence in Winning			Confidence in Winning		
	Certain %	Uncertain %	N	Certain %	Uncertain %	N
Yes and Perhaps	59	46		35	26	
No	41	54		65	74	
Respondents	(76)	(13)	(89)	(202)	(39)	(241)

Table 10 shows dramatically the existence of a cumulative effect when the three variables are employed together. The net shift across the whole table is 53 per cent, and the step increases across the table are fairly regular. The effect of each variable can be determined by calculating the average of the percentage increases between the low and high values of each variable, e.g., by comparing the small cities with the large cities, etc. The average increase attributable to size was 28 per cent; the increase due to the councilman's original desire for the office was 12 per cent; and the increase owing to his intention to run again was 13 per cent. Thus we find that all three variables have an independent effect on the councilman's ambition for other political and governmental offices.

Conclusions

The primary purpose of this paper was to show that the structure of the political system in which men are recruited for office can have a profound influence on the type of men who emerge as candidates. In order to accomplish this purpose, I have elaborated a model of political ambition based on the principles of utility theory; this model was employed to develop a set of empirical hypotheses that could be tested at least partially with the data available to us. The analysis of those hypotheses suggest several important conclusions:

(1) The structure of political systems may play a significant role in shaping the ambitions of the men who occupy offices within the system. Although we only dealt with two such variables—the size of a community and the degree of competition in community elections—our findings suggest that the structural characteristics of systems can shape both the risks that politicians face and the investments that are required to reach political office. In a sense, our findings complement those of Schlesinger, who suggested that the structure of political opportunities has a similar effect.

Although these structural variables were

found to be associated with the values assigned to offices and the ambitions of our politicians, we have avoided suggesting that these variables *cause* changes in the evaluations of politicians. A more accurate and methodologically useful interpretation is that structural variations create barriers that the aspiring politician must overcome. In this way the structure of a political system acts as a filter that allows some types of individuals to move up through the system, while others are either stopped in their progress or diverted in less risky and costly directions. Thus, the system does not *cause* either ambition or success in a direct sense; what it does is to determine indirectly the kind of men whom we will find in various types of offices.

(2) A second conclusion, more tentative than the first, concerns our view of the social choice process for the individual, using utility theory as an analytic tool. One implication is that the study of political ambition generally can benefit from an approach that assumes that politicians attempt to behave in a rational manner in seeking their political aspirations. All of our tables were consistent with an interpretation that the career choice process for politicians is based on some simple elements of utility theory outlined earlier. This finding suggests that a rationalistic explanation of the career choice process may prove to be the road to an adequate theory of political ambition.

A second implication is that it may be desirable to seek the logic behind political choice in the immediate environment of the political actor rather than search his distant past for the motives and experiences of childhood. Much of the work on political recruitment has been focused on the social background or family experiences of the politician, and while these may have some impact on the career choices that politicians make, these findings suggest that more immediate forces play a powerful role as well.

(3) Our findings also suggest that political ambition develops in part as a result of the investments that politicians make in their politi-

Table 9. Relationship of Political Ambition to Intention to Run Again and to Original Desire for the Council Position

Desires Other Political Office	Desire to Be Elected			Intention to Run Again		
	Considerable %	Some & Little %	N	Prob. %	No %	N
Probably	50	35		48	38	
No	45	65		52	67	
Respondents	(101)	(186)	(287)	(191)	(96)	(287)

Table 10. Relation of Political Ambition to Size of City, to Original Desire for the Office, and to Intention to Run Again for the Council

Desires Other Political Office	Large Population					
	Considerable desire for the Council Intention to Run Again			Some or little desire for the Council Intention to Run Again		
	Prob. %	No %	N	Prob. %	No %	N
Probably	73	62		63	53	
No	27	38		37	47	
Respondents	(30)	(13)	(43)	(19)	(15)	(34)
Desires Other Political Office	Small Population					
	Considerable desire for the Council Intention to Run Again			Some or little desire for the Council Intention to Run Again		
	Prob. %	No %	N	Prob. %	No %	N
Probably	49	35		36	20	
No	53	65		64	80	
Respondents	(41)	(17)	(58)	(101)	(51)	(152)

cal activity. As a politician invests in one office, even if he has little desire at the time to seek higher offices, he is altering his evaluation of other offices in a potential career sequence. This alteration occurs because some part of his investment in politics is transferable to the pursuit of other offices, and the ability to transfer investments increases his expected return from higher offices. Thus, each step in a political career sequence alters one's evaluation of the other step: in that sequence, especially in regard to nonpolitical alternatives. As the politician's investment increases, his evaluation of political alternatives is likely to become more pos-

itive while his evaluation of nonpolitical alternatives is likely to remain about the same. The net effect is the development of higher levels of ambition in the politician. This description of the development of ambition differs markedly from the view that sees the politician as a driven man who decides his course early and plans his whole life accordingly. Perhaps there are such men, but we suspect they are a distinct minority. The tides of politics are too great to permit men to chart an undeviating route through the uncertain and troubled waters of political life.

The Committee Function: An Influence Equation*

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In legislative systems a bill is commonly considered and reported by an appropriate committee before it is considered (with a simple majority required for passage) on the floor of the house. Since motions on the floor (amendments et al.) frequently relate to bills, when speaking of a motion it is often apposite to refer to *the relevant committee*. With respect to a given motion and its relevant committee, the extent of committee agreement *on the floor* is obviously consequential. There exists a substantial volume of folklore on this point, particularly related to the special case of committee unanimity,¹ but for precision and generality, a mathematical relationship between the relevant committee's divisions on the floor and a motion's probability of passing (or not passing) is preferable. The purpose of this note is to present such a mathematical model.

A first approximation, a linear function in one variable is easily extracted from the data in Table 1: If P = probability of passing = $f(x)$ where x = proportion of relevant committee voting yea, then $f(x) = 0$ when $0 \leq x \leq 0.2$ and $f(x) = 1$ when $0.8 \leq x \leq 1$ are obvious albeit incomplete conjectures; a straight line extrapolation for the remainder of the curve yields $f(x) = 5x/3 - 1/3$ when $0.2 \leq x \leq 0.8$. This function yields the theoretical values given in the fourth column of Table 1.

The function $f(x)$ is a respectable approximation, as a comparison of the third and fourth columns of Table 1 nicely shows. However, although continuous, that function is not a smooth function—specifically, the function $f(x)$ lacks tangents at the points $f(0.2) = 0$ and $f(0.8) = 1$. Mathematically speaking, a function with de-

derivatives at all points and numerical values similar to those of the function $f(x)$ is to be preferred. Fortunately, $P = g(x) = x^2/[x^2 + (1-x)^2]$ is continuously differentiable and yields the values shown in the fifth column of Table 1.

The function $g(x)$, unlike the function $f(x)$, has the intuitively desirable property of being parsimonious with the limiting probabilities of one (1) and zero (0); specifically, it yields 0 if and only if $x = 0$ and 1 if and only if $x = 1$. Both functions are symmetrical with respect to the yeas and non-yeas; neither $g(x)$ nor $f(x)$ discriminates between the nay-saying and non-voting members of the relevant committee. However, presumably, the probability of a motion's passing is adversely affected if (say) one-half of the relevant committee members vote nay rather than abstain. Thus a smooth function in two variables is desired.

If $P = h(x,y)$ where y = proportion of relevant committee not voting, then the proportion of the relevant committee voting nay is a derived quantity $z = 1 - x - y$. Since committee members (as an empirical matter of fact) tend to abstain less frequently than other members of a legislative body, the numerical value of y is often quite small. This suggests that the smooth function $h(x,y)$ should approximate $g(x)$ when y approximates zero (0).²

The committee function $h(x,y) = (x^2 + y^2)/(x^2 + y^2 + z^2) = P$ was tested using roll call data from the U.S. House of Representatives, 91st Congress, 1st Session. The relevant committee and its membership were ascertained for 145 roll calls.³ The probability of passing was

* I thank Harvey Arnold, Franklin Burkeen, Harold Casstevens II, Glenn Friedly, Richard Hahn, William Ice, John Lecznar, Roger Marz, Jan Ozanich, and Harvey Smith.

¹ "When they [the Appropriations Committee Members] stick together, you can't lick 'em on the floor." (A remark attributed to an anonymous House leader and quoted by Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* [Boston: Little, Brown and Company, 1966], p. 37.) "Now is it to lower the price of corn, or isn't it? It is not much matter which we [the Cabinet] say, but mind, we must all say the same." (A remark attributed to Lord Melbourne and quoted by Walter Bagehot, *The English Constitution* [London: Oxford University Press, 1961], p. 13n.) This essay adopts Bagehot's view that the British Cabinet is not only a committee but also *the* committee of Parliament and especially of the House of Commons.

² There is nothing absolute about these desiderata, but they did guide the search for a function to be empirically tested. Our $h(x,y) = g(x)$ when $y = 0$.

³ *Congressional Roll Call* (Washington: Congressional Quarterly Service, 1970) was used to determine each member's vote on all 177 roll calls and to ascertain the relevant bill for each motion; given the relevant bill, *Digest of Public General Bills* (Washington: Government Printing Office, 1970), 2 volumes, yielded the relevant committee for each of 145 roll calls; given the relevant committee, *Congressional Directory: 91st Congress, 1st Session* (Washington: Government Printing Office, 1969) gave its membership. Motions requiring exceptional majorities were excluded. Changes in committee memberships during the Session were ignored, except when a member vacated his seat in the House (which was noted in *Congressional Roll Call*) and then his committee was considered to have been reduced in size.

Table 1. Preliminary Perspectives on the Committee Function

Proportion of Committee Voting Yea*	Number of Motions*	Proportion of Motions Passed*	Probability of Passing f(x)	Probability of Passing g(x)
0.0-0.1	16	0.0	0.0	0.0 -0.012
0.1-0.2	17	0.0	0.0	0.012-0.059
0.2-0.3	19	0.16	0.0 -0.167	0.059-0.155
0.3-0.4	24	0.21	0.167-0.333	0.155-0.308
0.4-0.5	28	0.43	0.333-0.500	0.308-0.500
0.5-0.6	23	0.56	0.500-0.667	0.500-0.692
0.6-0.7	26	0.89	0.667-0.833	0.692-0.845
0.7-0.8	11	0.91	0.833-1.0	0.845-0.941
0.8-0.9	8	1.0	1.0	0.941-0.988
0.9-1.0	43	1.0	1.0	0.988-1.0

* Donald R. Matthews, *U. S. Senators and Their World* (New York: Vintage Books, n.d.), Figure 22. These data are drawn from the 83rd and 84th Congresses; the lower bound, but not the upper bound (except for 1.0), is included in each proportional category.

computed, using $h(x,y)$, for each of those 145 motions.

A committee-by-committee comparison of the actually observed and theoretically expected results is given in Table 2. Altogether, $120/145 = 0.828$ of the motions passed and the expected value was $115.575/145 = 0.797$. A comparison of the observed and expected values, classified by intervals of $1/10$ in the probability values, is given in Table 3.

The tabular data are quite compatible with the committee function $h(x,y)$. However, given the small number of cases in some cells of the partitions, the goodness of fit may be underestimated in a cursory examination. Fortunately, as arguments such as the following show, the manifest goodness of fit should be taken seriously. (1) Consider the House Committee on Foreign Affairs. The difference between its actual and expected results is the largest such difference in Table 2. The mean probability of passage for the 8 motions is the expected value divided by the total number of motions, to wit, 0.709. Thus, approximately, the probability of 7 motions (the observed number) passing is

$$\binom{8}{7} (0.709)^7 (0.291) = 0.210.$$

Similarly, the approximate probabilities for the passage of 8 and 6 motions are 0.064 and 0.302, respectively. Therefore, if the committee function generated correct probabilities, there is a probability of approximately $0.210 + 0.064 + 0.302 = 0.576$ of obtaining the observed result ± 1 . (2) Consider the probabilities such that $0.2 \leq P < 0.3$. The difference between their actual and expected results is the largest such

difference in Table 3. The mean probability of passage for the 7 motions is 0.236. Thus, approximately, the probability of 3 motions (the observed number) passing is 0.155, the probability of 4 motions passing is 0.047, and the probability of 2 motions passing is 0.306. If the basic probabilities are correct, then the prob-

Table 2. Observed and Expected Numbers of Motions to Pass, by Committee

Name of Committee	Total Number of Motions	Actual Number to Pass	Expected Number to Pass
Appropriations	32	27	25.774
Rules	15	15	14.537
Banking & Currency	15	10	10.353
Interstate & Foreign Commerce	11	9	9.374
Education & Labor	10	9	8.393
Ways and Means	9	8	7.692
Foreign Affairs	8	7	5.674
Armed Services	8	6	5.889
Public Works	7	6	5.943
Judiciary	6	4	2.794
Post Office & Civil Service	5	3	2.384
District of Columbia	4	3	2.840
Science & Astronautics	3	3	3.000
House Administration	3	2	2.113
Agriculture	2	1	1.902
Interior & Insular Affairs	2	2	1.900
Merchant Marine & Fisheries	2	2	2.000
Government Operations	1	1	0.966
Joint Atomic Energy	1	1	1.000
Veterans Affairs	1	1	1.000
Total	145	120	115.528

ability is about 0.508 of obtaining the observed value ± 1 .

Messrs. Burkeen, Friedly, Hahn, Lecznar, and Ozanich have also tested the committee function by analyzing 223 roll calls (all those for which they could determine relevant committees) in the U.S. Senate, 91st Congress, 1st Session. There were 63 consecutive roll calls related to the tax reform bill; this was an obvious fluke so those 63 were analyzed separately: 31 actually passed and 22.977 were expected to pass. The remaining 160 cases yielded 107 motions which actually passed and 100.386 which were expected to pass. Disturbed by this persistent albeit slight underestimation, Messrs. Lecznar and Ozanich proceeded to analyze 243 roll calls (all those for which they could determine relevant committees) in the U.S. Senate, 90th Congress, 1st Session. They found that 126 motions actually passed and that 135.890 were expected to pass. In sum, 611 roll calls have been analyzed: $384/611 = 0.628$ of the motions actually passed and $374.828/611 = 0.613$ of the motions were expected to pass; if the 63 manifest flukes are omitted, $353/548 = 0.644$ of the motions actually passed and $351.851/548 = 0.642$ of the motions were expected to pass.

The theoretical and practical⁴ utility of the

⁴For example, if a lobbyist can accurately forecast the committee members' reactions to his desired proposal (and this is a manageable job for a lobbyist), then the lobbyist can derivatively forecast its prospects on the floor (if it is introduced as an amendment).

Table 3. Observed and Expected Numbers of Motions to Pass, by Intervals of 1/10

Probability of Passing	Total Number of Motions	Actual Number to Pass	Expected Number to Pass
0.0-0.1	9	1	0.436
0.1-0.2	5	0	0.795
0.2-0.3	7	3	1.654
0.3-0.4	5	1	1.859
0.4-0.5	4	2	1.741
0.5-0.6	2	1	1.069
0.6-0.7	3	3	1.982
0.7-0.8	5	5	3.756
0.8-0.9	10	10	8.682
0.9-1.0	95	94	93.602

committee function $h(x,y)$ is transparent. For a given session of a given legislature, a more exact equation can no doubt be constructed. Indeed, any desired standard of accuracy can be surpassed if a polynomial of any degree is permissible. The simplicity of $h(x,y)$, by contrast, indicates its potential generality as an acceptable empirical law.

When a member of the relevant committee does not vote, the effect is purely to increase the probability of the motion passing. However, in general, the committee function does *not* equate not voting with voting yea.⁵ Nevertheless, if $x \leq y$, the committee function does imply that a committee member's switch from nay-saying to nonvoting would increase P at least as much as would his switch from nay-saying to yea-saying.

The effect of a committee member's not voting might intuitively be expected to be symmetrically distributed between increasing the probabilities of a motion's passing and not passing. Specifically, it was initially conjectured that $P = (x^2 + y^2/2)/(x^2 + y^2 + z^2)$. However, if yea-saying is not weighted more than nay-saying,⁶ then that intuitive expectation is simply confounded by the preceding data because the computed data err (if at all) on the low side.

In fact, the asymmetrical effect of not voting (by a member of the relevant committee) is actually quite plausible. Legislative folkways prize reciprocity between members; and on motions, this reciprocity seems to be translated as: "Unless the relevant committee says it's a mistake to do so, I'll support any other member's motion."

In summary, the purpose of this note has been to make explicit the previously implicit importance of the committee function as a concept and a problem in the mathematical theory of legislative behavior. The solution to the problem and implicit definition of the concept is $P = (x^2 + y^2)/(x^2 + y^2 + z^2)$, where P is the probability of a motion passing on the floor, x is the proportion of the relevant committee's members who vote yea, z is the proportion who vote nay, and y is the remaining proportion.

⁵The pertinent arithmetical fact is that $(x + y)^2/[x(x + y)^2 + z^2] \geq (x^2 + y^2)/(x^2 + y^2 + z^2)$, when x, y , and z are non-negative real numbers.

⁶Equal weights for yeas and nays are even more intuitively appealing than a symmetrical impact for nonvoting.

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To Nurture a Discipline

ROBERT E. LANE

To Nurture a Discipline

ROBERT E. LANE

Yale University

It is time to take more seriously the nurturance, that is, the support and planned development, of our discipline. The failure to do this implies more current unemployment than need be the case, poorer education than we are capable of providing, a training program incongruent with social needs, an ineffective address to public policy, lost information, and wasted talent. Since men cannot act without concepts to guide their actions, I wish first to set forth some paradigmatic models for our discipline and association. Then, following a brief sketch of the discipline, comparing it to some others, I plan to turn to two specific problems: graduate education and research creativity and productivity.

I. Association Paradigms

I think of university-based professional associations as having three stages of growth and development. First there was the *learned society*, the model that informed the consciences of the founding fathers in 1903 when they defined the purpose of the American Political Science Association as follows: "It shall be the object of this Association to encourage the study of political science. . ." The constitutions of the other social science associations were similar, except that the psychologists, geographers, and statisticians also mention a public service purpose. The principle activities of such learned societies were to publish one or more journals and hold meetings where scholarly papers were delivered and discussed. The National Office was not established until fifty years after the founding of the original learned political science society.¹

The second model, reflected in the term we use now, is that of the *professional association*. The constitution of the anthropologists, adopted one year before the political scientists', gives the main theme of this model: "to further the professional interest of the American anthropologists." In many ways this reflects the organizing principle of the associations formed by the doctors, lawyers, and engineers, even prior to the formation of discipline-based asso-

ciations.² Guided by the concepts implicit in this model, the social science associations established personnel services, committees on professional standards and ethics; they promoted their interests as these were affected by government and they sought to improve their working conditions, including in the early 'fifties and late 'sixties, the most important working condition, academic freedom. Finally, and belatedly, they sought to improve the conditions of such disadvantaged groups as Blacks, Chicanos, American Indians, and women. These activities, as reported to me by the executive secretaries of the eight main social science associations, were in addition to the learned society activities; it was an enlargement of duties and not a substitution of one set for the other.³

Underlying the operating principles of these two concepts were some fundamental beliefs about what might be called the "knowledge process," the process by which the sciences are developed, research conducted, ideas transmitted, men inducted into a training process and then placed in suitable occupations. The most important of these beliefs was the belief in *laissez-faire*, the satisfactory working of the market for trained specialists, for research information, and for information exchange on educational programs. This belief held that without central information services individuals were able to make intelligent decisions on graduate study and career choices, departments and universities were sufficiently informed to decide on what kinds of programs to offer and what degrees to award, research scholars, perhaps with the aid of a librarian, were able to locate the information they needed for their research, editors and regional or specialized associations were able to decide on what journals were needed and how circulation and business problems were to be met. I am arguing that under present conditions of size, specialization, and complexity the market forces, informal net-

² See Corinne L. Gilb, *Hidden Hierarchies: The Professions and Government* (New York: Harper & Row, 1966).

³ "Social Science and Public Policy: The Role of the Professional Associations," address to the Annual Meeting of the National Research Council, March 22, 1971. A questionnaire to the executive secretaries of eight professional associations forms the basis for some of these remarks.

¹ See Albert Somit and Joseph Tannenhaus, *The Development of Political Science: From Burgess to Behaviorism* (Boston: Allyn Bacon, 1967).

works, and unaided individual decisions now are wasteful and inefficient.⁴ It is time for a new paradigm.

I will refer to this as *science management* because this is an established term in other areas,⁵ but I hope you will understand that by "science" I mean all knowledge, humanistic as well as the more narrowly scientific, and by "management" I mean chiefly the provision of information and facilities by a central service to help guide individual freely made decisions. (I might have called it "knowledge nurturance" but for the awkwardness of the term.) This paradigm embraces several processes designed to help a discipline carry on its business more effectively. First, of course, there is the function of gathering information about the discipline, disseminating it, and analyzing it. That analysis is no longer a simple matter of generalizing from experience, or common sense inferences from raw data. The world has proceeded beyond that point, for along with the substantive development of the disciplines there have developed some meta-disciplines dealing with the organization, dissemination, and utilization of both knowledge and talent.⁶ Using the theo-

retical and empirical knowledge thus developed, we can begin to understand the recruitment and flow of students into the discipline, the more efficient ways of educating and training these students for the purposes we have in mind, the uses of our human resources, and the development of better information exchange systems in the discipline. These processes of analysis represent a form of disciplinary self-consciousness, one that we are just now beginning to employ (a little later than some disciplines—and earlier than others).

The second function involved in science management is a cost-accounting function. The economists have been neglectful of the social costs of the decisions of individual firms, calling them "externalities" and dismissing them. We have done the same thing; the benefits to any individual department of entering upon a Ph.D. training program may be substantial, but the effects on the discipline of these many individual decisions may be harmful. In the same way, it may serve the purposes of a regional association to publish a general journal, but the costs of information loss to the discipline may be very great. Some kind of social, discipline-wide cost-benefit analysis is implied in science management.

Third, there is the ingredient of forward planning, planning for new careers, planning for new archival facilities, planning for information exchange. Just as random unplanned use of land can result in ugliness and waste, so the random unplanned development and utilization of talent can result in personal tragedy and wasted resources. Plans need not and should not be coercive; but if they are well done, they are likely to be persuasive over the long run.

II. Portrait of a Discipline

The characteristics of a discipline help to shape both the agenda and the possibilities of

⁴ Political science shares the general characteristics of the growth of science, but in recent years has exceeded even these normal growth rates. See Derek D. Price, *Science Since Babylon* (New Haven: Yale University Press, 1961); for an intelligent critique of this, see Daniel Bell, "The Measurement of Knowledge and Technology," in Wilbert Moore and Eleanor B. Sheldon, eds., *Indicators of Social Change: Concepts & Measurements* (New York: Russell Sage, 1968) pp. 145-245, since Price reports a doubling of scientists and publications about every fifteen years, it is evident that in the past 25 years, at least, political science has exceeded the "normal" rate. For a different view of "the growth of science" see my "The Decline of Politics and Ideology in a Knowledgeable Society," *American Sociological Review*, 31 (1966), 649-62.

⁵ Robert Gilpin and Christopher Wright, eds., *Scientists and National Policy Making* (New York: Columbia University Press, 1960); Don K. Price, *Government and Science* (New York: New York University Press, 1954); Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage, 1969); *Reviews of National Science Policy: United States* (Paris: Organization for Economic Co-operation and Development, 1968).

⁶ There is an ample literature in these two fields of human resources policy and information science. In the first of these the *Journal of Human Resources* is especially valuable. There are two excellent recent studies on education and manpower: John K. Folger, Helen S. Astin, and Alan E. Bayer, *Human Resources and Higher Education: Staff Report of the Commission on Human Resources and Advanced Education* (New York: Russell Sage, 1970), and Dael Wolfe, *The Uses of Talent* (Princeton: Princeton University Press, 1971). The information field has its own professional organization, American Society for Informa-

tion Science. For political scientists one place to start is with W. J. Paisley, *The Flow of (Behavioral) Science Information: A Review of the Research Literature* (Stanford, Calif.: Institute for Communication Research, 1965); an excellent model for a disciplinary study is provided by W. D. Garvey and B. C. Griffith, *Reports of the American Psychological Association's Project on Scientific Information Exchange in Psychology* (Washington, D.C.: American Psychological Association, 1963-65); see also the report of the Committee on Information in the Behavioral Sciences (David Easton, Chairman), Division of the Behavioral Sciences, NRC: *Communication and Resources in the Behavioral Sciences* (Washington, D.C.: National Academy of Sciences, 1967), and Kenneth Janda, *Information Retrieval: Applications to Political Science* (Indianapolis: Bobbs-Merrill, 1968).

success for a program of disciplinary nurturance of science management. It is surprising how little we know about ourselves. Here are some rapid glimpses into the character of our discipline.⁷

Size. In terms of numbers of professionals, political science is among the smallest of the sciences in the *National Register of Scientific and Technical Personnel* (about a fifth of the size of physics and a quarter of the size of mathematics), but it is a medium sized social science. That is, it is about one quarter the size of psychology, half the size of economics, and about the same size as sociology.⁸ It is four times the size of anthropology. Its growth rate (in terms of earned degrees) has been a little slower than psychology and sociology, faster than economics.

Degrees and level of training. In 1970 about 61 per cent of the political science registrants in the National Register had doctorates (an increase of two per cent over two years.) This is a smaller percentage than anthropology and psychology, but a little larger than economics and sociology, and much larger than the natural sciences.

Salaries. In 1970 political scientists with the doctorate received a median salary less than

that of any other of 15 disciplines in the *National Scientific Register* except linguistics. This is due to the lower pay they receive in educational institutions; in the Federal government they receive among the two highest salaries (but only a few of the political scientists registered in 1970 worked in the Federal government.)

Work settings. Of the fifteen scientific fields in the *National Register*, a higher proportion of political scientists worked in educational institutions than any other discipline, except for anthropology: three quarters of all political scientists (and 85 per cent of the doctorates) may be found in academia. Fewer political scientists worked in industry and business than any other disciplinary group, again except for anthropology. About 5 per cent worked in the Federal government in 1970 and this was about average for the social sciences. Further, and tragically, this proportion has declined over the past two years.

Type of work. Political scientists think of themselves more as teachers than research scholars. In the *National Register*, a higher proportion of political scientists recorded themselves as primarily teachers than is the case for the natural scientists, and more than the psychologists or economists, but about the same as the sociologists and anthropologists. At the same time, a relatively high proportion of political scientists are in administrative work (whatever the setting)—more than any of the natural sciences and also more than any of the social sciences except economics. For example, compared to all other natural or social sciences, political science has the second highest proportion of faculty serving as deans—and political science deans are the best paid.

Financial support. Of the fifteen disciplines in the NSF roster political science ties for last place in terms of the proportion of members of the discipline receiving federal research or fellowship support. Put another way, in 1969 the total support from the Federal government for the various social sciences including psychology was \$129 million; support for political science was by far the smallest amount awarded to any discipline, a little less than \$3 million.

Without regard to Federal support, fewer political science graduate students received fellowship, teaching assistantship, or other support than was true of any other social science (1966). The average stipend for political science graduate students was lower than that of any other social science and more of them were, perforce, self-supporting.

The Association. The APSA has the third

⁷For the data underlying the following statements, I have relied on many sources. The chief one is *American Science Manpower 1968, a Report of the National Register of Scientific and Technical Personnel* (Washington, D.C.: National Science Foundation, 1969; NSF 69-38) as supplemented by a special report on political science from the 1970 Register, not yet in print, and *Reviews of Data on Science Resources* (NSF 70-50; No. 19, Dec. 1970); also the National Center for Educational Statistics, *Higher Education: Earned Degrees Conferred: (year) Part B—Institutional Data* (Washington, D.C.: Office of Education, various years: OE 5.254:54013-yr.); *Students Enrolled for Advanced Degrees, Fall (year). Part B—Institutional Data* (OE-54019-yr-B); National Research Council, *Summary Report 1970: Doctorate Recipients from United States Universities* (Washington, D.C.: NRC, various years); *Federal Funds for Research, Development and Other Scientific Activities, 1970-71*; *Survey of Science Series* (Washington, D.C., NSF, 1971); and the BASS report by Neil Smelser and others, *Sociology* (Englewood Cliffs: Prentice-Hall, 1969). I wish to thank my research assistant, Hiroko Shiozaki, for many long hours spent in locating data and calculating statistics.

⁸The criteria employed for inclusion in the Register were as follows: 1968—"A master's degree in political science or 2 years of graduate work with one year of professional experience; or a Ph.D. in political science; or substantial professional achievement in political science; or the equivalent in professional experience. In 1970 the last clause was omitted and the next to the last clause was modified to read "... professional achievement in political science as evidenced by contribution to the professional literature." See *PS*, IV (1971), 33.

largest membership of the eight social science (including history and law professors) professional associations and it has grown more rapidly than any of the others in the past decade. On the other hand, financially our Association has increased its budget and resources less rapidly than most other associations, although APSA expenditures in 1970 were second only to psychology. About two-thirds of the political scientists with doctorates in the field are members of the Association, and less than a tenth of those with masters degrees in political science have joined. On the other hand, something like half of the political science graduate students are members—a higher proportion, I believe, than that of any other discipline.

Enrollments and student body. The number of political science majors has increased over the past ten years more rapidly than most natural sciences and humanities and more rapidly than any of the other social sciences except psychology and sociology. In this sense, undergraduate student interest in our subject is relatively high. On the graduate level, there are more political science enrollees than in any other discipline except history and psychology, and the rates of change suggest that this will not be altered rapidly, even though psychology, anthropology and sociology are increasing their enrollments more rapidly than we are. In the last four years there has been in all fields a declining rate of increase and by now an actual decrease in doctorates in some fields, such as international relations. (See Figure 1 and footnote 29)

As for the characteristics of these graduate students, something we shall refer to later, suffice it to say here that they are intellectually fully as able, by any measure so far employed, as those of other disciplines, with the exception of anthropology. Furthermore, for what it is worth, some studies show that these graduate students compared to those of almost all other disciplines come from homes of higher socioeconomic status.⁹ At the same time, except for the sociologists, they are more radical¹⁰—the usual mixture these days.

Morale and divisiveness. There are many is-

ues that divide us (teaching versus research, elite institutions versus others, behavioralism, and so forth); it may be asked whether a discipline so torn and wracked by division and uncertainty can pull itself together to govern itself and, beyond that, to engage in the science management activities I have described. The answer is "yes." The evidence for this is: (1) when we increased the dues for annual members from \$15 to a range up to \$30, in a depression year, we increased association membership by 3.7 per cent. (2) In 1969 when the American Council on Education asked junior and senior scholars to rate graduate programs, the junior scholars' ratings correlated with senior scholar ratings .98.¹¹ (3) When the ACE asked over two hundred members of each of several disciplines whether the educational programs they rated were getting better or worse, the ratios of "better" to "worse" in four social science disciplines were as follows: psychology 127, economics 165, sociology 191, and political science 257.¹² Since this is the only comparative study of morale we have, it deserves consideration. (4) When, in 1969, the mixed faculty-graduate student University of Oregon study group asked graduate students at 12 universities whether or not their graduate program was equipping them for the careers they had in mind, 79 per cent said yes and only 12.5 per cent said no.¹³ (5) When in 1966 Luttbeg and Kahn asked 56 department chairmen whether the behavioral-traditional split was seriously divisive in their departments, three quarters of them said no.¹⁴ Dahl's "epitaph for a monument to a successful protest" deserves a fresh wreath.¹⁵ And finally (6) given our concern about coalitions and congruent lines of conflict, we should note that "there is little relationship between the divisiveness of faculty in political science departments and the divisiveness of students."¹⁶

⁹ Kenneth D. Roose and Charles J. Andersen, *A Rating of Graduate Programs* (Washington, D.C.: American Council on Education, 1970), p. 34.

¹⁰ Roose and Andersen. Calculated from reported data for each of the disciplines.

¹¹ Unpublished report sent to me (8/6/71) by John Orbell and Lawrence C. Pierce, Department of Political Science, University of Oregon.

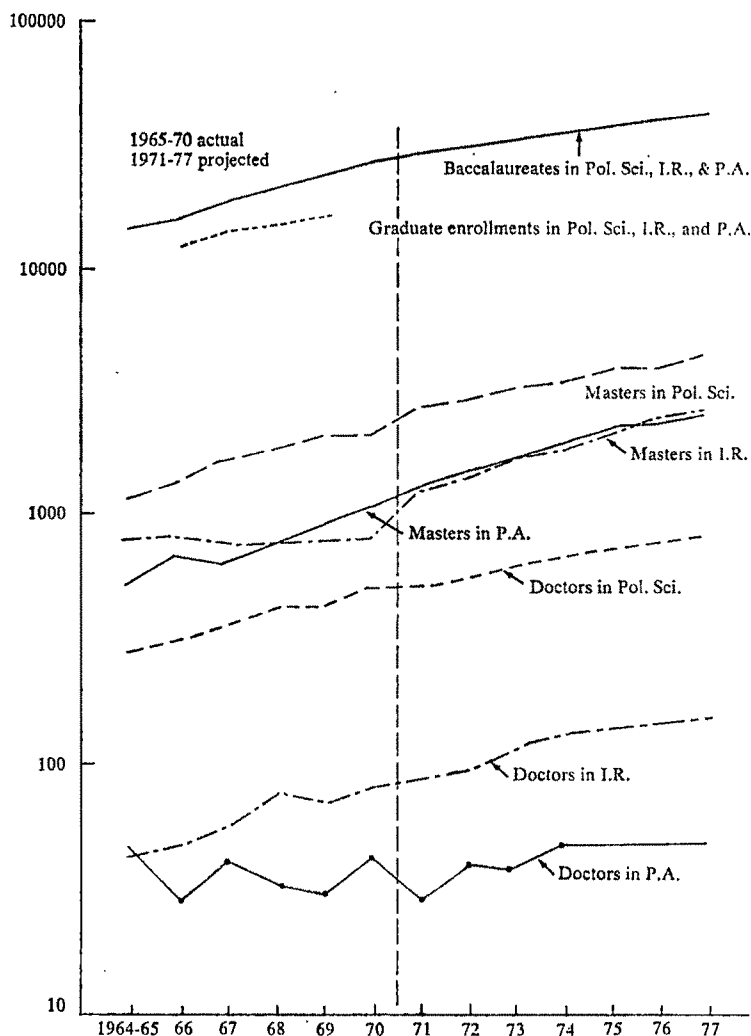
¹² Norman Luttbeg and Melvin Kahn, *The Making of a Political Scientist: An Empirical Analysis of Ph.D. Programs* (Carbondale, Ill.: Public Affairs Research Bureau of Southern Illinois University, 1969), p. 67.

¹³ See Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review*, 55 (1961), 763-72.

¹⁴ John Orbell, Alvin H. Mushkatel, and Lawrence C. Pierce, "The Structure of Professional Education

⁹ James A. Davis, *Great Aspirations: The Graduate Plans of America's College Seniors* (Chicago: Aldine, 1964).

¹⁰ Everett Carl Ladd, Jr. and Seymour Martin Lipset, "The Politics of American Political Scientists," *PS*, 4 (1971), 135-44. The inference that graduate students are, next to sociologists, the most radical, rests on the finding that the junior faculty in political science is among the most "radical" group in the social sciences—a little tenuous, but probably valid.



Sources: see footnote 7.

Figure 1. Earned Degrees in Political Science, International Relations, and Public Administration 1965-77

There is, of course, other evidence of division: junior scholars are more radical than senior scholars and they would accord students more of a voice in departmental affairs.¹⁷ Graduate students are dissatisfied with their training as teachers but not with their training as research scholars.¹⁸ There is widespread dissatis-

faction with the "relevance to contemporary problems" of much of our research—shared, incidentally, by students and faculty.¹⁹ And both students and faculty believe, contrary to much evidence, that our enterprise lacks the prestige or status it should have.²⁰ But on the

in Departments of Political Science," paper read at the Annual Meeting of the Western Political Science Association, April 1971, p. 17; also unpublished data from the same source.

¹⁷ Ladd and Lipset, "The Politics of American Political Scientists."

¹⁸ "Obstacles to Graduate Education in Political Science," Report of the APSA Committee for an Exploratory Study of Graduate Education in Political Science, *PS*, 2 (1969), 622-41.

¹⁹ "Obstacles to Graduate Education . . .," and Oregon study cited above in Footnote 16.

²⁰ In the past, surveys have consistently revealed that college professors rank high in occupational prestige. For example, in Paul K. Hatt and C. C. North, "Prestige Ratings of Occupations," in S. Nosow and W. H. Form, eds., *Man, Work, and Society* (New York: Basic Books, 1962), pp. 277-83, college professors rank 7th out of 90 occupations, well ahead of bankers and lawyers. Nevertheless, as reported in *The Academic Mind* by Paul Lazarsfeld

evidence as I see it, the division within the discipline, our dissatisfaction with our own research, and our sense of lack of public appreciation are not such as to hamper our common effort to improve the quality of the discipline.

The salient features of this discipline, then, are that it is modestly trained (in terms of proportions of Ph.D.'s), is relatively more concerned with teaching and administration than research, is located largely in academia, is relatively poorly paid (except in government), is poorly financed at both faculty and student level, has a professional association with more members than money (but even so fails to include many practitioners of the art), has disproportionately increasing enrollments at the undergraduate level and a levelling off of enrollments at the graduate level, has excellent students, and finally, has a membership divided on many issues but demonstrably capable of common effort.

I have briefly outlined the concept of disciplinary nurturance of science management and I have sketched a portrait of our discipline.

III. Graduate Training in Political Science

The problem for disciplinary nurturance or science management as it belatedly comes to grips with graduate education, is to think in terms of discipline-wide aspects of the field; the individual universities will take care of the teaching and to a large extent the curricular planning. A discipline-wide point of view is different from a departmental point of view in the following respects: (1) it often sees the virtues of the diversity of programs and specializations and the wastes of duplicated efforts, whereas the individual department may try to be like other departments, straining its resources to meet too many objectives within its own confines. (2) To the person with a disciplinary perspective, the mobility of faculty is a more or (often) less efficient method of personnel allocation and of improving individual satisfaction; to the department, mobility means the loss of a valued person (or relief that he is going) and the opportunity or requirement to hire a substitute. (3) The discipline-wide perspective offers possibilities for both special studies of common problems, that are too expensive for a single department, and for the simple exchange of in-

formation on curricula, courses, teaching methods, and so forth. At the department level, these matters are viewed idiosyncratically, often personally. In our efforts to understand what we are doing, everything that has been said of the advantages of comparative studies in politics applies to comparative study in education; perhaps even more so. The reason for this special importance has to do with the fact that we are studying ourselves, hence, without alternative models, we are unlikely to have perspective on the possibility or wisdom of change.²¹

The institutional complex. The Association has no committee concerning itself with graduate education, no staff person responsible for collecting and exchanging information about graduate education, indeed, little demonstrated concern, aside from a rather uninformative report ten years ago and a student inquiry two years ago.²² The costs of this neglect are substantial. The best we can do here is to sketch the general outline of the enterprise and propose some tentative recommendations.

There are about 108 institutions offering doctorates in political science, of which 74 were included in the 1969 American Council on Education survey.²³ Of these, 22 were rated "distinguished" and "strong." These superior institutions produced (1969) about half of the doctorates in political science (18 per cent from the six distinguished universities; 34 per cent from the 16 strong ones.) There were another 22 programs rated "good" or "adequate plus" which in 1969 all together produced another 24 per cent of the doctorates. There then remained in the ACE study 30 doctoral programs generally considered marginal or totally inadequate but included in the report because

²¹ For discussion of resistance to change in university faculties, see Theodore Caplow and Reece J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958); Logan Wilson, *The Academic Man* (London and New York: Oxford University Press, 1942); Bernard Berelson, *Graduate Education in the United States* (New York: McGraw-Hill, 1960).

²² "Political Science as a Discipline: A Statement by the Committee on Standards of Instruction of the American Political Science Association." *American Political Science Review*, 56 (1962), 417-21; *Goals for Political Science: Report of the Committee for the Advancement of Teaching, APSA* (New York: Sloane, 1961); James W. Fesler and others, "Goals for Political Science: A Discussion," this *Review*, 45 (1951), pp. 996-1024. Somit and Tannenhaus's study mentioned above, and an earlier volume by the same authors, *American Political Science: A Profile of a Discipline* (New York: Atherton, 1964). The cited graduate student inquiry is the "Obstacles" report mentioned above. See also Luttbeg and Kahn and John Orbell and others, also cited above.

²³ K. D. Roose and C. J. Anderson, *A Rating of Graduate Programs*, pp. 64-5.

and Wagner Thielens, Jr. (Glencoe: Free Press, 1958), social scientists believe they would be ranked lower than businessmen and lawyers. The low estimate of occupational prestige by graduate students is reported in the APSA "Obstacles to Graduate Education" study cited above: only 32% are even slightly "satisfied with 'the prestige of political science today.'"

they were located in institutions that had been in the business some time and were producing doctorates. Finally, there are about 34 institutions offering doctorates in political science, not even included in the ACE report because they were too new or too weak, or often both. This complex of 64 inadequate programs, more than half of the total, turned out a quarter of the doctorates in that year—a possible tragedy for the discipline and for the individuals receiving this weak training.

Eleven years ago Berelson said “more students are needed more than more programs,”²⁴ and, with some modifications about the “more students,” this is equally true today. So far as I can tell from published reports, 20 programs (many of them new) had not awarded a single doctorate in the thirteen years up to 1969, while 16 others had awarded from one to three in this period.²⁵ They offer, but almost no one comes, or at least almost no one finishes. This does no great harm for those who do not enroll, but those who do enroll and don’t finish are likely to have wasted their precious years in nonproductive study.

At a guess, I should judge that about 35 new political science doctoral programs have been established in the twelve years up to 1969 or about three a year.²⁶ New programs are almost always weak ones. For example, of the ten new programs in the ACE study (those in institutions with some modicum of strength) half were judged clearly marginal or inadequate, and only three were judged adequate or better. One of the reasons for this weakness is that it takes time to create both a faculty and a tradition of distinguished research and graduate study, a fact revealed in the clear relationship between a university quality rating (based on ACE data) and the years elapsed since a doctoral degree (in any field) was established: the median years for quality A group is 82 years,

quality B is 66, and quality C is 30 years.²⁷

On balance it appears that the reasons for establishing a new research-oriented doctoral program are clearly local; the educational requirements of the national discipline do not call for many of these new programs, although some regional needs may modify this judgment. Furthermore, had the Association been alert to the clear warnings eleven years ago²⁸ (in spite of the recent BASS miscalculations) the current overextension of graduate research training might have been avoided and the cost of our inadequate attention to these problems might have been reduced. Perhaps we have learned a lesson.

But the graduate training enterprise is much larger than the 108 institutions offering doctorates; there are in addition at least 104 institutions offering masters but not doctorate programs in international relations or political science. We will return to these in an Appendix, but taking these together we can estimate graduate enrollment. There are well over 13,000 students registered in advanced degree programs in political science and international relations—next to psychology the largest enrollment in the social sciences.²⁹ We have no idea of the size of the faculty teaching these students, certainly it is over 3,000, many of whom do not have doctoral degrees.³⁰ The cost of this enormous enterprise is about \$109 million dollars per year.³¹ The produce of this effort, aside from some valuable training for those who drop out, was in 1969 about 600 doctorates

²⁴ National Science Board, *Graduate Education*, p. 57.

²⁵ See Bernard Berelson, *Graduate Education*, pp. 70–80.

²⁶ These data are from the Office of Education *Enrollment* series mentioned above. The figures for recent years are: 1966 10,438; 1967 11,880; 1968 12,326; 1969 12,862. These are combined political science and international relations enrollment figures. The percentage increases in doctorates for 1967, 1968, 1969 are: political science: 13%, 6%, 5%; international relations 16%, –7%, 0.3%.

²⁷ The 1968 Register is inadequate for two reasons: (1) political science was included at the last minute and the survey was limited to members of APSA, and (2) there are many more political scientists, judged either by degrees or positions than are members of the APSA. This inadequate data base gives a total of 5,176 political scientists, of which 3,921 were employed by educational institutions. Of this latter group, 2,567 or 65% had doctorates. The 1970 data are better but do not yet provide the detailed breakdown necessary for these calculations.

²⁸ Estimated by taking the NSB estimate of the total cost of graduate education in 1969 (6.4 billion) and multiplying by the proportion of political science graduate enrollees to total graduate enrollees in 1969 (1.7%) = \$108,800,000.

²⁴ B. Berelson, *Graduate Education*, p. 106.

²⁵ This statistic, by no means easy to come by, is an inference from the fact that none of them was included in the ACE survey (which implies that none of them produced a Ph.D. in political science in the period from 1957–1966) and none of them awarded a degree in 1967, 1968, or 1969 (*Earned Degrees* series.) The data in the next paragraph come from the same source.

²⁶ Estimated from the above lists and a sampling of past reports on earned doctorates. This compares with a gross increase in the number of institutions offering the Ph.D. of 6.5 per year (1957–66) less those discontinuing such degrees, 2 per year; making a net increase of 4.5 per year. See National Science Board, *Graduate Education: Parameters for Public Policy* (Washington, D.C.: NSF, 1969), p. 30.

and 3,000 masters degrees (in political science and international relations, but not public administration.)

Geographic Distribution and Regionalization. In 1969 the National Science Board recommended that every state and every urban center of over 500,000 population be encouraged and helped to develop a major university. The argument rests on grounds of "equitable distribution of Federal funds for research and development," including access to the benefits of higher education by all segments of the population, "the benefits to society . . . directly realized by the interaction of the institution and the local population," and the influence of a university on the economic development of a region.³² For political scientists there is another argument: the mutual advantages of close working relations between a state or urban government and a university.

The contrary argument, of course, is expressed in the points made above: we do not need more programs, but better ones; if we need more scientifically trained persons, candidates should be directed to those quality institutions that have excess capacity. Since mobility increases with level of training, there are strong arguments for community colleges, but given the mobility of doctoral candidates, it would be better to bring them to the training institutions than to bring the institutions to them. The regionalization of master's programs has advantages and disadvantages: to the extent that they train for secondary education or local "practice" they might well be regionalized. But on the other hand, they should not be divorced from close association with doctoral training programs.

The regionalization of the markets (output as contrasted to input), for persons trained in some doctoral programs is already a feature of the system—but only for the lower quality institutions.³³ Because of the desirability of heterodoxy and cross-fertilization and because of the current availability of well-qualified persons from national institutions, regionally defined doctoral markets should be discouraged.

Given these considerations, it seems to me that, contrary to the NSB recommendations, our policy should be to encourage the strengthening of existing institutions before new ones are created, at least until a special case is made for a particular place. In 1964–65 the five

states with the lowest per capita graduate enrollments were: Texas, Virginia, Georgia, North Carolina, and Ohio. Four metropolitan areas with populations of over 500,000 and no graduate enrollments were: Gary, Norfolk, Tampa, and Youngstown.³⁴ The argument for new political science doctor's programs in these areas has yet to be made.

The distribution of quality is another, but related, matter. In political science, the South, with 31 per cent of the population has only one third of its share of quality programs (2 out of the top 22 in the ACE 1969 rating). More important for political science nationally is the low quality and high production record of the programs in the Washington, D.C. area. None of the five Washington programs in the ACE report received a rating as high as "adequate plus," yet they produced 165 doctorates in the ten years measured, or 7 per cent of the total. The development of a strong coordinated doctoral program in that area is a matter that should concern both the profession and the faculty of the Washington programs.

Changing quality of graduate education. Concern over the quality of graduate education and the expansion of programs is chronic; for example, in 1934 the Association of American Universities formally resolved that it "views with concern the growth of the number of institutions conferring the Ph.D. degree in fields in which . . . they are not adequately staffed or equipped for work."³⁵ There are three aspects to the problem: (1) the quality of training at the best universities, (2) the average quality, as affected by the new or inferior programs, and (3) the opportunity cost of those who enroll in inferior programs, combined with the quality of training of those displaced by the products of the new programs.

On the first of these, eleven years ago Berelson said, "as far as the paper standards are concerned, they have gone down," and he cited the loss of the dissertation publication and the language requirements, and what he believed were the less stringent examination requirements.³⁶ I do not agree, nor does the political science panel (or other social science panels) employed in the ACE report.

(2) The *average* quality of doctoral training may have declined, however, through the entry of new and weak programs. This would occur if the better programs produced decreasing proportions of the doctorates in a field—as they are bound to do if the higher quality programs

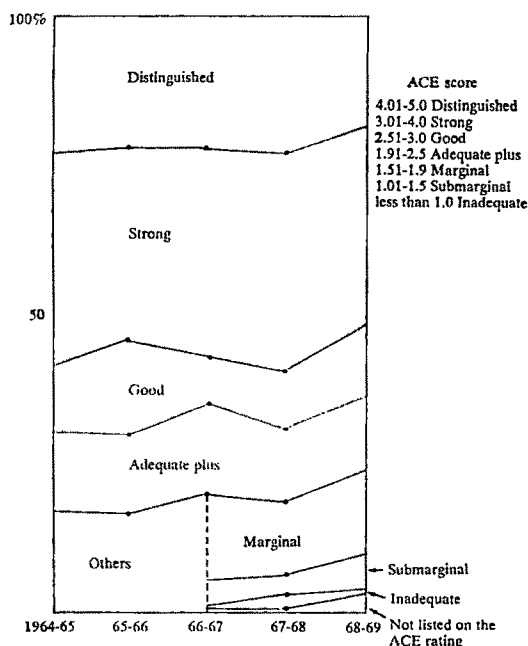
³² National Science Board, *Graduate Education: Parameters for Public Policy* (Washington, D.C.: NSB, 1969), pp. 35, 40; and NSB, *Toward a Public Policy for Graduate Education in the Sciences* (1969), p. 26.

³³ B. Berelson, *Graduate Education*, p. 118.

³⁴ NSB, *Graduate Education*, pp. 41, 42.

³⁵ Quoted in Berelson, *Graduate Education*, p. 31.

³⁶ Berelson, p. 217.



* ACE, *A Rating of Graduate Programs*, Washington, D.C.: 1970.

Figure 2. Percentage of Ph.D. Degrees in Political Science, By Quality of Program*, 1965-69

do not expand and the new programs attract the students they hope for. At this point the answer is not clear for many of the newer programs are growing and our most recent data are for 1969. As indicated in Table 1 and Figure 2, in 1966 54 per cent of the doctorates in political science were awarded by distinguished and strong departments (as measured in the 1969 ACE rating); in 1967 the percentage was 56 per cent; in 1968 it was 58 per cent; but in 1969 it drops to 52 per cent. Comparatively, this 1969 proportion is about the same as that

of the sociologists and historians, higher than that of the psychologists and economists, lower than that of the anthropologists. But Berelson's calm view of this growth process as seen eleven years ago is important to note:

The production of more second-class doctorates, even though they may lower the average, does not affect the production of first-class doctorates. We shall have as many or more of the latter *plus* the former as well.⁸⁷

(3) If the candidates enrolling in the new or inferior programs would otherwise have gone to better quality programs with excess capacity, there is a net loss in disciplinary quality. If they would not have applied, or if they would not have been admitted if they had applied, *and* if they take the places of persons with training less suited to their jobs (a master's degree in college teaching or policy analysis, for example), there is a net gain in quality of instruction and practice. The average figure is irrelevant. I am reasonably sure that the products of these new programs do take positions that otherwise would have been filled by persons with only the master's degree, but there is no way of knowing whether or not the establishment of new doctoral programs in their own institutions or regions affected their decisions to take a doctorate in political science.

Criteria for the structure and size of departments. In undergraduate education the fewer the students per faculty member the better the education. In graduate education, the National Science Board found that "as the quality of the graduate program [measured by ACE ratings] . . . increases, so does the student-faculty ratio." that is, the better the program the more students per faculty member.⁸⁸ On the other hand,

⁸⁷ Berelson, p. 109.

⁸⁸ NSB, *Graduate Education*, pp. 93-4.

Table 1. Percentage of Ph.D. Degrees in Political Science, by Quality of Program,* 1965-69

Type of Institution		No. in Group	% Total (N=101)	Percentage of Ph.D.'s Awarded				
ACE Score				1964-65	1965-66	1966-67	1967-68	1968-69
4.01-5.0	Distinguished	6	6%	23%	22%	22%	23%	18%
3.01-4.0	Strong	16	16	36	32	34	36	34
2.51-3.0	Good	10	10	11	16	9	10	11
1.91-2.5	Adequate plus	12	12	13	13	14	12	13
1.51-1.9	Marginal	15	15	—	—	15	12	13
1.01-1.5	Submarginal	12	12	—	—	5	5	6
Less than 1.0	Inadequate	3	3	—	—	0.5	2	0.6
Not included in the ACE list		27	29	—	—	0.8	1	3

* Rating according to the 1969 survey of graduate education by the American Council on Education. ACE, *A Rating of Graduate Programs* Washington, D.C.: 1970.

the Oregon study cited above found that the fewer students per faculty member, providing there were enough students, the better the "organizational climate," that is, the more informal and relaxed were faculty-student relationships.³⁹ Such an organizational climate is not related to the ACE quality ratings; it may or may not be found in the less distinguished universities. While the relationship between quality of program and high student-faculty ratios is probably an artifact of the greater attractiveness, hence enrollments, in these programs, there is something else which both the Oregon study and the NSB study support. Students learn from each other and a large number of students is important for this process to take place. In the Oregon study, 69 per cent of the graduate student sample were satisfied with student-student teaching, while only 40 per cent were satisfied with the formal programs.⁴⁰ For what it is worth (given the artifactual nature of the NSB conclusions about the desirability of a high student-faculty ratio), it may be reported that of the sixteen sciences and social sciences in the NSB study, the distinguished political science departments ranked third in "median graduate student-faculty ratio" (5.88), and even the marginal departments had high ratios.⁴¹ It seems to take fewer political scientists to teach graduate students than it does for the faculties of other disciplines.

Next, there is the question of the minimal size of a faculty that is required before a department should enter into graduate training. The National Science Board finds that the advantages of interaction among scholars, their mutual stimulation, the communications of new ideas, the representation of various approaches to a field, have a joint theoretical basis in the rapid rate of increase in combinatorial possibilities for conversational groups up to seven. Empirically, the NSB finds that of the 16 disciplines in their sample the minimum department size for the distinguished departments is, in fact, seven.⁴² But, unless the program is limited

³⁹ J. Orbell and others, "The Structure of Professional Education," p. 11. These authors find the relationship to be a weak one and they also find that the "organizational climate" is better in the larger universities.

⁴⁰ Unpublished data from the Oregon study cited above.

⁴¹ NSB, *Graduate Education*, p. 96.

⁴² After the seventh person the increment in combinatorial possibilities reaches its asymptote and levels off; at that point the addition of one more person doubles the number of combinations, but this doubling rate does not increase further (NSB, *Graduate Education*, pp. 97-8). A critique of this theory might point out that the doubling of a good thing, if it works that way and disutilities do not begin

to a very narrow range of specialties, perhaps mathematical political science, or, at most, a subfield like international relations, seven is too small for political science. With five major commonly used (although intellectually hard to justify) subfields in this discipline, and with the requirement that each subfield be represented by more than one faculty member, ten is minimal faculty size for doctoral programs in political science. In fact, the smallest of the 22 distinguished and strong departments on which we have data is 12.

Without some idea of the actual size of distinguished and strong departments, this is misleading. In the sixteen disciplines in the National Science Board sample, the median size for "distinguished" departments is 28.5; for "strong" departments it is 20.0. In political science, these median sizes are: "distinguished" about 40; "strong" 34.

Graduate instruction should be done by established research scholars; empirically the ranking of graduate programs declines as the proportion of full professors to the rest of the faculty declines.⁴³ This is for some obvious reasons, but among others, it is clear that the emergence of a graduate student's sponsor (or dissertation supervisor) is directly related to the student's later achievements, his placements, his creativity. And this, in turn, is because of the crucial importance of an early start along fruitful research lines. Indeed, the prestige of the student's sponsor is more closely related to his later productivity than the quality of the training institution—although, since closeness of the student-sponsor relationship is not a factor, the causal force here may be the selection by bright students of the most imaginative and productive faculty members. But the point is that on the principle of comparative advantage, the more distinguished research professors should spend more time with graduate students and less with undergraduates.

Admissions. In 1903 William James stated that the Ph.D. requirement for university employment meant the recruitment of "men without marked originality or native force . . . the unfit in the academic struggle for existence." In 1909 President Lowell of Harvard warned, "There is a danger of attracting an industrious mediocrity which will become later the teach-

to appear (such as anomie), is a good rate of return on any investment.

⁴³ In sixteen natural and social science fields the institutions characterized by a high proportion of distinguished departments had a median percentage of faculty positions held by full professors of 42 per cent; in the institutions of lowest quality this was 28 per cent (NSB, *Graduate Education*, p. 91).

ing force in colleges. . .⁴⁴ These themes, as in so much else having to do with our enterprise, hardly change over the years. The problems are (1) attracting and selecting the best, and (2) developing the talents of students to the fullest extent possible.

For political science, attracting and selecting the best requires (a) attracting more persons with mathematical aptitude (our students already have the highest—or second highest—verbal ability scores in the social sciences),⁴⁵ and more persons with creativity; and (b) devising better measures for predicting achievement both in and beyond graduate school. The verbal and quantitative ability scores on the GRE, like other intelligence tests, are poorly related to life achievement, including scholarship, and related in curious and idiosyncratic ways (by field and institution) to graduate school achievement.⁴⁶ Further, we do not take advantage of measures of creative potential that identify important abilities only partially identified by intelligence type tests. The matter is complex, but since results are field-specific the discipline has an independent stake (separate from that of the graduate deans) in general ability measures as well as the advanced field tests.

Preparation is important: economics majors do as well as political science majors in our graduate work (as judged by advanced placement scores a year or two after admission to graduate school) and a minor in economics is particularly helpful. Within our own discipline specialists in political thought do best.⁴⁷ What these have in common, I think, is their training in the use of abstractions.

As for the distinctive qualities needed for teaching and "practice" (work in government), it has long been agreed that we should admit by criteria of intelligence (thus specified and broadened) and screen for other qualities later.

The best development of the pool of talent that is attracted and selected for the discipline is contingent on the strategic use of our

combined educational resources. There is every reason to admit "from the top down," maintaining full cohorts at the distinguished and strong universities at the cost of empty seats at the newer and weaker programs. This is not only good for the individuals concerned (the place of training marks a man for life—in that sense the academic class system is a caste system),⁴⁸ but it is good for the discipline as a whole, including, in the long run, the newer institutions and colleges that will later employ these new doctorates.

Program content. There is space here for only a few suggestions on doctoral programs: (1) Programs should be more diversified and specialized—we tend to copy each other too much; (2) conceptual analysis is a better preparation for a changing discipline and state of knowledge than is "mastery of its content"—it prepares for the utilization of new knowledge; (3) those who take methods courses do better on content tests⁴⁹—methodology may improve understanding of content; (4) while general social science programs tend to be "thin" and often fail, bilateral interdisciplinary training, as in sociology and law, psychological study of politics, political economy, serve useful purposes—emphasizing depth rather than breadth does not mean staying within disciplinary boundaries; (5) while "professionalization" is discouraged by graduate faculties,⁵⁰ economics and psychology offer single programs that serve both future "practitioners" and academic research scholars—we should develop our analytical applied fields, especially policy analysis; (6) our dissertations are the longest of any discipline (in a sample of 22 in 1958)⁵¹ and they should be shortened, with experience given in writing article length pieces; (7) in general, central intelligence on graduate programs, an exchange of information on them, will pay for itself many times over.

III. Creativity and Productivity

It is the function of a discipline to explain and interpret a certain range of phenomena. The nurturance of a discipline means, above all,

⁴⁴ Quoted in Berelson, *Graduate Education*, pp. 18, 23.

⁴⁵ Special report from the Educational Testing Service on 1970 scores for students taking the advanced test in political science. Also, Philip R. Harvey and Gary L. Marco, *An Analysis of the Graduate Record Examinations Scores by the Undergraduate Major Field of Study, 1963-64* (Princeton, N.J.: ETS, Aug. 1965).

⁴⁶ Gerald Lannholm, *Review of Studies Employing GRE Scores in Predicting Success in Graduate Study, 1952-1967*; and G. V. Lannholm, G. L. Marco, and W. B. Schrader, *Cooperative Studies Predicting Graduate School Success* (both Princeton: ETS, 1968).

⁴⁷ Special report from ETS; see footnote 45.

⁴⁸ See the works of Berelson, Folger et al., and Wolfe cited above; also Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition," *American Sociological Review*, 30 (1965), 699-714. Hiring of one's own doctorates is directly related to quality: in the "top twelve" 47 per cent of the faculty were trained by the university that employed them (1958); in the next ten 27 per cent were inbred; the next group 20 per cent. Berelson, p. 115.

⁴⁹ Special report from ETS; see footnote 45.

⁵⁰ Berelson, *Graduate Education*, p. 86.

⁵¹ Berelson, p. 181.

the improvement of these explanatory capacities.

A. Creativity. What is creative political or social science?⁵² It is formulating an unconventional or novel question, as in Schelling's inquiries into rational strategies for "exploiting potential force," or the turning around of old questions, as the authors of *The Authoritarian Personality* did when they reversed questions on anti-Semitism to ask about the ethnocentric person. It is a novel juxtaposition of things not put together before, as in Herbert Spencer's use of evolutionary theory to "explain" the probable outcomes of political intervention in a system, or as in Deutsch's use of communications theory to explain facets of international relations. It is a perception of a new "figure" or theme against a familiar background, as in Lasswell's discernment of displacement mechanisms in political thinking, or the Supreme Court's finding of a "right to privacy" in the Constitution. It is the discovery of new logical relationships in old materials, as in Arrow's analysis of *Social Choice and Individual Values*, or in Popper's substitution of criteria of falsification for verification of universal propositions. It is in the development of new paradigms for analysis, as in Wiener's development of the ideas of cybernetics, or Almond and associates' restatement of elemental political processes and political crises, or in Daniel Lerner's theory of modernization. It is the invention or novel use of new methodologies, as in L. F.

Richardson and Quincy Wright's quantitative and mathematical studies of war, or in McPhee's or Simon's, or Pool, Abelson and Popkin's computer simulation of social and political systems. It is in the definition and elaboration of a new field of study, as in Luther Gulick and Leonard White's development of the field of public administration. It is in the discovery of ordered relationships in a novel way as in S. M. Lipset's study of the conditions of stable democracy, or in Miller and Stokes's empirical study of representation, or in Riker's studies of political coalitions. It is in the successful use of metaphor and analogy as may be seen in Thurman Arnold's *The Folklore of Capitalism* and Veblen's *Theory of the Leisure Class*.

Deutsch, Platt and Senghaas identify "major advances" in the social sciences with creativity: such advances must help people "to see something not perceived before," or "create the possibility of doing something that had not been done before" (and have a "substantial impact that led to further knowledge").⁵³ The ingredients of this creativity are the recombination of elements into new wholes or decomposition of wholes into new elements, discernment of new patterns or orders, the reformulation of questions, the invention or novel application of new methods, and above all, the reconceptualization of a problem or field.

As a process, creativity has been said to involve four stages.⁵⁴ The first is *preparation*, the learning of the materials and content of a field a stage which can, if improperly handled, destroy the possibility of creativity by encouraging premature closure and too great deference to authoritative scholars and materials. The second stage is *incubation*, the mulling and puzzling over some problem, sorting things out, tentative search procedures. It is at this stage that openness to fantasy, the promptings of the unconscious, the entry of heterodox ideas, come upon the scene. Almost all studies of creativity emphasize the importance of the unconscious free association process, and some specify that deliberate inattention to a problem, allowing the unconscious further scope, precedes the dis-

⁵² Like the study of the use of human resources, information exchange, and education, the study of productivity and creativity has developed a corps of professionals, a body of literature, and a growing corpus of knowledge. Chapter 8 "Determinants of Professional Achievement and Rewards Among Scientists" in the Folger *et al.* book cited above summarizes some of this material. See also: Calvin W. Taylor and Frank Barron, eds., *Scientific Creativity: Its Recognition and Development* (New York: Wiley, 1963); Morris J. Stein and Shirley J. Heinze, *Creativity and the Individual: Summaries of Selected Literature in Psychology and Psychiatry* (New York: Free Press, 1960); John C. Gowan and others, eds., *Creativity: Its Educational Implications* (New York: Wiley, 1967); Howard E. Gruber, ed., *Contemporary Approaches to Creative Thinking* (New York: Atherton, 1962.) Studies emerging from the work of the Institute for Scientific Information employing the Science Citation Index are interesting, especially Jonathan and Stephen Cole, "Measuring the Quality of Sociological Research: Problems in the Use of the Science Citation Index," *American Sociologist*, 6 (1971), 23-29; C. Roger Myers, "Journal Citations in Scientific Eminence in Contemporary Psychology," *American Psychologist*, 25 (1970), 1041-48. I am indebted to Melvin Weinstock of the Institute for Scientific Information for bringing these last two studies to my attention.

⁵³ Karl W. Deutsch, John Platt, and Dieter Senghaas, "Conditions Favoring Major Advances in Social Science," *Science*, 171 (1971), 450-59. See also critical letters and rejoinder, *Science*, (1971), 1191-92.

⁵⁴ Graham Wallas, *The Art of Thought* (London: Cape, 1926). For a brief review of the literature and further discussion of creativity as process, see Ralph J. Hallman, "The Necessary and Sufficient Conditions of Creativity," in J. C. Gowan and others, eds., cited above.

covery.⁵⁵ Simon argues that unconscious processes are identical to conscious processes, but he is virtually alone in this belief.⁵⁶ The third stage is *illumination*, when the germ of the new idea or solution enters consciousness, but, as is often pointed out, the stimulus to the new idea will only be recognized for what it is by a mind prepared for receiving it; Deutsch's familiarity with early cybernetics theories and his knowledge of previous studies of nationalism prepared him for the communications theory of national and international integration.

The fourth and more familiar stage is *verification*. Except, perhaps, for the use of novel methods or the invention of indicators and tests, verification is a standard procedure relatively well understood in the behavioral sciences. What is risky here is that the importance of verification be so stressed in graduate school that there is developed in the student an unconscious censorship of novel ideas. Almost all creativity studies mention the need for deferred judgment, for postponing the critical faculties until the new ideas have a chance to emerge. In short, rigor can stifle imagination (but imagination can also discourage rigor).

As a process, creativity can be encouraged and taught by conscious attention to its elements.⁵⁷ Students can be encouraged and shown how to avoid too narrow a definition of a problem at the outset, and also how to isolate elements of the problem from the background complexities. They can learn to identify and analyze the properties of these elements (e.g., the functional equivalents of Western "due process" in non-Western societies, the need-satisfying elements in social thinking, etc.). They can be encouraged to develop forced relationships and paradoxical statements for the stimulation of their imaginations; they can be taught to defer judgment and especially to postpone closure on any one solution before canvassing alternatives. The very notion of "idea production" is foreign to most of us, but there is evidence that encouraging the quantity develops

better quality, and that later ideas in an idea sequence are better and more original than earlier ones. Indeed, the encouragement to state research as problem solution, rather than as topics for investigation, often helps creativity, hence some attention to anomaly and problem-icity will prove useful.

As a discipline we can encourage this rare quality of creativity in several ways. (1) I mentioned the "creation" of the field of public administration; attention might be paid, therefore, to the ways in which new fields are created by *branching*, as political socialization branched off of the field of attitude formation which had previously branched off of the broader field of political parties and public opinion. Branching develops new knowledge partly by intense concentration on a smaller area, but also through the process illustrated by the envelope curve: as the explanatory power of a new set of ideas levels off, those ideas then serve as a platform or culture for another new set of ideas with a thrust of their own.⁵⁸

(2) We can encourage synoptic thinking as a substitute for aggregative thinking, that is, the integration of separate fields of knowledge through the use of new and more abstract concepts, such as "system maintenance" or "interest aggregation."

(3) The conceptual tools for analysis are underemployed in political science. The current apparatus includes game theory, cybernetics, exchange theory, collective choice models, and various kinds of simulation. On the threshold of acceptance are ecological models, mutation models (as in the analysis of language), diffusion models (including epidemiology), and energy-entropy models. These items of intellectual machinery and metaphors have enormous capacities to stimulate the imagination as well as to give precision to analysis.

(4) The integration of concepts themselves awaits exploration: one thinks of the marriage of structural-functional (teleological) models with causal modelling; of developmental and economic theory (Freud used both to good effect, even speaking of "the economy of the mind"), or of the employment of cost-benefit analysis in normative theory, a natural integration since they both deal with the same thing: values given up for values gained.

(5) Creativity is not limited to pure science; it is equally needed and possible in the policy sciences, first in identifying problems and then

⁵⁵ See Sigmund Freud, *On Creativity and the Unconscious, Papers on the Psychology of Art, Literature, Love, Religion*, edited by Benjamin Nelson (New York: Harper, 1958); also reports by creative scientists and mathematicians in Brewster Ghiselin, ed., *The Creative Process* (New York: Mentor, 1955).

⁵⁶ Herbert Simon, "Understanding Creativity," in Gowan and others, *Creativity: Its Educational Implications*, pp. 43-52.

⁵⁷ See Sidney J. Parnes, "Education and Creativity," in Gowan, pp. 32-43, and especially the seminal piece by J. W. Getzels and P. W. Jackson, "The Highly Intelligent and the Highly Creative Adolescent," in C. W. Taylor and F. Barron, eds., *Scientific Creativity*, pp. 161-72.

⁵⁸ See Daniel Bell, "The Measurement of Knowledge and Technology," in Wilbert Moore and Eleanor B. Sheldon, eds., *Indicators of Social Change* (see footnote 4).

in inventing solutions. For example: how shall we develop the moral equivalent of community in a contract society? Or, if freedom of movement concentrates social problems: what selective disincentives to migration should be combined with more direct attacks on the problems at their sites?

Creativity is sometimes studied in terms of the *capacities of individuals*, or as the product of certain personality characteristics. To the extent that these capacities are fixed at the time of student selection, the problem for the discipline is one of selecting the most creative students. One thing is clear: creativity is related to but not the same as the intelligence measured by intelligence tests. Measured separately (through word association tests, imagining various new uses for things, discernment of hidden shapes, making up fables, inventing problems), creativity predicts scholastic achievement as well as intelligence tests do.⁵⁹ Moreover it is a relatively general faculty (not subject- or situation-bound). It is associated with social heterodoxy and a sense of humor, and involves a relatively high risk intellectual strategy. In some studies it is seen to be positively associated with dominance, with self-esteem and self-confidence, with the kind of personal security that permits regression and fantasy because an assured return to reality is present,⁶⁰ and with the exceptional courage needed to challenge conventional thought as well as some guilt over this challenge process.⁶¹ The important point here is that if it may be reliably measured, such measures should be employed in our graduate student selection. There are obstacles to the selection of creative minds: some studies show that teachers prefer the highly intelligent (high I.Q.) to the highly creative.⁶² Thus letters of evaluation may be misleading. But at least we should look for it.

In puzzling over the flowering of creativity in the natural sciences compared to what he thought to be a lower rate of creativity in the social sciences, Wiebe concludes that one important cause for this is the greater discouragement of heterodoxy in the social sciences. To counteract this, he suggests:

If the social scientists are to increase creativity in their field, they should select their young people with an eye to intellectual audacity, personal

courage, and the kind of psychological autonomy that can weather the pressures toward conformity that characterize the fields in which social scientists work. Such people having been selected, their professional training should blend academic rigor with the promotion of dissent, skepticism, unorthodoxy, and audacious innovation.⁶³

As indicated in these words of Wiebe's, the environment in which creativity flourishes is both important and subject to some degree of control. Following one line of thought, Maslow's, we might say that the identification of creativity with the process of self-actualization (developing one's potentials), taken together with the belief that self-actualization takes place only after other basic needs, such as safety and "belongingness," are reasonably satisfied, means that some attention to these basic needs is called for.⁶⁴ Thus the provision of tenure for faculty and of guaranteed fellowships for a period of time for students helps satisfy the need for safety, while the development of a better organizational climate (warm faculty-student relationships and non-competitiveness) helps satisfy the "belongingness and love needs," both serving to promote creativity.

The earlier argument for encouraging some professionalism (as contrasted to academic emphases) in our graduate programs may, through a tendency to define problems more narrowly, discourage creativity. But given what we said above about the importance of creativity in identifying social problems and inventing solutions, the contradiction might be resolved by seeking, within the confines of professional training, to encourage creative policy solutions. High level business executives come out well on creativity tests.

The kinds of creativity identified by Deutsch, Platt, and Senghaas flourish in metropolitan areas, large universities, and well-financed research centers. More generally we might say that cosmopolitanism and the tolerance of heterodoxy, the achievement of a critical mass of innovative scholars and hence an appropriate on-campus atmosphere of stimulation, and the provision of time and resources are favorable environmental conditions.

Finally, the very lack of coherence in the discipline that we all deplore may imply less pressure towards orthodoxy; the state of knowledge in the discipline is ripe for creative development—perhaps more than in economics or the law.

⁵⁹ J. W. Getzels and P. W. Jackson.

⁶⁰ Frank Barron, "Originality in Relation to Personality and Intellect," *Journal of Personality*, 23 (1957), 730-41.

⁶¹ Gerhart D. Wiebe, "An Exploration into the Nature of Creativity," *Public Opinion Quarterly*, 26 (1962), 391-97.

⁶² Getzels and Jackson.

⁶³ G. Wiebe, "An Exploration . . ." p. 397.

⁶⁴ A. H. Maslow, *Motivation and Personality* (New York: Harper, 1954), pp. 80-98.

B. Productivity. Measured by books and articles published, regardless of quality, political scientists are as productive as members of other disciplines and they seem to be more productive than they used to be. In a recent study of the productivity of biologists, psychologists, and political scientists, where the measure of productivity was weighted in favor of continuity of research (thus implying "impact") all three disciplines were equally productive.⁶⁵ Two earlier studies of productivity in mathematics and political science (1958) found 46 per cent of the mathematicians but only 25 per cent of the political scientists totally non-productive.⁶⁶ In 1960, Berelson found that 33 per cent of the psychologists and 66 per cent of the historians ten years from the degree had published nothing; the comparable figure for political scientists (not quite comparable since the data are for 1968 and the method is a little different) is about 13 per cent. Average publications for this group of ten year doctorates (books and articles) is a little lower for political scientists (3.5) than for psychologists (5.4) but higher than for historians (0.5).⁶⁷

Are political scientists becoming more productive? V. O. Key found in 1958 that 25 per cent of the doctorates awarded twenty years earlier had been totally nonproductive; I find that 17 per cent of the reporting political scientists in the *Biographical Directory* are nonproductive. The findings are noncomparable in many ways, but there is reason to believe that the indicated direction is correct; individually we are more productive of research than we

used to be. This, taken together with the increase in doctorates produced each year (about 10 per cent increase per year for the past ten years) has created the information explosion that concerns us all and lies behind the increased *Review* backlog and the increase in the number of journals—at the rate of about five new journals per year. If all political science doctorates produced articles at the rate of those in the *Biographical Directory*, the group of doctorates from one to eight years from their degree would alone have produced about 1,000 articles in 1970–71, enough to fill twenty years of the *Review*. Similarly, they would have produced about 588 books in that one year, almost twice the number that could be reviewed in the *Review*.⁶⁸ Fortunately (I guess) these figures overestimate research productivity because (1) there is an attrition in the profession (about 20 per cent in the first five years), and (2) doctorates from the new programs are probably less productive. Nevertheless, something must be done about our abstracting and book reviewing capabilities.

C. Conditions of Productivity. The factors related to productivity are much better understood than those related to creativity; we will discuss them together.

1. *High intelligence.* Creative productivity studies using such sensitive measures as the citation index, reveal that after the minimal intelligence level is reached (one estimate of minimal I.Q. is 122, another is 130) additional intelligence is not related to research productivity—indeed the independent influence disappears when other factors are controlled.⁶⁹ Since political science gets its full share of the able and the very able—more than most social sciences—whatever defects our research productivity may have, they do not come, comparatively speaking, because we are stupid.

2. *Motivation.* Speaking of a group of distinguished natural and social scientists, Anne Roe says, "Perhaps the most influential of these other (than intelligence) aspects is motivation—the degree to which the individual's work is important to him. All of these men are, and always have been, so immersed in their work that

⁶⁵ Diana Crane, "Scientists at Major and Minor Universities."

⁶⁶ The data on mathematicians are cited in Logan Wilson, *The Academic Man*, p. 107; the data on political scientists are from V. O. Key, "The State of the Discipline," *American Political Science Review*, 52 (1958), 969. V. O. Key goes on to say "The meaning of the data . . . is that we allocate the most inadequate resources to the labor of inquiry essential to the development of the discipline. . . . From my examination of my sample, I would judge, by what seem to me to be latitudinarian standards, that not over 10 per cent had made . . . [significant] contributions." Perhaps this comparison was hard on mathematicians but Folger *et al.* find that their mean citation count is lower than that in other sciences, but higher than psychology (p. 261).

⁶⁷ Berelson, *Graduate Education*, pp. 54–55. The figure for political scientists is derived by selecting a random sample of 150 from those with biographical entries (not all political scientists, but most of the doctorates, I think) in the 1968 APSA *Biographical Directory* and counting listed publications. These figures are more reliable for locating nonpublishers than for "average number of publications" due to the shortening of bibliographical entries for older scholars with many entries. The ten-year limit, however, makes this shortening less likely.

⁶⁸ Average articles published per person for the first eight years after the doctorate (when articles are still reported in the *Directory*) is .36. In the eight years 1963–70 we produced 3277 doctorates. Multiplying, the result is 1180 articles, reduced to 1,000 because of the lower productivity rate (.28) of those from one to three years from the degree.

⁶⁹ Folger, Astin, and Bayer, *Human Resources and Higher Education*, pp. 259–60.

other considerations play much smaller roles."⁷⁰ Another study of a large group of scientists found that research motivation, as measured by the time a scientist took to get his degree, had an independent effect on later productivity.⁷¹ Students planning to go into political science at the graduate level are as interested in research as others, but unfortunately, fewer of the very best are so oriented.⁷² Further, asked about their primary work activity, the mature political scientists in the scientific manpower register were less inclined to say "research" than was the case in other disciplines.⁷³ Putting these various studies together, I find the pattern of high research productivity and low motivation puzzling and disturbing. It does not suggest much creativity and raises questions about the quality of the research produced under these circumstances.

3. *Research and quality of training.* All studies of research productivity, creativity, and reputation stress the importance of the quality of the training institution, where quality is measured either by the American Council on Education ratings or some similar earlier studies. There is an interaction, of course, between the quality of the training institution and the quality of the current work setting, but the relationship to productivity is stronger in the case of quality of training. In Diana Crane's careful study, which included the field of political science, the proportions of highly productive scientists under the four conditions possible with these two variables were:⁷⁴

Educated in a major university and working in a major university	48%
Educated in a major university and working in a minor university	41%
Educated in a minor university and working in a major university (a small group)	36%
Educated in a minor university and working in a minor university	12%

⁷⁰ Anne Roe, "Changes in Scientific Activity with Age," *Science*, 150 (1965), 313.

⁷¹ Folger *et al.*, p. 260; Bernard M. Meltzer, "The Productivity of Social Scientists," *American Journal of Sociology*, 55 (1949-50), p. 28.

⁷² Davis, *Great Aspirations*, p. 230. Of those in the top fifth of their classes the proportions in each discipline indicating research interests are: psychology 84%, anthropology and related fields 69%, sociology 56%, economics 55%, and political science 49%.

⁷³ The percentages of the members of the various disciplines in the 1968 *National Register* who report "research" as their primary work activity are: psychology 26%, sociology 22%, anthropology 21%, economics 21%, and political science 12%. In 1970 this political science percentage dropped to 10.5%.

⁷⁴ Diana Crane, "Scientists in Major and Minor Universities," p. 704.

An early study of the productivity of sociologists reveals the same pattern; the later major study by the Commission on Human Resources, working with a very large sample, confirms it.⁷⁵

Under these circumstances, how should one appraise the influence of the new graduate programs on research training and capacity? I have argued above that there are advantages for college teaching to be derived from the new doctorate programs if (1) they are additive to (and not substitutive for) the stronger and older programs, and (2) substitutive for the master's degree programs. But as training institutions and work settings for research, the contribution of these programs is different, since there is no substitution effect for the master's programs and there is reason to believe they are only marginally additive to the stronger programs in generating high quality research. I think the summary answer is that these new programs should be regarded as primarily teacher training; their doctorates and faculty may, of course, contribute to quality research, but historical experience does not offer much hope of rapid fulfillment of this aspiration.

4. *Research and quality of work setting.* The proportion of political scientists working in distinguished or strong departments has been predicted to decline from 34 per cent in 1961 to 29 per cent this year, to 28 per cent in 1976.⁷⁶ Yet interaction of these two principle conditions of creative research productivity may have an interesting effect. If Crane is right, then the products of the better universities going to the lesser places will, after a little time—because this syndrome produces delay more than despair—increase the productivity and research orientation of the faculties of the less distinguished universities.⁷⁷

⁷⁵ Folger *et al.*, pp. 262-65; B. Meltzer, "The Productivity of Social Scientists," p. 28.

⁷⁶ Heinz Eulau and James G. March, eds., *Political Science* (Englewood Cliffs: Prentice-Hall, 1969.) The increased number of programs rated "distinguished" and "strong" should not affect these predictions since the proportions of programs with these ratings has remained constant.

⁷⁷ D. Crane, "Scientists at Major and Minor Universities," p. 704. I have limited this discussion to universities, but something should be said about colleges as work settings. In Folger *et al.*, *Human Resources and Higher Education*, the ratio of distinguished work in the natural sciences (measured by citation counts) done in universities compared to colleges was 5.85 to 1.0; but in psychology, a hybrid natural-social science, it was only 3.25 to 1.0 (calculated from data on p. 261). In political science, because of its humanistic components, the ratio might be smaller. The matter needs further investigation, but certainly one thinks easily of distinguished political scientists at Swarthmore, Williams, Oberlin, Amherst, and so forth.

5. *Time and resources.* Both the single creative scholar, working with library resources and his own rich store of ideas, and the empirically oriented, data-using work team need as a prime condition of creative productivity the factor of *time*. In speaking of the environmental conditions of creativity, Taylor and Barron say that only one thing is known for certain: "the obvious need for ample time in which to work freely on problems of one's own choice."⁷⁸ Time can be "made" and it can be "bought"; if "made" it is squeezed by an act of will out of other duties, and the conflicts, harassments, and guilts that ensue are familiar to all of us. For these reasons bought time is better. The sources of funds to buy time, are, of course, the individual universities and colleges, the private foundations, and the national government, especially, in our case, the National Science Foundation. Consider, then, the cumulative implications of the following:

Almost half of the political science departments in graduate schools have normal teaching loads for junior faculty of more than 10 hours a week; about 70 per cent of the graduate schools have virtually no way in which they can reduce these loads to help a man get over a hump in his research schedule.

Seventy per cent of the political science departments in four year colleges have teaching loads of ten or more hours a week; 84 per cent of them have no way of reducing these loads for research purposes.⁷⁹

Apparently, then, aside from sabbaticals, which are by no means universal, there is little help from the universities and colleges.

As reported above, fewer political scientists (29 per cent) get help from government than any other natural or social science, except linguistics.⁸⁰

Postdoctoral fellowships and traineeships are specifically and consistently correlated with high research productivity. Thus it is disturbing to find that while about 15 per cent of all new doctorates in all fields taken together receive some kind of postdoctoral study grants or fellowships, only 5 per cent of the political scientists do (although this is more than the econo-

mists or historians receive).⁸¹

In political science, bought time is hard to come by.

D. Disciplinary Policy and Research. If we think of research findings and publications as a disciplinary "joint-product," measures to improve the educational, communications, and manpower infrastructure may find more support.

(1) Given what we know about the importance of training in a major university as a condition of future productivity, as stated above the disciplinary policy should be to devise ways of filling training capacity from the top down, with no reduction in admissions by these quality universities, continued and improved free-choice fellowships by the Federal government (now threatening to close down),⁸² and informed advice by all counsellors of college students, taking into account the above information.⁸³

(2) Providing that there are enough first rate universities to ensure heterogeneity (and there are), the policy of collective inbreeding—but not necessarily inbreeding by each university—should be encouraged. This traditional hiring policy has been somewhat eroded with the growth of the number of quality institutions, but it is still strong: from 1958 to 1965 the proportion of the faculty at the "top twelve" with degrees from any of the top twelve seems to have declined.⁸⁴

(3) The 22 universities in the ACE ranks of "good" and "adequate plus" should avoid collective inbreeding; rather they should recruit from the now plentiful supply of doctorates from the stronger universities. Again this is a traditional policy; in 1958 about 57 per cent of their faculties came from these sources.⁸⁵ In this way the current rate of improvement from "good" to "strong" of .6 universities upgraded per year may continue.

⁸¹ National Research Council, *Summary Report 1970*, Table 2.

⁸² See M. Crawford Young and Robert M. Bock, "Hard Times for the Graduate Schools," *Change Magazine*, 3 (1971), pp. 20-22.

⁸³ For accounts of the nonprofessional and uninformed criteria employed by candidates for graduate school, see T. Caplow and R. J. McGee, *The Academic Marketplace*, p. 225; B. Berelson, *Graduate Education*, p. 143.

⁸⁴ Berelson, p. 115; D. Crane, "The Academic Marketplace Revisited: A Study of Faculty Mobility Using the Carter Ratings," *American Journal of Sociology*, 75 (1970), 953-64. If the rate of erosion were to continue at one per cent per year, the 1971 figure would be 72 per cent.

⁸⁵ Berelson, p. 115.

⁷⁸ C. W. Taylor and F. Barron, *Scientific Creativity*, p. 373.

⁷⁹ Calculated from data in *The Political Science Job Market: Arizona Survey of Hiring Practices and Employment Conditions* (Tucson: Institute of Government Research, University of Arizona, 1970), pp. 57, 59. The survey on which the data are based received useable answers from 163 graduate schools and 461 four year colleges.

⁸⁰ 1968 *American Science Manpower*, p. 42.

(4) The evidence that the doctorates of minor universities working in major universities have research records almost as good as any, implies that a constant open search for talent at all levels by the top universities is in their interest as well as in the public interest.

(5) A recent study of the invisible colleges shows that their members (working jointly and severally on a common set of problems) need both the stimulation of ideas from outside their special fields and the assured cumulation of research findings in an identifiable place.⁸⁶ In political science there is too much scatteration of articles in too many general journals. Perhaps this is one reason why political scientists are less concerned with continuity, and therefore the cumulative impact of their work, than psychologists or biologists.⁸⁷ It may also help explain the high rate of information loss.

(6) Information search instruments should be strengthened. *The International Political Science Abstracts* contains only about 2,000 abstracts per year; the *Sociological Abstracts* contains about 6,000 and the *Psychological Abstracts* about 20,000. Furthermore, journals need more frequent cumulative indexes.

(7) Among the 7,000 political science Ph.D.'s produced in the last 35 years there are more people with research skills than with good research ideas. The study of the invisible colleges mentioned above suggests that these groups are held together more by their members' relations to a few creative scholars than by lateral reciprocal connections. If, as I would argue, the posing of questions and the agenda for research needs more attention than its execution, those who have a special talent for identifying new research areas and fruitful ways of entering them should have their influence magnified in some fashion. It is a central problem for knowledge nurturance.

The terrain covered in this address has been extensive and rocky; there are no prophets on any mountain tops here. Rather, I think, we shall all have to grub amongst the data a little more.

Appendix on Master's Programs*

The most neglected aspect of graduate education is the training of candidates for the master's degrees, a neglect that is especially critical

⁸⁶ Diana Crane, "Social Structure in a Group of Scientists: A Test of the 'Invisible College' Hypothesis," *American Sociological Review*, 34 (1969), 335-52.

⁸⁷ D. Crane, "Scientists at Major and Minor Universities," p. 707.

* The data for this Appendix are derived from the same sources as those cited for the doctorates; see especially footnote 7.

because of the size, proliferation, and diversity of the programs. The resources devoted to master's programs are larger than those devoted to doctorates; more faculty members are involved in teaching terminal master's candidates; up until very recently more college students were taught by instructors with master's degrees than by those with doctorates; many high school students are affected by the content and quality of master's programs from which their teachers received their degrees; more master's degrees recipients than Ph.D.'s work for federal, state, and local government; a master's degree in political science qualifies a person for the National Register of Scientific manpower and 38 per cent of political scientists in the Register (1970) have the master's as their highest degree.

Programs. In political science, there are about 100 master's programs in institutions that offer the doctorate and another 100 programs in institutions that do not. The first group is likely to be in much closer touch with the main currents of the discipline and, generally speaking, is of much higher quality. (The master's degree in international relations is likely to be given by a department offering a political science doctorate, hence tends to be of higher quality; generally it represents an interdisciplinary specialization.)

The recently established master's programs in political science tend to be a first (and sometimes final, and sometimes reversible) step into graduate education by departments in undergraduate colleges. During the three year period 1967-69 at least 30 new programs of this kind were established, or about 10 a year. Many of these programs had small enrollments (eight of them had only one student enrolled each), and the completion rate is low (89 enrollees and 11 degrees conferred). Unless a special case can be established (the advantages of a regionalized or of a specialized program), new master's programs in nondoctorate departments should be discouraged.

Students. There are more students in political science master's programs than in similar programs for any other social science except psychology (a much larger discipline); more of them are part time and apparently more of them drop out. On the other hand, those students who enter these programs immediately or soon after leaving high school are more able than those entering master's programs of any other social science. Again we get more than our share of the best, but compared to those going into doctoral programs, master's candidates (that is, those students planning on a ter-

minal master's degree) are markedly poorer (on GRE scores).

Careers. Since more than 90 per cent of the master's degree recipients do not join the APSA, and only a small fraction are recorded in the National Register (about 2,500—less than 10 per cent of the total) we know little about their careers. Of those in the Register, about two-thirds are in educational institutions, but a higher proportion of master's degree recipients than of doctorates work for government. Some years ago, a study showed that about half of the candidates for the master's degree planned to work for the Federal government, while only a quarter planned on college teaching; they may have escaped the Register questionnaire, or their intentions may have changed.

Policy. Eleven years ago Berelson (p. 228) said:

The Master's degree has lost status in the arts and sciences; has become largely a professional degree, especially in education; is no longer under the control of the top institutions; and cannot be recovered as the acceptable degree for college teachers.

Since that time there have been 21,289 master's degrees awarded in international relations and political science, many of whose recipients are teaching in four and two year colleges. Part of the reason for this has been the shortage of doctorates, but that shortage has now changed. Further, enrollment in master's programs has

levelled off, while doctorates have continued to increase, with the result that the proportion of doctorates to masters has been increasing slowly but consistently: in 1966 it was about 1 to 6 while four years later it was 1 to 4.5. We are now in a position to try to enforce Berelson's dictum that the master's degree is not "the acceptable degree for college teachers."

What, then, are master's programs good for? First, they serve to upgrade high school instruction and insofar as they are teaching degrees they should be so designed. Second, they permit special interdisciplinary teaching programs for international relations, public administration, urban studies, and so forth. These degrees are then certificates of specialized study, partly for practice, and partly for teaching in specialized programs, perhaps on the way to or in addition to a doctorate in a basic discipline. Third, they are excellent vehicles for teaching practitioners (professionals), upgrading skills, and, incidentally, are the basis for salary increases. Fourth, as reported above, without some special strength (as in the case of Swarthmore, for example), and special local reasons (as in the case of an urban college teaching urban politics), departments not offering the doctorate should not offer the master's degree in political science. Finally, in order to keep recipients of master's degrees in touch with the discipline, it might be useful to establish a special class of Association membership at reduced rates whose members would receive a book review journal and *PS*.

COMMUNICATIONS

TO THE EDITOR:

In a recent letter to the editor (*American Political Science Review* [September, 1971] p. 779), Lawrence Mayer asserts that my argument concerning the Casstevens result (*American Political Science Review* [March, 1971] pp. 188–189) “contains a mathematical error,” and attempts to demonstrate this by trying to show my definition of a certain conditional probability to be inequivalent to an alternative definition. However, the error is Mayer’s, not mine, and lies in the fact that he defines the quantity n_i as “the number of votes received by candidate C_i ” (p. 779, left hand column lines –18, –17) and then implicitly equates this to the quantity denoted by the same symbol in my letter. In fact, I explicitly defined n_i as “the number of votes received by candidate i from all voters *except the one in question*” (p. 188, left hand column, lines –3, –2; italics added). In Mayer’s example the first voter is the one in question, so the n_i should refer only to the votes cast by voters R_2 and R_3 . Mr. Mayer informs us that he is a mathematician, so presumably he will be able to verify for himself the fact that when the proper definition of n_i is used, his “simple counting argument” shows the two definitions of the conditional probability $P(W_1/V_1)$ to yield identical results, as of course they must.

Casstevens himself, in a private communication, has criticized my argument on other grounds. He objects to my use of the action V_0 , citing the passage in his note which reads “The alternative courses of action available to the voter are to vote for C_1 , or to vote for C_2 , . . . , or to vote for C_n . (The problem is how the voter should vote rather than whether or not he should vote.)” (*American Political Science Review* [March, 1968], p. 205.) The difficulty lies in the fact that Casstevens was not explicit about definitions or assumptions in his note; thus it is not clear, for example, whether one should conclude from the quoted passage that he simply failed to take account of the possibility of nonvoting (irrespective of purpose of the analysis), or rather that he explicitly assumed that action not to exist. The more fundamental question concerns what constitutes a fair reading of the even less explicit statements about “ignoring ties”—apparently primarily on the grounds that they are improbable occurrences (footnote 4, page 205)—as they apply to such potential tie-producing events as $n_1 = n_2 > n_3$, $n_1 = n_2 +$

$1 = n_3$, $n_1 = n_2 = n_3$, etc. I personally think it not unfair to interpret Casstevens’s statements about ties to mean that he meant to assign zero probability to such events.

Probably Casstevens himself would not agree, and possibly he might even maintain that had he been asked some three years ago to clarify his assumptions on ties, he would have responded that what he really had in mind was to require the last event to have strictly positive probability, while simultaneously requiring events of the first two kinds to have zero probability (this being the only way the assumptions can be made consistent). I have some difficulty accepting this myself, but for purposes of argument suppose we grant the point and consider just what his result does then say. Many have long believed that under the plurality system of election, it can sometimes be counterproductive to vote for one’s most-preferred candidate. No one, so far as I know, has claimed it is *always* counterproductive to vote in this fashion: for example, a voter whose own candidate has the best chance of winning will not find it counterproductive to vote “sincerely,” nor will one who believes all candidates have an equal chance of winning. Casstevens’ result, properly interpreted, in effect amounts to showing that there is still another species of voter who does not find it counterproductive to vote sincerely. It is a bit difficult to characterize this type of voter explicitly, but he is one who believes, among other things, that there is absolutely no chance whatever of any kind of tie if he does himself vote, while simultaneously believing that should he not vote, there would be a strictly positive probability of an exact three-way tie between all candidates (in the three-candidate case). Perhaps this fact is a useful result, or perhaps it is just a curiosity; but surely, it does not constitute a proof that a rational voter always votes for the candidate he most prefers.

GERALD H. KRAMER

Yale University

TO THE EDITOR:

I was much interested by the conclusions reached by David Nexon in his article “Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties, 1956–1964,” *American Political Science Review* 65 (Sept., 1971) 716–730. I think that it is fairly obvious that the conclusion that “the Republican Party organization is essentially an ideological one relying on ideo-

logical incentives to activism, while the Democratic Party Organization is a nonideological one relying on nonideological incentives to activism" (page 721) is not warranted by the facts that Nexon presents. To establish this would require data on incentives and motives for participation which Nexon does not have. He appears to have been engaging in some wild speculation. However, his thoughts are intriguing and might be researched further.

More relevant to the data examined is the conclusion that: "On issues that polarized the mass membership of the two parties, Democratic activists had about the same opinion distribution as rank and file Democrats, while Republican activists were substantially more conservative than their rank and file membership" (page 720). This is, as Nexon notes, the same conclusion reached by McClosky *et. al.* back in 1960. McClosky found, however, that Democratic leaders too were significantly different from their rank and file. McClosky's Democrats were closer in some assumed spatial model to both their own voters and the Republican voters than were the Republican leaders. In any case, both articles purport to show that Republican gladiators are the "odd men out" ideologically.

Distances on spatial models of ideology cannot be legitimately determined from the kinds of questions asked by McClosky and Nexon. The averaging of individual scores used by McClosky and the mean scores used by Nexon do not permit the differentiation between the unity of opinions within a group on the one hand, and the content of those opinions on the other. The critical factor here is not the mean or the average, but the distribution. The group of Democratic workers may not be a unified group of moderate men, but rather a group composed of a large gang of "extreme conservatives" and another large bloc of "extreme liberals." Such a group would have a mean score which would be "moderate" (i.e., in the middle), but the conclusion that it is similar to some other group which has a middle-range score may or may not be valid.

Much the same thing applies to the Republican groups. The fact that their scores deviate from the scores of the other groups may not be because their solutions to public problems are very dissimilar to those of the other groups. It may just be that the Republicans are a fairly unified group of individuals who (to use Converse's terms) share a common belief system which has a great deal of constraint. The Democrats and the groups of voters, on the other hand, are much more divided on issues.

This conclusion is particularly likely given Nexon's observation that: "... a disproportionate number of Democratic party identifiers have, at least since 1860, come from the South" (page 722).

Issues and opinions are very complex matters. To really get at these questions requires more in-depth questioning than used in the interviews utilized by either McClosky or Nexon. And judgments on whether a particular proposed government policy is relatively close or far away from another proposal are rather difficult to make. It does not suffice for Nexon to use questions about "how strongly" a person agrees or disagrees with a certain statement or policy. A *strong* disagreement does not necessarily mean that the respondent would ideally prefer a policy which drastically differs from that given in the statement, just that he is very much committed to *something* different.

In making these changes in Nexon's conclusions, it may be that I have justified his assertion that the Republicans are more "ideological" than the Democrats. For what Nexon and McClosky have actually shown is that Republicans share a common belief system, and Democrats do not. In a certain sense this means that Republicans are ideological and Democrats are not. It does not, however, show that the members of either party are motivated by ideology, nor does it show that either party's policies are more or less like those of the voting public. The Democrats do resemble the voting public in that both groups are internally divided on public issues.

THEODORE S. ARRINGTON

University of Arizona

TO THE EDITOR:

Professor Arrington criticizes the conclusions reached in my article on a number of grounds. First, he argues that my statement regarding the differing basis of organization in the two parties is not justified by the data presented. It is true that direct data on the "incentives and motives for participation" of activists would have been more conclusive evidence for my finding. Since such data were not available, I relied on the data I had. It is possible to classify the process by which I progressed from the available data to the conclusion noted above as "engaging in some wild speculation." Understandably, I prefer to regard the process as reasonable inference. The logical basis for that inference is discussed at some length in the article itself and reiterated briefly in the last paragraph below.

I find Arrington's second point rather con-

fusingly stated. At one point he seems to be arguing that it is invalid to say that Republican activists are more conservative than other groups because questions of the type used by the Michigan Survey Research Center do not capture opinions and issues in their full richness and complexity. At another point, he seems to be arguing the more restricted point that one cannot say that two distributions of opinion are alike (or different) simply from comparison of mean scores.

The latter point certainly has some validity in a mathematical sense, at least insofar as two similar means are involved. A symmetrical bimodal distribution can certainly have the same mean as a unimodal distribution. Parenthetically, I might note that the "true" shape of a curve is rather moot in any opinion data, given the absence of interval scales. I did compare the shape of the curves of the groups discussed in my article by dividing the sample into quartiles of liberalism on each of the opinion scales and then looking at what proportion of each group fell into each quartile. This procedure produced the same result as looking only at means. The tables of this type were not presented in my article, because, given the number of groups simultaneously compared, I thought the mean was a more readily comprehensible statistic and because the number of activists, particularly among the Democrats, was so small that dividing the cases among four cells was of doubtful statistical validity.

Arrington's belief that the kinds of questions used by the Survey Research Center do not "really get at" opinion is a valid one for certain kinds of theoretical interests. One could hardly hope to develop the findings presented in Robert Lane's *Political Ideology* from reliance on the SRC's national sample alone. On the other hand, the questions used in my scale are fully adequate for indicating, for example, that Republican activists as a group are more likely to favor the nomination of a candidate like Barry Goldwater than are ordinary Republicans, ordinary Democrats, or Democratic activists. Conservatism in this sense has both a theoretical and a practical interest for most political scientists.

Finally, Arrington discusses possible meanings of ideology that may be legitimately regarded as applicable to the Republican activists in my article. The meaning of ideology that I regarded as most important was drawn from the distinction between amateur and professional made by James Q. Wilson and noted in my article. The ideologue in this sense is the person who participates in politics because of his opinions about public policy and not for

some other motive. I believed that Republican activists could legitimately be characterized as ideologues in this sense because they were quite distinct, in opinion terms, from other Republicans. This difference between activists and ordinary members must have some causal roots. Controlling for demographic variables did not, with the ambiguities noted in the article, eliminate the difference. I suggested that a plausible and reasonable causal explanation of the difference might be that the Republican Party, unlike the Democratic Party, relies on ideological incentives to activism and thus has a tendency to draw the most conservative of its identifiers into activism. In default of a more plausible hypothesis or proof to the contrary, I hold to that explanation.

DAVID NEXON

Pitzer College

TO THE EDITOR:

In the *American Political Science Review* (September, 1970, p. 979), you published a review of my book, *Ghana's Foreign Policy, 1957-1966: Diplomacy, Ideology and the New State*, by Kofi Ankomah. In a subsequent issue, Professor William Zartman criticized Ankomah's review of my book on a number of grounds. With the same sort of innuendo and *ad hominem* arguments used in his original review, Mr. Ankomah has now cast aspersions on Professor Zartman, by noting that I thanked him in the preface of my book for his helpful suggestions on the manuscript.

Professor Zartman undertook the reading of my manuscript for Princeton University Press in the autumn of 1967. I had not met him at that time. I subsequently came to know him through the intermediary of Princeton University Press. His comments were no less helpful for the fact that they were done for the Press. This does mean, however, that Ankomah owes Professor Zartman an apology.

W. SCOTT THOMPSON

*Associate Professor of International Politics
Fletcher School of Law and Diplomacy*

TO THE EDITOR:

We write to correct three errors of interpretation which mar Kenneth Shepsle's otherwise balanced review of our proposals for *The Analysis of Political Cleavages* (this *Review*, September, 1971). We have no objection to his quite appropriate overall judgement—a skeptical admiration informed by a sense of the problem's enormity. But on three main points of substance, he has misread or misunderstood the

book, leaving himself and his reader with a mistaken impression. The rubs are these:

1. *Pairwise analysis of cleavage relations:* Shepsle says we make a "... unique and imaginative (if not altogether justified) *sociological assumption*, namely that the relevant theoretical focus is not on individuals *per se* but on *pairs* of individuals." We have italicized "*sociological assumption*" to draw attention to its inaccuracy. We make no such claim, as the reasoning behind the measure being discussed makes quite clear. Shepsle quotes us on the point by substituting his word "is" for our words "may be" and by omitting the beginning of the pertinent sentence, "For these reasons . . ." This bespeaks his neglect of the reasons themselves, which are given in the passage just before he quotes us. A look at these suggests that we have made no sociological assumption—no assertion that politics, like "love and tennis," is "played in pairs." We have just, in fact, been discussing a committee of *ten* members, and have followed that example with the suggestion that relations of sameness and difference in such groups can usefully be exfoliated from the examination of pairwise comparisons (pp. 26–27). In the passage omitted, we suggest that the pair is a useful *analytic* unit on two grounds: 1. it is the smallest unit for which similarity and difference (say, agreement and disagreement) have any meaning, and 2. these relations (similarity and difference) in larger groupings can be analytically reduced to the outcomes of these pairwise comparisons. The review argues against an approach we simply haven't considered.

2. *Definition of groups:* The analysis we propose assumes that one can figure out what cleavages—which divisions of a community into nominal groups—matter. The book illustrates a serious problem with a rather fatuous example—the silliness of the supposition that India is religiously homogeneous because the classification Christian-Jew-other indicates little fragmentation (p. 34). Admitting that the authors are "aware" of such a possibility, Shepsle transposes the example to Ulster and gives it as his own. He then suggests that we offer "no theoretical guidance" on the matter. He has here omitted two considerable passages of the book, one outlining the underlying assumption about nominal groups (pp. 14–17), and the other offering two specific strategies for dealing with the central problem (pp. 34–35). Here, Shepsle notwithstanding, we *do* depend on a brittle sociological assumption, namely, that well-defined nominal groups are there to be found. This is the point we make and it is a

point which makes us cautious about any indiscriminate application of the analysis.

3. *Cross-cutting and fragmentation:* Shepsle credits us with the "carefully derived" result that "... the amount of cross-cutting between two cleavages (and hence the likelihood of political stability) is high if fragmentation is high on one of the cleavages and low on the other." This is a two-fold misreading: 1. we claim to have demonstrated nothing about political stability, and 2. far from being a carefully derived result, the claim about cross-cutting and fragmentation is denied by the book itself. We do say that the combination of low and high fragmentation is a necessary condition to high cross-cutting, but deny that it is sufficient. We go on to show that the functional relationship between these variables upholds this denial (pp. 98–99.) Since his proposed counter-example assumes a necessary and sufficient relationship, we can only guess that he failed to read the whole of the (difficult, perhaps turgid) chapter in question. It is, however, only by means of *both* misapprehensions that Shepsle is able to have us predict no conflict over language in Belgium. The claim is, if anyone's, Shepsle's.

The review's fourth main point, on our failure to resolve the intensity problem, is quite right as the book's preface suggests. And, as we indicated at the beginning, the overall judgement expressed by the review is unobjectionable.

DOUGLAS W. RAE
MICHAEL TAYLOR

TO THE EDITOR:

The book review editor has invited me to respond to the Rae-Taylor critique of my review of their book and has requested brevity. Therefore, in response I should like to make two points. First, regarding the second and third "rubs" Rae and Taylor outline above, the thrust of my review was to underscore a serious omission in their monograph. In the absence of any indication of the *salience* of cleavages, their assumption that "well-defined nominal groups are there to be found" is indeed "brittle." In order to analyze political cleavages one must be able to identify the subset of partitions of the populations into nominal groups, i.e., the subset of cleavages, that is *politically relevant*. The assumption that such cleavages can be identified—Rae and Taylor's assumption—may be a reasonable research strategy, but it does beg an otherwise interesting and important question, namely, why do some cleavages manifest themselves in political conflict whereas others do not? On this latter question Messrs. Rae and

Taylor may be referred to the work of the late E. E. Schattschneider.

The cross-cutting cleavages hypothesis, of course, is subject to precisely this kind of criticism. Thus, the Rae-Taylor analysis of it suffers in the absence of a measure of cleavage salience. My own example of Belgium was employed to underscore this omission.

The first "rub" to which Rae and Taylor refer is mostly a matter of semantics. If they are embarrassed by my attribution of sociological properties to their assumption, and wish to call it something else, that is their prerogative. In any event I am quite willing to accept their pairwise treatment of cleavage relations as an *analytic* device. However, if one wishes to

judge the utility of that device he must look to its consequences; and it is this operation that is frustrated by errors of omission and commission that crop up later in Rae and Taylor's analysis (see point 4 in their letter above, as well as my discussion of salience, for examples of this).

To conclude, my review of Rae and Taylor's monograph had one summary theme—a theme applicable perhaps to many analyses of complex topics. That theme, I feel, still applies: *ca-veat emptor*. To this, at least, they seem to have no objection.

KENNETH A. SHEPSLE

Washington University

EDITORIAL COMMENT

Hello again. There are two hypotheses capable of explaining the absence from the *Review* this last half-decade of articles by Austin Ranney. The first hypothesis is that the tasks of managing the *Review* were so onerous as to create a pause in the productivity of one of the most active and interesting scholars in our profession. The appearance in recent years of an occasional article by Ranney in one or another of the regional journals does not wholly undermine this hypothesis, but weakens it sufficiently to suggest another, namely that Ranney thought it unseemly for the manager of a complex process of anonymous evaluations to give his own work what could be interpreted as a natural advantage.

It is therefore likely that the only benefit readers of the *Review* shall realize from the retirement of Austin Ranney as managing editor is his welcome reappearance in these pages as an author in the current and, one hopes, in many a future issue.

Two hypotheses are now available to explain the mysterious absence from the pages of the *Review* of articles by the current managing editor.

APSR and PS. A few years ago, the Association established a newsletter, *PS*. Part of the job of this new journal was to relieve the *Review* of some of the heavy burden that it used to carry as the sole medium of general communication among Association members. The *Review* has continued to provide articles and reviews of scholarly and scientific interest, while *PS*, especially under the innovating leadership of Professor James David Barber, has carved out another sort of niche for itself, providing information about teaching and the profession, notices of career changes and opportunities, official documents of the Association, and other such material.

As time has gone on, the activities of the profession itself have come in for occasional scholarly investigation. Since the *Review* is devoted to scholarship and *PS* to the profession, which journal should receive an article that seems to fall neatly between them? We have, in general, resolved the issue in favor of *PS*. Both journals have the same audience, of course, since both go to all members of the Association as a perquisite of membership. We take for our province contributions that are in some sense "about" politics, and refer to our sister journal articles that are "about" political science as a profession.

The one great anomaly in this orderly scheme of things is presented by the problem of the annual address of the outgoing APSA President. There is something to be said for preserving continuity in making the Presidential address available to readers in the same place, year after year. In the past, this place has been the lead article in the December or March issue of the *Review*. Yet carrying on the tradition creates its own asymmetries: the Presidential address is the only unrefereed article the *Review* prints. Some comments, rejoinders, communications, and book reviews are solicited, and most are normally evaluated only by the editor or book review editor. Hence these contributions are scrutinized less elaborately and less anonymously than articles. The fact still remains that the Presidential address alone out of the entire yearly output of the *Review* passes through no editorial screen at all. Moreover, no Presidential address has been stopped at the gate since Frederic Ogg refused to print President Benjamin Shambaugh's address in 1930, and none is likely to be again, not even by the most nostalgic managing editor. Some Presidential addresses however, perhaps most, violate a basic rule for inclusion in the *Review*; they are not "about" politics, but "about" political science, and by our normal criteria, now that *PS* is flourishing, such essays should logically appear in *PS*. If they do not, does this not gratuitously appropriate to the *Review* a little extra cachet and imply that *PS* is somehow number two? Would not the Association and the members be better off if, instead of living with this implied rank-order, each journal cultivated its own garden on a more strictly equal basis? If *PS* had existed in 1958, "The State of the Discipline," by V. O. Key, Jr., would presumably have been comfortably at home in its pages, whereas Robert A. Dahl's 1967 address, "The City in the Future of Democracy," to take a contrary example, would have been more suited to the *Review*. What about cases occupying something nearer middle ground, like Harold Lasswell's "The Political Science of Science" (1956) or E. E. Schattschneider's "Intensity, Visibility, Direction and Scope" (1957)? Logicians, hair-splitters, proponents of one solution or another, or one or another method of finding a solution (e.g., random assignment, Indian wrestling between the editors of *PS* and the *APSR*, a mail ballot of the Association, President's choice) could have had a field day with the problem. Heaven knows, political science provides little enough recreation

as it is, and so one almost regrets the past absence of the opportunity for this sort of exercise.

In future we may be more fortunate. The most likely outcome, however, in the absence of a strong Presidential initiative is that inertia, not logic, will prevail, and the *Review* will continue to steal this small advantage from *PS*, and print Presidential addresses, no matter how preoccupied with the profession they are. As a sop to our troubled consciences we will probably channel responses and reactions to Presidential addresses that "really" belong in *PS* into the *PS* editorial process, thus providing an inelegant but one hopes an intelligible solution.

Relevance. The *Review* is always in the market for articles that meet some standard of contemporary relevance. Although no reader who contemplates our year-long backlog will doubt that we publish first and foremost for the ages, there is a set of possible contributions—not wholly hypothetical—that speaks to contemporaries as well as to the next generation of graduate students, and still maintains a level of empirical demonstration or theoretical interest that makes it eligible for inclusion in the *Review*. Consider four such articles in the current issue, all on currently "hot" topics. One explores the forces at work in Britain surrounding its recently negotiated entry into the Common Market. One analyzes Congress and the Defense Budget. Two are on various aspects of Presidential elections: Austin Ranney casts a doubt upon the political justification underlying the drive for more and more Presidential primary elections, and Robert Axelrod marshals data that can aid in the task of determining how much credence to put in perennial claims about the reformulation of the major parties' electoral coalitions.

Obviously, this does not exhaust the ways in which political scientists can be "relevant," but it is a fair sample. We hope to be able to offer other examples as time goes on.

Articles Accepted for Future Publication

Paul Abramson, Michigan State University, "Intergenerational Social Mobility and Partisan Choice"

Richard W. Boyd, Wesleyan University, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election"

David W. Brady, Kansas State University, "A Research Note on the Impact of Inter-Party Competition on Congressional Voting in a Competitive Era"

Charles S. Bullock, III, University of Georgia,

"Freshman Committee Assignments and Re-election in the U.S. House of Representatives" and "House Careerists: Changing Patterns of Longevity and Attrition"

Charles E. Butterworth, University of Maryland, "Averroës: Politics and Opinion"

William D. Coplin and J. Martin Rochester, Syracuse University, "The Permanent Court of International Justice, the International Court of Justice, the League of Nations, and the United Nations: A Comparative Empirical Survey"

Robert S. Erikson, Florida State University, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections"

John Ferejohn, University of Illinois, "A Note on 'Voting or a Price System in a Competitive Market Structure' "

Alexander L. George, Stanford University, "The Case for Multiple Advocacy in Making Foreign Policy"

Wallace Mendelson, University of Texas, "From Warren to Burger: The Rise and Decline of Substantive Equal Protection"

Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"

Andre Modigliani, Harvard University, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy"

Sarah McCally Morehouse, Manhattanville College, "The State Political Party and the Policy Making Process"

Donald G. Morrison and H. Michael Stevenson, York University, "Integration and Instability: Patterns of African Political Development"

Edward N. Muller, State University of New York, Stony Brook, "A Test of a Partial Theory of Potential for Political Violence"

Willard A. Mullins, Carleton University, "On the Concept of Ideology in Political Science"

John M. Orbell, University of Oregon and Toru Uno, San Francisco State College, "A Theory of Neighborhood Problem Solving: Political Action vs. Resident Mobility"

Benjamin I. Page, Dartmouth College and Richard A. Brody, Stanford University, "Policy Voting and the Electoral Process: The Vietnam War Issue"

Gerald M. Pomper, Rutgers University, "From Confusion to Clarity: Issues and American Voters, 1956-1968"

Jeffrey L. Pressman, Dartmouth College, "Preconditions of Mayoral Leadership"

George F. Quester, Cornell University, "Some Conceptual Problems on Nuclear Proliferation"

Bruce W. Robeck, Texas A&M University, "Leg-

- islative Partisanship, Constituency and Malapportionment"
- Thomas W. Robinson, The Rand Corporation, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"
- John Gerard Ruggie, Dotation Carnegie Pour La Paix Internationale, "Collective Goods and International Organization"
- David Sankoff, University of Montreal and Koula Mellos, University of Ottawa, "The Swing Ratio and Game Theory"
- Kenneth A. Shepsle, Washington University, "The Strategy of Ambiguity: Uncertainty and Electoral Competition"
- W. Phillips Shively, University of Minnesota, "Voting Stability and the Nature of Party Attachments in the Weimar Republic"
- Lee Sigelman, Vanderbilt University, "Do Modern Bureaucracies Dominate Underdeveloped Politics? A Test of the Imbalance Thesis"
- Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich and Craig E. Schopmeyer, Ohio State University, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models"
- John L. Sullivan, Iowa State University and Robert E. O'Connor, Pennsylvania State University, "Electoral Choice and Popular Control of Public Policy: The Case of the 1966 House Elections"
- John L. Sullivan, Iowa State University, "A Note on Redistributive Politics"
- Herbert F. Weisberg, University of Michigan, "Scaling Models for Legislative Roll-Call Analysis"
- Peter Willetts, Makerere University College, "Cluster-Bloc Analysis and Statistical Inference"

BOOK REVIEWS

Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430–1450.

By A. J. Black. (Cambridge: Cambridge University Press, 1970. Pp. 189. \$10.50.)

In the 1950's Brian Tierney traced the canonist origins of conciliar thought, and Alan Gewirth analyzed and translated Marsilius of Padua. In the 1960's studies of the political thought of Gerson, D'Ailly, and Nicholas of Cusa by John Morrall, Francis Oakley, and this reviewer traced the further development of conciliar thinking. This volume moves the study of conciliarism one step further by looking at the thinking of the late conciliarists—principally but not only John of Segovia, and the anticonciliar papalists, mainly John Turrecremata, in the period from the split between Pope Eugene IV and the Council of Basel, to the papacy's final victory over its conciliar rivals. It also relates the efforts of the two sides to win the support of the European monarchs to "the birth of ideology . . . the peculiarly modern habit of justifying political acts by reference to abstract, metaphysical ideals" (p. 130) and argues that conciliarism "mediated between the medieval and modern conceptions of popular government" (p. 134).

In making the latter point, the author is at pains to distinguish the theories of the Council of Basel (1431–1449) from those of its better-known predecessor, the Council of Constance (1414–1418). Constance is seen as principally an emergency judicial response to the problem posed by three claimants to the papal throne, while the theorists of Basel are interpreted as asserting a general legislative supremacy of the representatives of the Christian church over their "minister," the Pope. Omitting most of the theological issues, Black sees the Basel conciliarists as carrying earlier corporation theory to its logical conclusion, when they asserted conciliar control of the papal financial and legislative system and finally in 1439 attempted to depose the Pope.

The argument is supported by extensive quotations from the sources and 32 pages of Latin excerpts from the writings of Segovia and Turrecremata. With its help we are able to fill in a further chapter in the development of constitutionalism in the West. The author's claims for the importance of the Basel theories in that development, however, raise some questions for this reviewer:

(1) Was the difference between the theories (as distinct from the practice) of Constance

and Basel that great? Constance after all did assert in its decree, *Haec Sancta* (not, as Black and many other writers call it, *Sacrosancta*) that a "general council representing the Catholic church, has its power immediately from Christ and all men, including the Pope himself, are bound to obey it in matters pertaining to the faith, the extirpation of schism, and reform of the church in head and members." In addition, in the decree, *Frequens*, it called for regular convocation of general councils. (The Basel Council was called in response to the *Frequens* decree.) At least in the areas mentioned, Constance seemed to assert a general legislative supremacy for the council, and it has been so understood by a number of recent commentators in the Roman Catholic Church including Hans Küng and Francis Oakley.

(2) Black frequently refers to the democratic implications of Basel and its contributions to the development of popular sovereignty. Yet most of the participants and theorists at both Constance and Basel considered the councils as meetings of bishops and cardinals with an assigned role in the constitution of the church, which limited the Pope in certain areas but not in others. The main impulse of conciliarism was thus constitutionalist but not democratic. No one, with the exception of the discredited but influential fourteenth century theorist Marsilius of Padua (whom Black unaccountably persists in calling "Marsiglio"), called for representation of the laity. Only insofar as the two councils attempted to assert a general community control over the ruler could it be said that their theories implied some lay role.

It is true that the papalist opponents of the Basel rump-council accused it of revolutionary democratic tendencies. They were able to do so because it allowed priests and doctors of theology and canon law, (but apparently not laymen) to vote along with the few bishops who were left in the Council after 1437. That the papalists could attack this procedure with such telling effect, however, indicates how far the assumptions of the period were from anything that one could call democracy or popular sovereignty.

There is no denying however the influence of the *example* of Constance, and to a considerably lesser extent, of Basel, on constitutional controversies in sixteenth-century France and seventeenth-century England. The conciliarists' theories were less influential, although Francis

Oakley has traced a direct line of influence in "On the Road from Constance to 1688" (*The Journal of British of British Studies*, I [1962], 1-32). Even less influential were the theories of the papalists on the ideologies of the rising national monarchies. Black asserts a connection between theories of papal sovereignty and later similar monarchical theories, but his assertion is flawed by the admission that *all* papalist writers accepted the possibility outlined in canon law of papal deposition for heresy—and possibly for other reasons. More probable is the common need of both the papalist and monarchist writers to develop a rationalized, stable legislative and judicial system. The papal system came first, but *post hoc* does not necessarily mean *propter hoc*.

Yet despite the doubts expressed above about certain of the author's interpretations, I believe that this is an important book which tells a fascinating story in the development of Western constitutionalism. The story continues to be of interest as the constitutional struggles of the fifteenth century are now being repeated with increasing intensity within the Roman Catholic Church.

PAUL E. SIGMUND

Princeton University

A Dialogue Between a Philosopher and a Student of the Common Laws of England. By Thomas Hobbes. Edited with an Introduction by Joseph Cropsey. (Chicago and London: University of Chicago Press, 1971. Pp. 168. \$7.50.)

Except for an edition published in Italy a decade ago, Professor Cropsey has presented us with the first modern separate edition of Hobbes's posthumously published *Dialogue*. He has endeavored to provide a more accurate text than is to be found either in Molesworth or in the original edition. His scholarly introduction runs to 48 pages (as compared with 115 pages for the *Dialogue*), and of this some 34 pages are devoted to a careful and exceedingly helpful analysis of the text. He also discusses such questions as when the *Dialogue* was composed, whether or not it is clear that Hobbes was the author, and whether it is right to assume, as has generally been done, that the work was incomplete.

The *Dialogue*, says Cropsey, "is to some extent a polemic against Coke" (p. 11). One must stress the qualifying phrase, "to some extent." Hobbes would appear to be concerned to reconcile his own teachings with those of the common law tradition—sometimes by explaining or qualifying, or even controverting, that

tradition, but also sometimes by explaining the practical operation of his own theory in a way that makes it consonant with that tradition. It does not appear, however, that in this process he ever qualifies the main thrust of his own doctrine.

Yet it may well be that many readers of *Leviathan* will find much here to surprise them. Hobbes's defense of absolute sovereignty and of the absolute power of the monarch in a monarchical regime was never a defense of arbitrariness. His insistence on the right of the king, in the final analysis, to do whatever he judged necessary for the preservation of the realm and the safety of the people, even if that meant violating liberties or other constitutional provisions that he had granted, was never intended as an excuse for unreasonable conduct. Nor did he believe that that would be its consequence. In the *Dialogue*, written many years after the civil war, Hobbes perhaps felt less pressure to brush aside all elements of constitutionalism for fear of providing opportunities for trouble-makers and was more concerned to show the continuity of the prevailing regime, and of the type of regime he favored, with something approaching Lancastrian constitutionalism (although of course he did not call it that).

Never, in the *Dialogue*, does Hobbes use the phrase "the state of nature," although his substance gave him occasion to. Cropsey's explanation for this silence is persuasive. It is that Hobbes is shifting his emphasis. Without for a moment giving up the command theory of law (which he derived from his conception of the state of nature), he now wished to argue that the rationality of the laws was enhanced by the fact that, in practice, kings consulted representatives of the realm, in parliament assembled. Indeed, wrote Hobbes, ". . . there is no King in the World, being of ripe years and sound mind, that made any Law otherwise; for it concerns them in their own interest to make such Laws as the people can endure, and may keep them without impatience, and live in strength and courage to defend their King and Countrey, against their potent neighbours" (p. 166).

Other passages might shed new light on Hobbes, at least to the unwary reader of *Leviathan*. Repeatedly, for instance, he distinguishes sins from crimes. A sin is not a crime until it is made so by law. And yet the sins he refers to are not necessarily things that are, in the modern sense, "sinful." They may indeed even have been good in themselves before they were made crimes (pp. 78-79). Or they may have been evil; or, as we would say, "wrong." Although

nothing here contradicts *Leviathan*, a different impression might have been gained by those who have read too hastily Hobbes's statement in his great classic that in the state of nature the notions of right and wrong have no place.

Another example of the same sort of thing may be found in the passage where the Philosopher asks how laws can be effective, even though they do embody the dictates of reason, "when the greatest part of Men are so unreasonable, and so partial to themselves . . ." (p. 58). Note that the charge is not against all men; but only against "the greatest part of Men."

About law as reason, he has much to say; nor can it be easily summarized, for he seems of a mind to take—at least to call attention to—the best of Coke on this subject, while insisting that it is kings (not lawyers and judges) who have made the law of England. But the law, he seems to concede, is the more rational because it has been fined and refined by a long succession of royal acts of legislation, with the advice of Lords and Commons.

For other matters discussed in the *Dialogue* and for a conspectus of its contents as a whole, the table of contents must suffice: Of the Law of Reason, Of Sovereign Power, Of Courts, Of Crimes Capital, Of Heresie, Of Praemunire, and Of Punishments. Hobbes students are indebted to Professor Cropsey for this scholarly and accessible edition of the *Dialogue*.

J. ROLAND PENNOCK

Swarthmore College

Latin American Political Thought and Ideology. By Miguel Jorrín and John D. Martz. (Chapel Hill: The University of North Carolina Press, 1970. Pp. 453. \$12.50.)

The publication of this book just about doubles the broadly framed contemporary literature on this important element of the study of Latin America. Whatever the merits of the effort may be, however, it is still a long way from making a significant contribution to the discipline.

The book is divided into two parts. Part I describes the evolution of Latin American political thought during the nineteenth century. It is chronologically and conceptually organized around several themes and schools of thought. The several chapters focus on The Eighteenth Century Background; Natural Rights and Popular Sovereignty; Romantic Liberalism and Realism; Positivism; Idealism; and Anarchism. Part I was written by Prof. Jorrín before his death in 1965.

Part II, written by Prof. Martz, shifts the fo-

cus to a description and analysis of the major ideological movements of the twentieth century. It discusses Nationalistic Populism; Marxism; Social Democracy; Christian Democracy; International Perspectives; and Ideologies of Development.

Overall, *Latin American Political Thought and Ideology* is unquestionably a welcome addition to a scandalously infinitesimal literature. While more specialized studies of Latin American politics describe and analyze political parties, elections, voting patterns, labor unions, the Roman Church, the military and even peasant leagues, little has ever been offered by way of setting out the overall currents of thought and ideology. Some beginning has been made on the thought of Bolívar, Latin American positivism and the Mexican Revolution; exploratory forays have been launched to describe and analyze Latin American communism and, more recently, Castroism and the New Left; a little has been written on Argentine thought; and some bits and pieces probe contemporary ideological movements, but only two other scholars—W. Rex Crawford and Harold Eugene Davis—have attempted to put the whole business together in anything approaching comprehensive form. Even Crawford and Davis are not quite on target; both are concerned with more (or less) than political thought and neither is current enough to capture contemporary trends. In this sense, Jorrín and Martz have made a contribution. They have begun the job of bringing together some understanding of the meaning of Latin American speculation on political problems from 1800 to 1970.

But, however noble their intention, the authors fail to bring off the project as well as it deserves. The book is flawed. In the first place, much of the discussion of Part I wanders far afield in the realms of speculative philosophy and theory without offering a specified and clearcut analysis of *political* thought and *political* problems. Though Part II is more clearly in focus, the longish discussion of *indigenismo* in Chapter X again slips off into literature and social thought without quite explaining the movement's relevance to political ideology.

The lack of a chapter on Fascism, moreover, is incongruent in a study attempting to decipher important elements of modern and contemporary Latin American thought and ideology. In the 'thirties and 'forties Fascism exercised considerable influence in Latin America. *Peronismo*, perhaps second only to Castroism in its impact on contemporary Latin American politics, was strongly influenced by Fascism. Fascism also informed the ideology of Bolivia's

National Revolutionary Movement and Getulio Vargas' *Estado Novo*. A book that posits Anarchism as a significant strain of Latin American thought (and it is) misses an equally salient component of the milieu in its failure to deal with Fascism.

The absence of a discussion of the ideology and policies of the modernizing military is more understandable, though no less lamentable. The prototypical Peruvian regime is only three years old, and its significance was probably far from clear when the book went to press. Though some surely exaggerate the modernizing military as Latin America's wave of the future, it is bound to be a semi-permanent part of the continent's political panorama for the foreseeable future, and Latin Americanists are obligated to ferret out its philosophic and ideologic traditions and context. Furthermore, both Parts I and II (and perhaps the entire book) would have been strengthened by concluding chapters putting the material in context. Chapter gives way to chapter with only marginal attempts (*re* anarchism, for example) to integrate the various schools of thought, chronological evolution or ideological positions. The whole is always more than the sum of its parts, but this presentation of Latin American political thought and ideology makes little attempt to show how and why.

Beyond these substantial weaknesses, *Latin American Political Thought and Ideology* is marred by occasional organizational and stylistic infelicities. Particularly in Part I, the book gives far too much space to the presentation of biographic data. The study also falls off into historical chronology in several places—particularly in Chapter IX's discussion of Castroism. Elsewhere, an equally incongruent country-by-country framework is employed to describe and analyze an essentially transnational phenomenon—the description of *indigenismo* in Chapter X.

In sum, Jorrín and Martz have made a beginning, but the importance of the subject merits a more comprehensive and careful treatment.

EDWARD J. WILLIAMS

University of Arizona

Mathematical Sciences and Social Sciences. Edited by William Kruskal. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. 83. \$4.95.)

One of the series of reports by the Behavioral and Social Sciences Survey Committee, this book addresses the many problems arising

from the close connections between the mathematical sciences and the social sciences. It is the joint product of the members of the Mathematical Sciences Panel, including (besides the editor) John Gilbert, Leo Katz, R. Duncan Luce, Alex Orden, and I. Richard Savage. Frederick Mosteller, though not formally a member of the panel, worked closely with it and wrote two of the chapters of the report—the initial chapter illustrating the whole focus of the study by an account of the use of mathematical models in psychological learning theory, and the chapter on "The Profession of Social Statistician."

Throughout the report the panel distinguishes three basic "mathematical sciences," which are sometimes considered much the same. These are mathematics proper—the study of formal symbolic structures; statistics—the study of making inferences from observations (most often on the basis of probability theory); and computation—comprising the variety of disciplines which have grown up around the modern high-speed computer. Each has its special value to the social scientist. Abstract mathematics is illustrated by Kenneth J. Arrow's axiomatic study of social choice, the use of calculus and linear algebra in economics, graph theory in social psychology, and group theory in the study of kinship groups in anthropology. Statistics has become so pervasive in all the social sciences that there are now recognized specialities in specific fields—econometrics, psychometrics, sociometrics, and so on. Modern computation has created great new flexibility in the use of mathematical and statistical procedures for analysis of the vast data banks long available to the social scientist but too unwieldy for earlier computational methods.

A major concern of the panel is how to meet the growing need for social scientists skilled in the analysis of the many problems which beset modern society. This demand is sharpened by the movement for a Council of Social Advisors and an annual Social Report, by the adoption of cost-benefit evaluation in program planning and budgeting, and by the frequency with which government is impelled to launch a program to meet an obvious problem only to find that basic data on which to build the program are lacking (as in the urban crisis and the war on poverty). The first formal recommendations presented by the panel are for the development and expansion of centers for training social statisticians, both in universities and in on-the-job training programs.

Of special interest to academic social science are the panel's recommendations in the field of computation. Along with the great advances in size, complexity, and speed of modern computers have come special difficulties. The report singles out several that plague social scientists in particular: the detection of obscure programming errors or undocumented program limitations, which may produce totally wrong results even though the results seem reasonable; the problem of numerical error structure, which social scientists (typically with modest mathematical training) are usually less well equipped to understand than are their physical science colleagues; and the conversion of most data banks from the standard Hollerith punch-card to a jungle of unstandardized magnetic tape systems—"tapes of different widths, tapes with the data recorded at different densities, and (probably most troublesome) tapes with the data recorded in different formats" (p. 69). The panel's recommendations to meet these and other difficulties deserve complete quotation:

(1) Develop a computer sciences curriculum for social science students that is not based upon impractically exigent mathematical prerequisites.

(2) Encourage more individuals to become proficient in both computing and behavioral science by establishing suitable faculty positions and appropriate paths for professional development.

(3) Stress the importance of program documentation, both for local usage and as a requirement for scholarly publications.

(4) Support measures to put the educational usage of computers within the reach of educational budgets (pp. 72-3).

The thorny problem of how to teach the mathematical sciences to social science students is fundamental, and the panel appropriately reserves it for its final chapter and its final recommendations. Who should teach the mathematical sciences? One proposed solution (not the only one possible since much depends on the specific individuals in the departments concerned) is to have the basic courses taught in the mathematical departments and some specialized courses taught in the social science departments. What mathematical topics? The panel agrees with Herbert Simon that the calculus is the basic entrée into most areas of the mathematical sciences. Social science departments should actively use in their own courses and seminars the competence assumed of their students by their mathematical science prerequisites—meaning that "a substantial portion of the social science faculty should have similar

competence" (p. 80). Theory versus methods? Some mathematical courses fail to interest social science students because of so great an emphasis on abstract theory that "all sense of application is lost"; social scientists, on the other hand, are often impatient with theory and want to know simply "how to do it" (p. 81).

The panel recommends at least some theoretical work; the inclusion in mathematical courses of motivating examples drawn from the social sciences; use of machine programmed teaching material for social science students in algebra, calculus, numerical analysis, matrices, statistics, and computer programming; development by the mathematics departments of courses including advanced topics of special interest to social scientists (Markov chains, matrix theory, numerical analysis, graph theory) without the rigor of advanced mathematical abstraction and numerous prerequisites; the use of joint faculty appointments in mathematical and social science departments—perhaps extending even to a distinct social mathematics or social statistics department; and, because of the need for training students in real world problems with real world data, the establishment of more schools of applied social research.

The detailed recommendations in the reports of the Behavioral and Social Sciences Survey are the responsibility of the panels preparing them and are not necessarily approved by the parent organizations. Nevertheless, this volume presents the most authoritative statement we are likely to get of the problems involved in quantitative social science, as well as the most authoritative set of recommendations on what to do about them. The National Academy of Sciences and the Social Science Research Council, cosponsors of this and the other reports of the Survey, are to be commended for this product, which will be helpful to all political and social scientists interested in using mathematics, statistics, and computer science more intelligently and in attacking the social problems with which their disciplines are concerned.

OLIVER BENSON

University of Oklahoma

The Growing Church Lobby in Washington.

By James L. Adams. (Grand Rapids, Michigan: William B. Eerdmans Publishing Co. 1970. Pp. 294. \$6.95.)

This timely book, with a foreword by Charles P. Taft, views the rapidly increasing economic lobbying by the leading church groups in the nation's capital through the

untinted glasses of a newspaper reporter. Mr. Adams, from the *Cincinnati Post & Times-Star*, has long been a specialist on religious news and writes with an easy acquaintance with the subject.

The time was, not long ago, when church lobbying was neither admitted nor written about. One of the pioneering books (L. E. Ebersole's *Church Lobbying in the Nation's Capital*, 1951), although startling at the time, now seems rather innocent because the church lobbyists have shifted their emphasis from "moral" issues to hard-core economic self-interest. Early church political activity took the form of "petitioning" the various legislatures to mitigate or abolish slavery, then passed on to Comstock-type laws involving moral conduct, and then to agitation for prohibition. Here we learn that the greatest pressure is for acquiring federal and state public funds to help operate church schools and hospitals, church-administered aid to the needy at home and abroad, and even vast building programs for church-operated or church-related colleges dedicated to the propagation of their particular beliefs. In Washington alone, Adams estimates that the church lobby spends a million dollars annually.

Adams of necessity concentrates on the Washington church lobby and then, more specifically, focuses attention on the drive for federal aid for church schools. He provides a detailed step-by-step account of how the coalition among Protestants, Roman Catholics, and Jews first began to function on the same side of the fence with organized labor in the 1963 drive for the Johnson Civil Rights Program. The moral thrust and the political know-how gained by this new alignment of forces carried over immediately and with great effectiveness to the drive for the Johnson Education Bills of 1965. This new thrust was greatly fortified, Adams points out, by the emerging ecumenical spirit paralleling the new compassion for the disadvantaged blacks who presumably were to be happily rehabilitated through education. The resulting climate of opinion put opponents of the education bills in the position of virtually opposing God and country. With all this going for the bills, enactment was assured.

But the author wisely warns us that it is easy to be deluded, as were some of the church lobbyists, into thinking that they alone put the bills through. Actually, Adams substantiates the conclusion that Johnson's "power" was sufficient at the time to have them enacted on his own; doubtless the substantial congressional margins would have been less impressive without the lobbyists' efforts. In fact, the Johnson

drive, we are told, was so great that it caught by surprise the Protestant and Jewish leaders who did not have access to the inside information enjoyed by their Roman Catholic colleagues. Adams devotes several later chapters to some of the aftereffects and afterthoughts about the church lobbyists, the laity, and the public. He senses a growing public cynicism regarding the special tax benefits and other privileges enjoyed by churches in the American economic and social system.

This developing negativism was later substantiated by Leo Pfeffer in the Winter, 1971, issue of *A Journal of Church and State*. Some of the leaders of the Negro community, for example, are becoming hostile toward the use of public funds to support a side-door escape from integration in the church schools. Pfeffer presents some convincing statistics to indicate that this is, indeed, what may be happening in the church schools.

This book should interest several groups of readers: Students of pressure-group politics, the growing numbers of clergymen and laymen concerned with church-state relations, and the newspaper fraternity itself which tends to handle the subject either superficially or with extreme caution. The writer has drawn heavily upon primary sources, but he rarely provides the reader with specific references—doubtless they may be found in his own notes. It is disappointing that there is no bibliography.

R. WALLACE BREWSTER

The Pennsylvania State University

Law and Justice: Essays in Honor of Robert S. Rankin. Edited by Carl Beck. (Durham, North Carolina: Duke University Press, 1970. Pp. 358. \$12.00.)

The unifying theme of this collection of essays honoring Professor Robert S. Rankin is an emphasis upon law and justice. The variations upon that theme are as diverse and generally as intellectually stimulating as a musical counterpart by Johannes Brahms. As indicated in the introductory essay the emphasis upon law and justice was designed to provide contemporary counterpoint to the national emphasis upon law and order.

Taylor Cole and John H. Hallowell have contributed a gracious biographical sketch in which they provided impressive evidence to the profession at large that the academic and public career of Robert Rankin successfully combined insistence upon empiricism and a deep personal commitment to the fulfillment of relevant political and social objectives. This portrait is complemented by Floyd M. Riddick's

essay entitled "State and Local Government: Scholar and Participant," this details Robert Rankin's public service career in state and local government, which was paralleled by his academic labors until his appointment as a member of the U.S. Commission on Civil Rights.

The substantive articles which comprise the major portion of this collection cover a very wide range of subjects. A number of the essays may appropriately be classified as contributions within the broad framework of what Herman Pritchett referred to as traditional constitutional law. These consist of several excellent articles analyzing particular constitutional doctrines or assessing a clearly defined subject matter area with regard to the development of legislative policy and judicial interpretation, e.g., S. Sidney Ulmer, "Searches, Seizures and Military Justice;" Claud H. Richards, Jr., "Religion and the Draft: Jehovah's Witnesses Revisited"; Francis Canavan, S. J., "Constitutional Casuistry: Cases of Conscience"; Spencer R. Gervin, ". . . And Kids Have Rights Too"; Joseph L. Bernd, "Equal Protection of Voting Rights: The Logic of One Person, One Vote"; and John A. Morgan, Jr., "From *Maxwell* to *Duncan*—Progress or Regression?" These essays range from constitutional case analysis to treatments which focus on the impact of Supreme Court decisions upon Congress (e.g., "From *Maxwell* to *Duncan*—Progress or Regression"). Clarence N. Stone devotes appropriate attention to Congressional reaction to specific criminal procedures in his article entitled: "Patterns of Voting on Mallory, Durham and Other Criminal Procedures in Congress."

In related essays several overall analyses of the current status of basic human rights are provided, as, for example, in Philip B. Secor's "Academic Freedom in Political Context: The North Carolina Speaker-Ban Law." Robert C. Clute, Enid Campbell, and W. D. K. Kernaghan summarize civil rights developments in selected foreign countries. Alfred O. Canon analyzes electoral developments in Memphis, Tennessee, in "A Negro Candidate for Mayor in the Urban South." Charles B. Hagan explores "Cigarettes and Public Policy: The Inauguration of a New Policy" and, finally, Laurence E. Noble, Jr. wrote a challenging essay entitled "Reflections on Political Change."

The essays are of uniformly good literary quality. The multiplicity and diversity of these essays do not permit broad or sweeping commentary. Suffice it to say that several of them are excellent expositions. Thus Sidney Ulmer's analysis of "Searches and Seizures and Military

Justice" is characterized not only by precision and commendable parsimony in its descriptive portion, but by incisive comparative emphasis upon the similarities and differences of civilian and military justice. Similarly, Clarence Stone's "Patterns of Voting on Mallory, Durham and Other Criminal Procedures in Congress" embodies appropriate utilization of roll-call analytical methods as well as conventional commentary.

In a rather remarkably relaxed fashion, the numerous contributors have captured successfully the sense of intellectual diversity and felicitous spirit of the fine scholar and public servant for whom this collection was written.

JOHN R. SCHMIDHAUSER

University of Iowa

American County Government; With an Annotated Bibliography. By John C. Bollens in Association with John R. Bayes and Kathryn L. Utter. (Beverly Hills, Calif.: Sage Publications, 1969. Pp. 433. \$17.50.)

The annotated bibliography takes up all but 87 pages of this book. It is the major contribution of the author and his associates and will be an excellent reference since hardly any significant work on county government is omitted. The annotations unfortunately are not critical, simply descriptive. The review of state and local textbooks indicates which ones devote attention to counties.

From a review of the annotations, it is quite clear that there is little sophisticated work on county government. Few political scientists give it more than passing attention. Certainly one would have to agree with Bollens that the literature "is structural, descriptive, and legalistic. It is prescriptive and pragmatic. Conversely, it is not systematic, behavioral, or theoretical. . . . In sum, the literature is incomplete" (pp. 12-13).

In the opening sections, Bollens suggests where we need to go in our studies of county governments, as others have suggested in municipal and state governments. In fact, he feels that some of the newer approaches to the other levels might be utilized by students of county government. He develops an "Index of Political Vitality," which he describes in some detail. But unfortunately, he does not attempt to tell us how to use the index and more importantly what it will tell us, except to suggest that without political vitality "either existing or potential, a local unit is a meaningless relic and a poor investment" (p. 40).

One problem with *American County Government* (a project supported by the U.S. De-

partment of Commerce under the Public Works and Economic Development Act of 1965) is that it deals with 3,049 units of local government called counties. Thus, as the author would agree, we talk in some cases about sparsely populated rural units that perform few functions and in others about major modern, urbanized units like Los Angeles County. It is interesting to know that Petroleum County, Montana (1950 population, 1,026), has a county manager, but not very important unless you come from Petroleum County. On the other hand, many people will want to know how Cook County, Illinois, is handling welfare and environmental control.

County governments are worth studying, particularly in urban areas. The role counties play in some urban areas may be enlarged now that the one-person, one-vote ruling is filtering down to the county level. This could well mean that counties in some parts of the country will now be concerned mainly with urban problems. Many feel that because of their size, counties will be the governments of the future in some urban areas, as suggested by recent restructuring in Jacksonville, in Indianapolis, and elsewhere. The evidence on this trend, however, is not in yet.

In some areas of the country, such as New England, counties are dying; in others, such as California, they are vital, active units taking on new functions and modernizing old ones. We need to know more about counties if we are to understand them and their role in a broader political perspective. But any attempt to measure them across the country will not help much. The author's suggested index of political vitality with five components of (1) resource utilization, (2) volume of intergovernmental linkage, (3) changes in organization and processes, (4) adequacy of public accountability, and (5) extent of voting and competition in elections, will tell us little except that some counties are performing needed services, others are not, some are tackling current problems with the latest methods, others face problems little changed for fifty years; or some counties are in the mainstream of the region's political system while others are not.

At this time and place in the development of the country, we should probably concentrate our studies on urban areas and their governments. As we probe, we may find that county governments are indispensable in the delivery of services to the people who need those services. If counties are necessary for the delivery of services they will be worthy of our study. If not, we might as well ignore them. When

enough people have made enough similar studies, we might be ready to come out with a revised volume called—to paraphrase an earlier work—"Big City and County Politics."

SAMUEL K. GOVE

University of Illinois

Democrats of Oregon: The Pattern of Minority Politics 1900-1956. By Robert E. Burton. (University of Oregon Press, 1970. Pp. 158. \$7.50.)

The general election in 1956 was a critical one in Oregon, a high point in the rebirth of the Democratic Party which had been the minority party in the state for more than half a century. The Democratic candidate was elected Governor, Wayne Morse won another term in the Senate for the first time as a Democrat, three Democratic Congressmen were elected, and the lower house of the State Legislature came under Democratic control. In this book, historian Burton tells the eventful story of this renaissance of the Democratic Party.

Burton begins his account at the turn of the century when the Democratic Party was weakened by division between Silver and Gold Democrats. During the Harding to Hoover era, a motley group of Conservatives and party hacks led an ineffectual and disorganized minority party, so that when Franklin D. Roosevelt came to power in 1932 many party leaders opposed New Deal policies. Younger Democrats who were New Deal supporters joined a new organization, the Commonwealth Federation, which sheltered homeless liberals and became the organizational nucleus for new leadership that would reconstitute the party. World War II and its aftermath was a period of population growth and economic expansion which brought an influx of new Democratic voters. Several other factors contributed to the growth of the Democratic Party. Politically conscious CIO unions were formed during the thirties and forties among lumber workers. Public power became an issue that forged an identity of political interests between farm and labor organizations. New political leaders, notably Monroe Sweetland and Howard Morgan, fought the entrenched old guard of the party, and aided by President Harry Truman, reached positions of party leadership in 1948 and 1950. From then until 1954, the Democrats produced a new generation of able candidates: Senator Richard Neuberger, Edith Green, Charles Porter, Al Ullman, Robert Holmes, and others.

The patterns of change described in this Oregon chronicle are not unique. They resemble the rebirth during the postwar years of the

Democratic Party in Michigan, Minnesota, and Wisconsin. The contributing factors are parallel: the growth and relocation of industrialization and urbanization, the crystallization of new issues, the rise of a new political generation whom Roosevelt inspired, Truman nurtured, and Adlai Stevenson ironically fulfilled. But the Oregon development has some uniqueness. The change in party fortunes has not altered the style of Oregon politics, which the author characterizes as "nonpartisanship, independence of party organization, and the politics of personality." To this might be added a subdued style of politics, rampant and capricious factionalism, and a deepseated populism.

Burton has written a lucid and coherent account of party politics which emphasizes elections, issues, personalities, and factional maneuvers. But there is more narrative than analysis. The clash of interests and the dynamics of electoral behavior are given inadequate treatment. Only one paragraph tells us that

Oregon's measured growth, the religious, ethnic, and racial homogeneity of its small population, and an economy dominated by the resources within the boundary of the state have combined to produce a high degree of intrastate cohesiveness. This, in turn, prompted a relatively stable attitude toward politics, a contrast to some states where cultural diversity and a rapid industrial pace nurtured continuing conflict (p. 5).

On the whole, this is true, but if one looks closely at the politics of some communities in Oregon, partisanship is intense, and cleavages are sharp. The absence of highly organized political parties doesn't necessarily indicate political consensus. We have become sophisticated about party competition and strong and weak party organizations. The analytic usefulness of such terms is limited when applied to states and communities where party organization is diffuse. When party apparatus is only a paper organization, party leaders do not act in concert, and middle level and party workers are in disarray, what then does party mean? The paradoxical answer is that often party attachments are woven into the fabric of cliques, social circles, religious denominations and occupational associations. A party organization, developed or not, may conceal durable political primary groups that make party organization unnecessary.

The author neglects other factors which deserve extensive analysis. The social differentiations in Oregon society reflected in voting data could have pointed to the continuities and change in electoral behavior. Regional differences and economic interests, expressed as they

were in the politics of the state legislature, were significant in the rise of the Democratic Party. The part that interest groups and party factions played in sponsoring candidates deserves greater attention. The author stresses the politics of personality, but does not probe it. When the issues divide and when elections are without issues, what explains the tendency to cluster around personalities? Is it perhaps just that politics and everything else is "personal" in small-scale political activity?

Overall, Burton's history narrates well the events in changing party politics. But that isn't enough. More and more historians are joining other social scientists in making explicit the hypotheses that guide their explanations of historical change. When they do so, they find rich use for sociological, economic, and political data. When historians ignore the perspectives of other social sciences they usually invent their own psychology, sociology, or economics. A broader perspective and further analysis would have enriched this fine study.

LESTER G. SELIGMAN

University of Oregon

The Higher Circles: The Governing Class in America. By G. William Domhoff. (New York: Random House, 1970. Pp. ix, 367. \$7.95.)

Professor Domhoff undertakes to make the case for the proposition that America has an "upper class," that it is a social class, cohesive and self-conscious, and that although it does not "control" policy (pp. 105-06), it does "dominate" it. The last section of the book takes on two sets of critics of his position—the Right (which his own view resembles) and the Pluralists, for all of whom the late Arnold Rose is made surrogate. Maurice Ravel said of one of his popular pieces—in disparagement—that it was "orchestral tissue without music." Professor Domhoff does all right with the music—the tune is a very familiar one—but supports it with a flimsy tissue of pop sociology. The existence of an upper class seems to be an article of faith with the author, an *a priori* construct, and one is reminded of Cabell's *Jergen* where Heaven and the God of Jergen's grandmother are created because she believed they existed.

Professor Domhoff creates the upper class out of the *Social Register* (coverage—thirteen American cities), the opinions of newspaper society editors, lists of directors, trustees, law partners and other such grandees in 15 American cities, *Who's Who of American Women*, alumni and alumnae bulletins of private schools, biographical and autobiographical materials,

and novels about "high society." Although C. Wright Mills treated celebrities as something apart from his power elite, Professor Domhoff says that Mills was mistaken. The jet set is the upper class at play (p. 70).

One of the wonders of this construction is that the upper class is the *only* class in America, which is like having one bookend. Professor Domhoff is willing to say that something like classlessness exists at all levels of society until you get to the top (p. 74). There the "continuum hardens into a social class with more or less definite boundaries and class consciousness," and cohesiveness. The evidence for cohesiveness is that everybody "is related to everyone else, and sometimes many times over" (p. 76); rich kids go to "a handful of exclusive institutions"; upper-class clubs have nationwide memberships; the upper class have their upper-class bodies at the same summer resorts, exercise them in the snows of the same winter resorts, and adopt different life styles from "the rest of the population" (p. 88). In one confusing sentence it is also said, "Finally, after all this, there is death, but it is not the great leveler it is claimed to be when it comes to differences in life style" (p. 90). At funerals, upper-class people cry less, practice cremation more, restrict viewing of the body, and bury it faster than just folks.

The evidence for the second proposition—that the upper class is class conscious—is anecdotal, impressionistic, journalistic, and inferential. Professor Domhoff says that for lack of hard data it is necessary to rely upon the testimony of the occasional insider-expert like E. Digby Baltzell, on novels like those of J. P. Marquand ("a perceptive observer"), and "remarks about the 'nobility' and 'peasants' that used to slip from cultured tongues. Then too, every once in a while an upper-class writer will drop a remark that implies class consciousness" (p. 93). He did, however, circulate a questionnaire to one-third of the 912 Freudian analysts in the country and all of the 70 Jungian analysts to find out what they had to say about the class consciousness of their upper-class patients. He got only 36 replies, but 27 of the 36 thought that upwards of 70 per cent of their upper-class patients were class conscious. It is not said whether this is what brought them into analysis.

The trouble with this kind of demonstration is that it is largely suppositious and subjective. Class is not defined in terms of social and economic functions but in terms of personal attributes. Now, everybody knows that there are rich men and women, that rich folks have pretty

ways, that some play hard and work themselves into cardiac and ulcerous bad health, that others do nothing except glow with gold or shine like dead mackerel and when all this is said, what one has said is that there exists a status group with the indicated qualities. Worth consideration is the view that our society is characterized by "nonegalitarian classlessness," as Stanislaw Ossowski has said in his *Class Structure in the Social Consciousness*—a form of social inequality without social stratification, with much overlapping, without clearly differentiated social and economic functions, unlike the concept of class with which Marx worked. All "classes" in effect do the same thing—they tend the bureaucratic machine—and if there are indeed social differences, they tend to blur, making integration as much a descriptive model as conflict to characterize social and economic relationships, as Marcuse has argued in *One Dimensional Man*.

Having established the existence of a cohesive and class-conscious upper class, Professor Domhoff is then required to show how it operates as the "governing class in America." In his previous work, *Who Rules America?* Professor Domhoff purported to show how the upper class controlled the executive branch of the federal government. His current claim is more modest. Now they only control "most major corporations, foundations, and private universities," but they do not control the political process. The influence that they have in the political process is "dominance," which means that "members of the upper class sit in pivotal governmental offices, define most major policy issues, shape the policy proposals on issues raised outside their circles, and mold the rules of government" (pp. 105–06), although it is also said that "relatively few members of the upper class concern themselves with political matters." The demonstration of dominance is undertaken in the areas of foreign policy, social legislation, and the "struggle for minds." Although the Council on Foreign Relations is said to be the key middle term between the corporations and the federal government in foreign policy, the author says that he cannot demonstrate the influence of the CFR on any single act of policy. Its study groups, however, lead to the publication of important works like Henry Kissinger's *Nuclear Weapons and Foreign Policy*, and of course Henry Kissinger winds up in the White House, and we may indeed have a demonstration of the upper class at work, although Henry Kissinger is not an Anglo-Saxon Protestant, never went to Groton, and may or may not eventually be cremated.

A close parallel to Professor Domhoff's discussion of the Council on Foreign Relations is the treatment of the Institute of Pacific Relations some two decades ago by the Internal Security Subcommittee of the Senate Judiciary Committee. Like Professor Domhoff and the CFR, the McCarran committee said that IPR activities were made possible largely through the support of American corporations, industrialists, and foundations; that it set up active and cooperative relationships with government officials in the foreign policy field; and that it achieved "close organic relationships with the State Department through the interchange of personnel, attendance of State Department officials at IPR conferences, constant exchange of information, and social contacts." Like the McCarran committee also, Professor Domhoff is suspicious of the academy, and in a piece of insinuation worthy of his predecessors in the early 1950's he says, "While I do not believe for a minute that the power elite dictate to these scholars [who participate in CFR activities] as to what they should say, it should be clear that members of the power elite see no reason to discontinue their support of such efforts" (127).

Professor Domhoff says that there are differences between him and the ultra-conservative Right. He does agree with them that the country is run by rich, Eastern, Ivy League-educated, well-traveled, businessman, internationalist, *Social Register* types many of whom "travel under the label of liberals"; and he joins them in thinking that they have some justification for being angry about the welfare state. But he does not accept the ultra-conservative concept of the power structure embodied in the phrase the "international Communist-Jewish conspiracy" because the internationalism of which he speaks is not ideological but economic; there is only a handful of Jews in the upper class and they "are more upper class than Jewish" (p. 297); and "Whatever we may say of him, David Rockefeller is not a communist" (p. 291).

In his last pages, Professor Domhoff disclaims conspiratorial theories of upper-class control or dominance, but he chides pluralists for underestimating the degree to which there is unity, consciousness, and manipulation in the social group that is the subject of his book, and he says, "If pluralists ask just how unified, how conscious, and how manipulative, I reply that they have asked a tough empirical question to which they have contributed virtually no data—at the same time pointing to findings such as I have presented throughout this book" (p.

299). So, it would seem to be up to the pluralists to prove Professor Domhoff's case for him, since the findings of the book do not support the conclusion with which he started.

EARL LATHAM

Amherst College

The Politics of Rescue: The Roosevelt Administration and the Holocaust, 1938–1945. By Henry L. Feingold. (New Brunswick: Rutgers University Press, 1970. Pp. xiii, 394. \$12.50.)

The hideous reality of the destruction of European Jewry by the Nazi Germans has been so difficult for us to accept as having actually occurred that we have avoided contemplating this event, analyzing its course of development, or relating it to our theories of political behavior, which seem so feeble in the face of such barbarity. Instead with notable exceptions such as Raul Hilberg, we have debased the meaning and implications of this catastrophe for modern Western civilization, or we have attempted to excuse the central actors by spreading the blame in ways that remove the focus from the fact that the Nazis really did commit genocide upon the Jews of Europe. Thus, we have trivialized the word "genocide," using it to describe a range of unpleasant activities; almost any policy or program which is disapproved these days becomes "genocide." Massive multi-billion-dollar programs to improve health, redistribute income, provide for welfare, redevelop urban centers, etc. are called genocide, as in this statement by a group of New York City Urban Fellows in a 1971 American Assembly document: ". . . We recognize that everyday government is a party to genocide by its inaction and the active intent of some of its agents." Everyday government! In America!

But to equate any and all grievances with "genocide" is as ridiculous as to attempt to find some other guilty parties for the Nazis' crimes. Some years back Pope Pius XII and the Roman Catholic Church seemed to be leading candidates in this share-the-guilt process. Then Arthur Morse in his polemical *While Six Million Died* tried to extend the guilt to the American government under the leadership of Franklin D. Roosevelt. Even worse was the implication of Hannah Arendt's study of the Eichmann trial that it was the Jews themselves who were at fault for not having resisted the Nazis more vigorously. If all are guilty, even the victims, then no one is really guilty, and what can we say or learn about the specific fact that the Nazis, and only the Nazis, organized and directed (with collaborators of course) a com-

plex system of literal mass murder that succeeded in its purpose of making Europe "clean of Jews"?

We need scrupulous scholarship as a corrective to the looseness of thought and language, the evasiveness, and the myths of the dispersion of guilt before we can begin to estimate what political science theory can contribute to an understanding of the holocaust, and Henry L. Feingold has provided us with such a corrective to Morse's sincere, but overstated case against the Roosevelt administration.

The facts, however, are ugly. While the Nazis were announcing their intentions publicly, and more secretly planning and implementing their "final solution" of the Jewish problem, i.e., extermination, the American government was aware of the great jeopardy of the European Jews. Yet, despite warnings, pleadings, moral appeals, and political threats, the United States did virtually nothing to succor the masses of Jews being killed by the Nazis.

How could this apparent passivity have occurred under F.D.R., whose administration and advisory circle were studded with Jewish luminaries who created what Feingold calls a "litany" of names—Morgenthau, Frankfurter, Rosenman, *et al.*? In Congress there were leaders like Bloom, Sabath, Dickstein, and Celler, and there was a host of private agencies devoted to the refugee problem. How could this government have remained indifferent to its Jewish and humanitarian impulses to provide relief and safe haven for the threatened millions?

As Feingold points out, the explanation for the American failure to act is compounded of diplomatic, legal, and high war-policy reasons combined with domestic political considerations, as well as less elevated phobias against a stream of refugees including possibly some spies or saboteurs. The Roosevelt administration did take the initiative in calling the Evian Conference in 1938, which led to the establishment of the ineffectual Intergovernmental Committee for Political Refugees, and, again, during the war, the Bermuda Conference in April 1943 met to review the plight of the Jews and others suffering under the Nazis. The scope and seriousness of the problem were not fully realized until much too late, when the War Refugee Board was created in 1944; by the time it was in full operation the war was almost over, and the European Jews were almost all dead.

Breckinridge Long, Assistant Secretary of State, who was primarily responsible for the administration of immigration law and refugee policy during the war, clearly resisted any liber-

alization of policy, and Long was supported at key junctures by F.D.R. personally. American policy was simply to win the war as quickly and decisively as possible, and this policy was believed to be the best answer to a host of problems, including what was considered the refugee problem. Atrocity stories had been punctured after World War I, and there was a deep skepticism concerning reports about Nazi horrors. Any move to liberalize the restrictive immigration laws, it was feared, would call forth a right-wing reaction that could harm the war effort. Our allies were equally coy about what to do about locating large numbers of refugees, and little despots like Trujillo in the Dominican Republic were not above playing games about admitting immigrants to embarrass their democratic neighbors. But above all, American policy makers simply did not believe that the Jews were being exterminated, despite the intelligence reports, and their reactions continued to take conventional forms—dispatch of relief funds, food and medical supplies, censuring the persecution of minorities, providing for refugees, and adjusting or enforcing immigration quotas. That the Germans were not merely persecuting the Jews, but were exterminating them did not finally sink into the consciousness of American policy makers until the death camps were opened by the liberating armies.

Professor Feingold has had the unusual courage to look at this maelstrom of events, actors, programs, and policies in the finest tradition of committed historical and analytic scholarship. His book is based on the thorough review of the primary sources one would expect in a work originating as a doctoral dissertation. As he shows it to us, American policy making in the face of Nazi crimes was so stupid and repulsive as to make one despair at our insensitivity and incompetence, even given the understandable limitations of imagination and will that foreclosed more direct and vigorous action to save a larger remnant than that which did survive the Nazi murder apparatus. Those interested in demythologizing the Roosevelt administration's role in relation to the holocaust can rely on Feingold's excellent volume for the details of policy, diplomatic exchanges, and accounts of conflicts among those interested in rescue and relief, and between them and the cautious and dilatory State Department bureaucrats.

For political scientists interested in theory, this book will not provide much encouragement. Our debates about pluralism and elitism pale into explanatory insignificance when one contemplates the enormities considered in Pro-

fessor Feingold's book. Reading it is a sober and necessary experience for those whose ambitions still lie in elaborating general theories of political behavior.

MORTON J. TENZER

University of Connecticut

Agenda for a City: Issues Confronting New York. Edited by Lyle C. Fitch and Annmarie Hauck Walsh. (Beverly Hills, California: Sage Publications, 1970. Pp. 718. \$17.50, cloth. \$4.95, paper.)

Having inflated the expectations of America's universities and underdeveloped nations elsewhere, the Ford Foundation has now decided to assist our central cities. Hence, among its various benefactions, a grant to New York's Institute of Public Administration to "analyze options open to the city administration taking office in 1970." And thus this advance agenda, its assignments commissioned prior to polling day, intended for whoever was going to win the 1969 elections. The purported result is a series of statements by accredited and nonpartisan experts, equally available for a John Lindsay, a John Marchi, or a Mario Procaccino. The volume provides a valuable case study—of how Ford and one of its prominent handmaidens undertake to counsel a city.

The book consists of 16 principal papers, five of them accompanied by critical "commentaries." Obviously the Institute of Public Administration wanted men of sober stature, sound judgment, and clearheaded experience, who could give the next mayor advice on how to cope with the tumultuous 1970's. It would have been fascinating to have sat in on the discussions where possible contributors were considered: a transcript of those sessions would illuminate much about the thinking which goes on in our more insulated institutions. At all events, the upshot was to turn to people who had ringside seats during the early 1960's. The articles on health, education, and welfare, for example, were assigned to men who had served as Commissioner of Health, Superintendent of Schools, and Commissioner of Welfare under Mayor Robert Wagner. In other words, former stablemates of Lyle Fitch, who happens to be editor of this volume, is head of the Institute of Public Administration, and was City Administrator during Wagner's tenure. They tell us how well they ran things and how much was accomplished while they were in charge.

And for "race relations," whom to invite but Nathan Glazer, who—in company with Daniel Patrick Moynihan—explored the city's ethnicity in this same era? His chapter, really a rear-

ranged version of a 1970 preface to *Beyond the Melting Pot*, tells the city that it should give greater encouragement to its Negroes, the better to undercut the appeal of its Blacks. The analysis of the Mayor's office comes from Wallace Sayre, who celebrated the city's pluralism as it was arranged back in 1960. He writes about that office almost as if someone named John Lindsay had never been mayor. And so on with a predictable roster: Roger Starr on housing, Dick Netzer on finance, and Jameson Doig on crime and the police. (Was James Q. Wilson tied up that week?) Some interesting information emerges: in 1968, for example, the 144,351 households in the city having incomes over \$15,000 on the average paid less than 12 per cent of their earnings in rent. But no surprises.

Symposiums of this sort always enlist one house maverick, brought in to be "provocative"—as chairmen are prone to put it in their introductory remarks. In this instance, Adam Walinsky joined in submitting a plan for decentralizing the city: not particularly original, considering how many blueprints of this sort are around, but refreshing nonetheless. However lest readers be carried away by Walinsky's impracticality, the editor recruited not one but two commentators in this case—Herbert Kaufman and Harvey Mansfield—who reviewed Walinsky's paper and graded it C+. (Its proposals, Kaufman concluded, "do not inspire confidence.") Throughout most of these 718 pages we see the city from the administrator's window, antedated to the days when bureau chiefs encountered much less caviling from opinionated citizens.

Given its auspices, we might expect an emphasis on official authority and professional prerogatives. Yet while such an approach may have been viable in earlier decades, it fails to come to grips with the kind of people who are now the subjects and objects of urban administration. There is an air of wistful thinking on these pages: if only eight million New Yorkers would consent to act as constants in the administrative equation, if they would only keep quiet and hold still, they could be efficiently serviced. However *homo urbanus*, at least in New York, has become a much more obstreperous creature in recent years; and governing him calls for talents nowhere elucidated in this volume. Strikes, sit-ins, and subtle and not-so-subtle sabotage are now the norm, with high school principals, police commanders, and welfare mothers continually available for confrontation. Most of the contributors all but omit mentioning a man named John Lindsay, and

hardly any suggest that his mode of operation may have something to be said for it. As an administrator he may not have adhered to the best of protocols, but in possessing a personality and impressing it on the city, he has single-handedly raised the spirits of all New Yorkers—including those who hate him—at a time when urban problems outrun the resources of available agencies. There is almost a grudging quality in this omission: the reaction of writers mistrustful of anyone who unsettles conceptions of governance a decade out-of-date.

If New York needs an agenda, I would much rather recommend the columns of Jimmy Breslin, Murray Kempton, and Pete Hamill, along with the observations of Bella Abzug, Charles Rangel, and any random group of parents in Bensonhurst or Bedford-Stuyvesant. Indeed, had the Ford Foundation solicited the views of the janitors and cleaning women who service its air-conditioned East Side offices, it would have come up with a much more useful and certainly a far less costly compendium.

ANDREW HACKER

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Public Expenditures and Policy Analysis. Edited by Robert H. Haveman and Julius Margolis. (Chicago: Markham Publishing Company, 1970. Pp. 596. \$11.95, cloth; \$6.50, paper.)

Analysis and evaluation of governmental expenditure programs have long been a challenge to both the practitioner and theorist of public administration. Particular concern has been devoted to achieving more rational analysis in the choice of policies and allocation of resources to a growing number of public programs. From the beginning of the budgeting movement in government to the present, the quest has yielded major milestones, ranging from A. E. Buck's treatise, *Public Budgeting*, through monumental efforts in methodological improvement by the Bureau of the Budget in the 1940s and 1950s and the introduction of performance budgeting, to the most recent attention by economists in the concept of "PPBS"—the Planning, Programming, Budgeting System. Professors Haveman and Margolis have made a significant contribution in this collection of twenty-six papers which is intended for use in "Economics and Political Science Departments and Schools of Business and Public Administration."

The thrust of the papers is almost wholly economic and illustrates high quality scholarship of noted authorities in the field of public finance. Conceptually its range is wide, encompassing discussions of microeconomic concepts

as well as evaluations of their application to programs of the federal government in PPBS. Papers on discount rates, shadow prices, the evaluation of externalities, and the experience of PPBS in defense and domestic applications give the work depth and breadth, though political scientists may find that inadequate attention has been given to noneconomic issues.

Organizationally, the book follows a useful sequential pattern. The question of how to delineate the parameters of the public sector by economic analysis is approached from different vantage points and provides a stimulating, though heavily theoretical, beginning. Peter O. Steiner examines the nature of public goods and the public interest, stressing the multiple factors which contribute to their determination and the limitations of using efficiency alone as a criterion. His discussion establishes a launching point for papers on issues involved in choosing market versus nonmarket allocations of resources (Kenneth J. Arrow), alternative governmental actions for dealing with problems caused by technological externalities (Otto A. Davis and Morton I. Kamien), the importance of uncertainty as a cause of market imperfections and a determinant of appropriate government action (Richard Zeckhauser), and a paper by Burton A. Weisbrod on the pervasive impact of most governmental economic actions on the distribution of income.

Of special note among the discussions of institutional considerations in determining effective public policies through the expenditure route are Charles L. Schultze's treatment of incentives, penalties, and rewards and Murray L. Weidenbaum's analysis of numerous formal restraints in the current federal budgetary process. Schultze lucidly examines the role and potentialities of incentives in the several stages of policy making, the types which hold most promise, and the theoretical and political constraints which determine their relevancy. Weidenbaum perceptively indicates how federal trust funds, permanent and indefinite appropriations, fixed earnings, and construction or other long-term projects have provided major stumbling blocks to rational decision making.

The section devoted to selected analytical problems in policy determinations is of generally high quality and a pertinent prelude to the discussion of PPBS. Two papers merit particular attention: William J. Baumol's discussion of discount rates and the development of principles for determining them, and Julius Margolis's discussion of shadow prices for incorrect or nonexistent market values.

Papers in the final sections on PPBS in the federal government are especially valuable.

Certainly no single methodological advance in federal decision making has had more impact and been surrounded by more controversy than the introduction of this conceptual system. These chapters are mostly by authors who have been deeply involved in implementing the PPBS approach, and their insights reveal the causes of both its failures and successes.

Unfortunately, the implication that PPBS heralded the beginning of "analysis" or "rational analysis" in federal budgeting and decision making seems to pervade a number of the papers. This, of course, is a somewhat circumscribed view of administrative history, since the basic concepts of PPBS—planning, programming, defining objectives, cost-benefit analysis, multi-year financial plans—did not originate in the mid-1950s or '60s. They were recognized in the theory and practice of federal budgeting and decision-making during the 1940s and 1950s. PPBS performed the major feat of combining these elements into a single rational system, emphasizing them, extending their scope, altering and structuring their presentation, establishing a standardized language, creating an educational system to supply technicians, introducing some newly refined tools of analysis, and imposing a rigorous discipline upon the process. It is stretching to represent it as more. Perhaps greater balance might have been given to the discussion of PPBS in the federal government had papers been included by scholars and officials who participated in the work of the Bureau of the Budget and the more advanced federal agencies before the advent of PPBS.

To some extent the need for greater historical and political perspective is provided by economists with governmental experience. Alice M. Rivlin, in examining the application of PPBS to the Department of Health, Education, and Welfare, places PPBS in a real-world perspective, recognizing its strengths and potential for achieving more quantitative analysis and reason in decision making, but at the same time realizing the constraints imposed both by politics and individual value systems. It is a refreshingly candid discussion which helps to dispel some of the more assertive claims of accomplishment elsewhere in the book.

Political scientist Aaron Wildavsky provides a colorful analysis in "Rescuing Policy Analysis from PPBS," and probes an obvious weakness of the more passionate advocates of PPBS—a limited political perspective. But, in all fairness, he is at times inclined to overstatement, as in the following comment: "PPBS must be tremendously inefficient. It resembles nothing so much as a Rube Goldberg apparatus in which

the operations performed bear little relation to the output achieved. The data inputs into PPBS are huge and its policy output is tiny . . ." (p. 469).

Space does not permit an adequate critique of the many fine papers on the strengths and limitations of PPBS in enhancing budgeting and decision-making. But those on PPBS and Congress by Senator William Proxmire and on PPBS's status and future by Assistant Director of the Budget, Jack W. Carlson, as well as the lucid appraisal and analysis of resource allocations in health by Robert N. Grosse all provide much insight and substance.

All in all, *Public Expenditures and Policy Analysis* is a most useful core work for courses of study in public finance, budgeting, and decision making.

WALTER G. HELD

Washington, D.C.

The Seniority System in Congress. By Barbara Hinckley. (Bloomington: Indiana University Press. 1971. Pp. 146. \$5.95.)

Barbara Hinckley sets out to show that the seniority system, instead of being a villain, and a bastard to boot, is a legitimate offshoot of the American political system, not responsible for all governmental ills, and possibly even possessed of some heroic qualities. Her goal is modest, as are her expectations, for she doubts that the study will have much effect on the conventional wisdom concerning seniority. Clearly, the book will be most convincing to those who are already convinced. I believe, however, that even the skeptical will have to admit that *The Seniority System in Congress* is a well written, ingeniously researched, and carefully organized monograph.

Prof. Hinckley focusses on the claims that the seniority system gives leadership in Congress to aged, conservative representatives from one-party and rural areas who have a strong antiparty and anti-President bias.

Her research concentrates on the ten postwar congresses that served between 1947 and 1966. She frequently uses a peel-away technique which I found both interesting and valuable. For example, when she is studying the impact of the seniority system on the share of committee posts going to House members from rural districts, she lists, in the extreme left hand column of a table, the number of rural and urban congressional districts in the hands of Democrats in 1961; in the extreme right hand column, the number of committee chairmen from rural and urban districts in that year. In between, she lists, in descending order, the number of districts (1) that had been held by the

same party, (2) that had been held by the incumbent, and (3) that had had members on the same committee for at least the preceding decade. Her conclusion is that "none of these factors linked to the seniority system—even in combination—is sufficient to explain the rural predominance in chairmanships" (p. 62).

At the end of the study, she draws a similar but broader conclusion:

The effect of the seniority system is limited because its requirement of continuous service in House or Senate can be met by a majority of the congressmen. . . . Indeed, there are more congressmen qualified by long congressional service for committee chairs than there are chairs to be filled; hence factors other than congressional seniority, such as original committee assignments, and subsequent reassignments, can and do influence the selection of chairmen (pp. 108–109).

She does suggest that changing the system would be "something on the order of redirecting ocean currents," (p. ix) and that the status quo may have some advantages: "Power centralized in the hands of party leaders could be more easily controlled from the outside than power dispersed among many centers. The seniority system . . . strengthens the Congress in its well-known desire to be independent of the Presidency" (p. 112). Then she goes on to describe it as "a profoundly conservative institution—not because it biases the kind of leaders selected, but because it reinforces the conservatism already present in Congress" (p. 113).

I must admit that I wish Mrs. Hinckley had been a little more positive in her last chapter. Is it necessarily conservative to have a seniority system which reinforces separation of powers and, in so doing, keeps the President somewhat closer to being the first among equals rather than monarch among courtiers? I would think that faculty members, who enjoy the benefits of a tenure system, would be able to understand the benefits to the American government of a not too dissimilar seniority system.

GEORGE GOODWIN, JR.

University of Massachusetts, Boston

The Years of MacArthur. Vol. 1: 1880–1941.

By D. Clayton James. (Boston: Houghton Mifflin Company, 1970. Pp. 740. \$12.50.)

The first volume of a two-volume life of General Douglas MacArthur being written by D. Clayton James, a historian at the University of Mississippi, is a major achievement in its own right. It is the first scholarly biography of this paradoxical military figure, based on exhaustive examination of documents and interviews held with high-ranking military officers, including General Eisenhower.

In part, it is the biography of a family, since Douglas MacArthur represented the third generation of public figures. The grandfather was sufficiently active in politics to become a lieutenant governor and a federal judge. The father, General Arthur MacArthur, began a precocious military career in the Civil War. After many years of professional military life, he became Military Governor in the Philippines, and while achieving the rank of Lieutenant General, was probably cut off from the highest army position of Chief of Staff because of a feud with William Howard Taft. The fierce pride of a protecting mother helped Douglas become the noble patrician—a role the author sees as far more characteristic of Douglas the military man than was the role of military despot.

Since Professor James makes the book "in some respects, a story of the development of the modern American Army" (p. viii), it is valuable for its description of West Point during General MacArthur's years as a cadet. This description serves as background for the very striking chapter on General MacArthur's term as a young superintendent after World War I. The author asserts that "he, more than any other man, led West Point across the threshold into the rapidly changing world of military education, . . . one of his most important contributions to the development of the modern Army" (p. 294).

Since I was both a G.I. in MacArthur's theater during World War II and a junior officer in his headquarters, I found this first volume tantalizing because it takes MacArthur only as far as Pearl Harbor. Moreover, the author himself states his belief that General MacArthur "a century hence will be most appreciated for his role as an administrator, rather than as a warrior" (p. viii), and that the most important period of his life was 1946–1950, which will be covered in the second volume. The account of MacArthur's service as a combat general in France during World War I, however, foreshadows some of the characteristics he displayed in the Pacific War.

One of the paradoxes of General MacArthur's military career is on the one hand his ability to evoke the fierce loyalty of subordinates and on the other his phenomenal lack of judgment in certain basic areas. His ability to promote loyalty is seen in his years as public relations officer under Secretary Newton D. Baker before going off to France, and later as Chief of Staff, fighting to regain a viable force after the lean years following World War I. His lack of judgment as Chief of Staff is apparent in his approval of plans for military mobiliza-

tion that were found unrealistic by his successor and in his failure to foresee the need for mechanization. Furthermore, when MacArthur became Field Marshal under President Quezon in the Philippines, he grossly overestimated the defense program, and he underestimated the Japanese military capacity and did not anticipate the war. His Philippine plans for a citizen's army were criticized by some military men and journalists, both in the Philippines and in Washington, as unrealistic. Professor James accomplishes a great deal in setting up this record of MacArthur's achievements and shortcomings.

Valuable sections of the book cover MacArthur's term as Chief of Staff, his contribution to a program of reorganization of the Army, his role in the court-martial trial of General William Mitchell, and the notorious routing of the Bonus Army. As war clouds became unmistakable in the Pacific, MacArthur got the combined command by virtue both of his lifelong concern with that area and of some personal maneuvering. The Philippine Army was not remotely prepared for the combat which it would face, and for this the author places blame squarely upon MacArthur for "his over-confidence and unjustified optimism as to the abilities of himself, his staff, and the untried Filipino soldiers. . . ." (p. 609) in the context of support from Washington that was too little and too late.

Professor James, although meticulous in his scholarship, does not shun judgments. The force of these judgments is not always clear, because sometimes they do not flow clearly from the material he presents, or they lose strength from the manner in which they are presented. All in all, however, this is a distinguished beginning, and Volume II will be eagerly awaited.

DALE PONTIUS

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Felix Frankfurter on the Supreme Court: Extrajudicial Essays on the Court and Constitution. Edited by Philip B. Kurland. (Cambridge: The Belknap Press of Harvard University Press, 1970. Pp. x, 572. \$12.50.)

The verdict on Felix Frankfurter the Justice is not yet in, and there is every reason for believing that the jury will be deadlocked for some time to come. Result-oriented detractors will no doubt continue to stress that in twenty-two years on the Court he authored remarkably few enduring "landmark" constitutional decisions: after all, *Gobitis*, *Wolf*, and *Colegrove*, for example, failed to survive even his own tenure on the Court. Loyal followers, on the other

hand, will just as assuredly persist in avowing that the measure of individual greatness is neither the number of Opinions of the Court that make the casebooks nor the number of dissents that eventually become the Law of the Land. Felix Frankfurter's greatness, the acolytes will continue to argue, is to be found in his craftsmanlike opinions, which set a principled and scholarly tone for constitutional decision making: he clarified issues for a sometimes stumbling and befuddled Court; illuminated the past and probed the future for a sometimes myopic Court; and, most important, constantly reminded an increasingly bold Court of its own limitations.

Whatever the judgment on the judicial tenure of Justice Frankfurter, his extra-judicial comments on the Constitution and the Court, meticulously and thoughtfully edited by Professor Kurland, lend weighty support to one proposition: Felix Frankfurter, off the bench, was the most astute student of the Supreme Court in the first six decades of this century. Such a bold judgment requires some explanation; there are of course other worthy claimants—each of whom was in some respects Frankfurter's peer or better. To confine the roster to those no longer among us, Edward S. Corwin, Charles Grove Haines, Zachariah Chaffee, and Mark DeWolf Howe were at least his equal in mapping the highways and byways of constitutional development. And it is difficult to imagine more skillful dissectors of cases or expositors of faulty reasoning than T. R. Powell or Henry M. Hart. Yet on balance, Frankfurter possessed a versatility and catholicity of knowledge which none of the others could match. He was the one Court-watcher who was at once a legal scholar, historian, philosopher, and a close—and active—student of the social and political scene. He was the only scholar to bring such a background to the study of that which he so adored—the Constitution and its principal expounder, the Supreme Court. Whether writing for the *Harvard Law Review*, the *New Republic*, or a convocation of notables, he managed with characteristic clarity and grace to bring fresh insight to the topic at hand.

Professor Kurland's rich collection of off-the-bench Frankfurter has much to tell about Justice Frankfurter's performance on the bench. First, although many of the Justice's cruder critics have accused him of changing his views (i.e., of abandoning "liberalism") when he donned the robe, anyone familiar with his early writings can hardly claim surprise at his opinions in the area of individual rights. A 1925 article presaged his future stand in *The Flag Salute Cases*, *Dennis*, *Feiner* and a host of

other cases involving Jehovah's Witnesses, Communists, and sundry street-corner rabble-rousers. In a *New Republic* essay entitled "Can the Supreme Court Guarantee Toleration?" he applauded the Court's holdings in *Pierce v. Society of Sisters* and *Meyer v. Nebraska*; at the same time he made it equally clear that striking down illiberal legislation in the name of the Constitution was hardly distinguishable from vetoing minimum wage legislation, and "that the real battles of liberalism are not won in the Supreme Court." Similarly, a 1930 piece previewed his celebrated *Adamson* concurrence. Rejoicing over the recent decision in the *Scottsboro* case, he observed that the fourteenth amendment "is not the basis of a uniform code of criminal procedure federally imposed . . . [i]n no sense is the Supreme Court a general tribunal for the correction of criminal error." What the Court "will not suffer [is] . . . 'judicial murder.'" Justice Frankfurter practiced pretty much what Professor Frankfurter preached.

Second, if the early writings suggest that the essential commitment to judicial "self-restraint" was well established by the time Frankfurter was elevated to the Court, they also provide a clue to the source of his agonizing and the ambivalence which characterized so many of his most notable opinions. One possible reason that pat solutions did not flow facilely from the Frankfurter pen was that, observant as he was, he did not fully realize that his two acknowledged mentors and heroes, Justices Holmes and Brandeis, held rather dissimilar attitudes about the nature of law and the judicial process: Holmes was a detached and skeptical philosopher who eschewed the popular press in favor of the classics; Brandeis, a committed and policy-oriented statesman who was concerned with what he labeled in quite un-Holmesian language "the concrete problems of practical justice." As a law professor or publicist between World War I and the Second New Deal it was relatively easy to maintain simultaneous allegiance to both, since their approaches usually led to the same result when it came to concrete cases. As a Justice in the 1940's and 1950's, after the Court's focus had shifted from economic regulation to civil rights and liberties, it was difficult to be *both* Holmes and Brandeis. The skeptical Holmes might well have voted to uphold the state in the *Flag Salute Cases*; his dissent in *Meyer v. Nebraska* dramatizes the extent to which he would defer to the judgment of the political branches. On the other hand, it is difficult to see Brandeis, who was with the majority in *Meyer*, in such a role; as Professor Louis J. Jaffe has brilliantly argued ("Was Brandeis an Activist? The Search for Interme-

diate Premises," 80 *Harvard Law Review* 986 [March, 1967]), Brandeis was an "activist" and, as his concurrence in *Whitney* and dissent in *Olmstead* evidence, he was forthrightly prepared to say no—and in sweeping language. (While Holmes joined Brandeis' opinion in *Whitney*, the language is pure Brandeis; and it is significant that Holmes elected to write a separate dissent in *Olmstead*.) Nowhere is the tension between the philosophies of Holmes and Brandeis more evident than in the opinions of Felix Frankfurter. Indeed, one could argue that there were perhaps two Frankfurters—the Holmesian Frankfurter of *Gobitis* and the Brandeisian Frankfurter of *Sweezy*.

Finally, his essays about, and tributes to others who had the privilege of sitting on the Supreme Court suggest that Felix Frankfurter labored under the burden of being the most self-conscious Justice ever to sit on the Court. The evidence abounds. He was notably preoccupied with measures of greatness, judicial style, and the art of opinion writing. Thus some argue, usually by implication, that Frankfurter was so engrossed with the critical judgments of posterity that his own opinions all too often sacrificed substance for style. Such a view is wide of the mark. Frankfurter never lost sight of substance; rather he saw clearly that style and substance were inextricably related. Law was of course not literature; but opinion writing involved the art of exposition, and he believed that the great Justices possessed the ability to set forth their ideas with compelling force and clarity. If Justice Frankfurter's opinions appear self-conscious, overwritten, or unnecessarily academic, it was because he was the Justice most painfully aware of the perennial and subtle paradoxes of constitutional adjudication.

Professor Kurland's skillfully edited compilation of Felix Frankfurter's extrajudicial essays is not just another collection of legal papers. Rather it is a fitting and revealing testimony to the persistently self-demanding, complex, and subtle qualities of mind that Felix Frankfurter brought both to the study of the Supreme Court and to the Court itself. Whether the essays reveal more about the Court or about their author is an intriguing question. Their merit is beyond dispute.

D. S. HOBBS

University of California, Los Angeles

The Wartime Journals of Charles A. Lindbergh. By Charles A. Lindbergh. (New York: Harcourt Brace Jovanovich, Inc., 1970. Pp. xx, 1038. \$12.95.)

While living in England in November 1937 Charles A. Lindbergh, the famed aviator and

national hero, felt that Europe stood on the verge of war and that as a close observer on the scene and consultant to the American government on European aviation he was himself taking an important part in the events that were forming "one of the great crises of world history." So he decided to do what he had never done before, to keep a daily log of his activities and thoughts. Although he had no intention at the time he wrote them of publishing his journals, he believed they could be valuable because aviation had become a possibly decisive element in warfare, a development he was in a unique position to appraise because he had access to political and military leaders in the great powers of Europe. Later, when he opposed American intervention in the Second World War, becoming a leading spokesman for the America First Committee, his journal keeping acquired another purpose, to serve as his personal record of the battle against intervention.

With only one gap of about a year, Lindbergh kept up his log books until the summer of 1945 when he visited wartorn Europe to examine once more what remained of German aviation. By that time his purposes in maintaining his daily diaries had been achieved and he wanted to devote himself fully to writing another book, *The Spirit of St. Louis*. Twenty-five years later, still smarting from a feeling of having been persecuted for his political activities during the debate over intervention, he decided to publish the journals so that posterity could view the record as he saw it. These journals, reduced by a third from the entries in the original manuscript books but intact in the essentials, comprise that record, an account of seven important years in Lindbergh's life. Believing that men can profit from past experience, he also had a utilitarian purpose in publication. He hoped his journals would "help clarify issues and conditions of the past and thereby contribute to understanding issues and conditions of the present and the future."

What emerges from this big, rambling book is the picture of a troubled, introspective, sensitive, articulate conservative man of inflexible opinions and of often naive political and social attitudes. As an isolationist, Lindbergh worked to keep the United States out of a war that he thought threatened the survival of Western civilization. This is why in 1939 he gave up the privacy he and his family deeply cherished to become a political activist and endure the abuse that politics brings. He opposed America's entrance into the Second World War not because he considered war senseless, but because he wanted his country to fight "on the *right* side of

an intelligent war!" Even though he allied himself with pacifists in his antiwar activities, he disliked pacifism.

Nordics elicited Lindbergh's admiration, and it pained him to see "our own northern people" fighting each other. This theme, a form of racism which sees Nordics as somehow superior to other peoples, weaves itself through the journals. "I cannot help liking the Germans," he wrote in the opening pages. "They are like our own people" (p. 5). His admiration for the Germans led him to believe that he had found more "personal freedom" in Nazi Germany than in any other European country, or the United States. Germany, in his view, stood as a bulwark against the hordes of Asia and the communism he detested. "A Russian-dominated Europe would," he felt, "... be far worse than a German-dominated Europe" (p. 599).

Along with the Soviets, Lindbergh disliked the English (in whom he had lost confidence), Jews, and Franklin D. Roosevelt. "England," he wrote, "is a country composed of a great mass of slow, somewhat stupid and indifferent people, and a small group of geniuses" (p. 22). He thought that British and Jewish propaganda was pushing the United States into the Second World War, and that Jewish interests in the United States, through influence in the press, radio, and motion pictures, were behind much prowar sentiment. He distrusted Roosevelt, believing him something of a dictator who was maneuvering the country into war. This view has since become standard among revisionist historians of the Second World War.

In his antiwar speechmaking Lindbergh encountered the usual restrictions that even a democracy places on unpopular dissenters. His protest over the difficulties placed in the way of the America First Committee when it sought to buy radio time to present its views has a contemporary ring. "It is a fine state of affairs," he properly complained, "if the question of war and peace cannot be debated before the American people because it is a 'controversial issue'!" (p. 294).

Lindbergh also reveals a sensitivity toward a foe that stands in contrast to his blindness to the brutality of Nazism. It is hypocritical, he rightly points out, to condemn the Japanese as cruel and barbaric, and then commit atrocities against them while maintaining the credo that America stands for "all that is 'good' and 'right' and civilized" (p. 880).

Despite such refreshing insights, Lindbergh's conservative views on international politics have not changed much in a quarter of a century. His anticommunism appears as rigid as ever. He laments the demise of the British and

French empires, implying that the temporary ascendancy of the West over the rest of the world could have been and should have been permanent. His is the attitude of a racial elitist, based on the assumption that the dominance of the Western "civilized" whites over the colored peoples of the world is a good and healthy thing.

Regardless of the rightness or wrongness of Lindbergh's views on world affairs, for political scientists and historians this book is a basic document for the study of conservatism and isolationism in twentieth-century America. Although the main parts of the picture it reveals are well known, it does give fresh details about the life and mind of a sincere, conservative isolationist, and on the inner workings, the tensions, and the problems of the America First Committee. It is most valuable, therefore, as a sourcebook.

ALEXANDER DECONDE

University of California, Santa Barbara

Grand Old Party: Political Structure in the Gilded Age, 1880-1896. By Robert D. Marcus (New York: Oxford University Press, 1971. Pp. 323. \$7.95.)

The Money Machines: The Breakdown and Reform of Governmental and Party Finance in the North, 1860-1920. By Clifton K. Yearley. (Albany: State University of New York Press, 1970. Pp. 377. \$12.00.)

Virtually everyone is agreed that the 1890's mark a basic watershed in American political history. But the question of precisely what changed and why is subject to much dispute. Dean Burnham has documented the sharp changes in political participation, and E. E. Schattschneider has shown the changes in political competition and public policy. It was in 1896 that McKinley and urban America decisively defeated Bryan and inaugurated a Republican era. The 1890's also saw the emergence of the "solid" Democratic South, based on final exclusion of Negro voters, and the emergence of the United States as a major figure in world diplomacy (and imperialism). After 1896, the House of Representatives began to display its distinctive modern pattern of high rates of incumbent reelection and consequent increased length of service of members. After the assassination of McKinley, the country could look to its first "modern" President and read of his exploits in the new mass journalism of the era. Only the Senate and Supreme Court seem to have escaped major change in the decade 1892-1902.

The two books reviewed here both deal with

change in the area of political parties, one emphasizing party organization and the other party finance. Robert D. Marcus set out to test the hypothesis of Robert Wiebe (who directed the dissertation from which the book emerged) that the late 19th century was an era of increasing bureaucratization, centralization, and "search for order." Marcus is disarmingly frank in announcing that at least for the Republican Party of 1880-1896, he found hardly a shred of evidence to support the hypothesis. Obviously the generalization may well hold for many sectors of American life—including the House and Senate—but *not* for national political parties (not even in the days of Mark Hanna, who was more the product of events than their master).

Fortunately, nothing is lost save a rather casual hypothesis. Marcus goes on to give us an excellent analysis, centered around the quadrennial conventions and elections, of the national GOP from 1880 to 1896. He writes exceedingly well and is obviously a master of his sources (primarily archival, though he is very much aware of recent quantitative political history). Perhaps to atone for the lost hypothesis, he closes with an extremely perceptive chapter on the changed role of presidential elections after 1896. This analysis involves some shrewd speculation on the reduced scope of parties and elections after 1896, such that *legitimation* of policy continued to be based on presidential elections, but most foreign and much domestic policy was effectively determined by various "private governments" (quasi-judicial commissions, pressure groups, and government bureaucracies). This hypothesis makes the 1896 realignment relevant to Gabriel Kolko's revisionist interpretation of the Progressive Era.

Clifton Yearley's *The Money Machines* tackles a more ambitious and much neglected subject—the relation of governmental and political party finance. The author has immersed himself deeply in economic, historical, and political materials for the period 1860-1920, but somehow he fails to pull the material together in any comprehensive or easily understood way. There is useful material here and there on the nature of middle-class "reform," the workings of local "machines," and competing theories of taxation. But the systematic view of the way in which parties—and local governments—went about raising funds, and the relations between parties and local governments somehow never jells. As the title might suggest, the author slips too easily into metaphor and never achieves a systemic overview.

Some scholars—one thinks of Dean Burnham and Grant McConnell—tend to write of

the politics of pre-1896 America as a sort of "golden age" of high participation and democratic groups. Neither Marcus nor Yearley concentrates on the sordid side, but they clearly do *not* portray the political scene of, say, 1874–1894 as any Garden of Eden. Indeed, a case could be made for viewing this period as an era of pathological politics. Partisanship was rampant, election administration was a joke, patronage and assessments were the rule, pension and mail fraud were widespread; and if participation was high, so was the investment in means—both fair and foul—for mobilizing both the faithful and the apathetic. Interestingly, the resulting powerful party organizations differed hardly at all when it came to public policy (other than returning Confederate battle flags). If bosses and machines had their latent functions, they also had manifest disadvantages.

The era of late 19th-century politics is not an easy one to make sense of, but it is important as the foundation from which our 20th-century politics has grown. Both of these books make modest contributions to that goal—Marcus by his balanced overview of party machinery, and Yearley by the range of his scattered sources. For a more complete understanding we will have to go beyond metaphors of "money machines" and pat phrases, or continue to hope that the facts will speak for themselves. They seldom do.

HUGH DOUGLAS PRICE

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Political Socialization and Educational Climates: A Study of Two School Districts. By Richard M. Merelman. (New York: Holt, Rinehart and Winston, Inc., 1971. Pp. 268. \$3.95.)

Unlike much research in political socialization, Merelman's is clearly and overtly policy oriented. In this book, he is mainly concerned with the school as an agent of political socialization (which he dimensionalizes as democratic attitudes, political knowledge, partisanship, and desire to participate in politics) and with "whether the aspects of education that occupy most of the community attention, such as the quality of instruction and the character of the teaching staff, have effects on the political socialization of students" (p. 8). The impact of these independent variables on political socialization is selected for study because they may be influenced by educational policy makers, and because they are highly "amenable to policy innovation or change" (p. 8).

The book is an important addition to the existing literature on political socialization: it cov-

ers a broad range of hypotheses, breaks new ground in its methodological approach, and explores the normative implications of its empirical findings. But in touching so many topics of interest, one cannot treat any single area of concern very thoroughly. Furthermore, innovative methodological approaches can sometimes be questioned because they are new and insufficiently explored. Finally, the normative implications of data are sometimes difficult to discern, because they are often dependent on ideological interpretation.

Sixth-, ninth- and twelfth-graders in two Los Angeles school districts filled out questionnaires measuring commitment to several dimensions of democratic norms (support for the vote, freedom of speech, majority rule, minority rights, sense of civic obligation, rules of the game, importance of elections, opportunity to enter politics, and political efficacy); as well as level of political knowledge, support for domestic liberalism, party identification, anticipated level of political activity; and such nonpolitical variables as participation in school organizations, mass media consumption, sex, social class, and school achievement. Merelman selected these two districts because, while they were similar in some respects, the wealthier one "supported education poorly" (p. 57) while the other was more committed to quality education (some examples of quality education are: greater financial outlay, smaller size of classes, more educational innovativeness). While a positive relationship between "district financial support for education, teacher quality, and the development of democratic values in students" (p. 21) was initially predicted, the results of the study surprisingly failed to confirm this hypothesis. The school that was more committed to quality did not produce "superior" political socialization of its students. It is possible that some of the demographic characteristics of the districts (for example the district committed to quality is 30 per cent Jewish and somewhat more politically liberal and Democratic than the other) confounded the manipulation of district commitment to quality education. In any case, the author is refreshingly open in pointing out both the strong points and the problems in his research design and analysis. He leaves his reader with few unanswered questions.

Merelman groups the data from his two districts together to analyze the effects of conditioning agents on dependent variables. He notes that "the party system exerts little attraction" (p. 88) and that "participation motivation disintegrates" as age increases (pp. 88–89). Low acceptance of "democracy" even in these two white middle-class school districts is not a rare

phenomenon. Several other studies of political socialization made after 1967 have reported similar findings. These results reconfirm the marked differences in political orientations noticeable between children studied in recent years and those polled by political socialization specialists in the late 1950s and early 1960s.

A sample of teachers from the two districts answered questions concerning their feelings about the profession, their career patterns, their level of morale, their satisfaction with teaching, their relationships to colleagues, students, and administrators, their self-evaluated performance in the classroom, their personal political attitudes, and so on. Few important differences were found on these variables between the teachers of the district which had a high commitment to quality education and the teachers of the district where quality education was a less important priority. This finding casts doubt on a district's differential ability to recruit superior staff even when community support and administrative commitment for such a goal is high. The cleavage was not between districts, but rather between teachers at different levels and in certain subject matter areas. Thus the secondary school teachers and the social studies teachers in both districts taken together were found to differ from the other teachers on many variables.

With respect to methodological innovation, instead of using a specific set of items to tap democratic structure, Merelman factor analyzes, for each grade level, a large pool of items in order to determine empirically what structure exists in the youngsters' response patterns. The results of this factor analysis are, in some cases, subsequently used to adjust which items will be used to construct the final scales of Support for Democracy. The disadvantage of this procedure is that while theoretically the final scales tap the same dimension, in some cases where required by the factor analysis, they are constructed of different items. This scaling technique may call into question comparisons between grades, because the same scale is constructed of different items at each grade level.

Merelman is also original in his attempt to link macro-level variables (district characteristics) with micro-level behavior and attitudes. It is difficult to evaluate definitively the appropriateness of this procedure. One is more willing to accept it when the macro-level variable has a clear impact on the individual student, e.g., in the case of size of class or district expenditure per student. But aggregate comparisons which are undertaken in order to make generalizations about individual behavior—for example,

the attempt to measure teacher influence on students—seem more dubious. Hess and Torney, in a similar analysis, try to determine if students come to share the political attitudes of their teachers. They use the students' own teachers as a reference point. Merelman, on the other hand, compares a sample of students, not with their own teachers, but with an independent random sample of teachers from the district (pp. 162–164, and p. 235). This is clearly a less powerful way to make such a comparison.

Merelman feels obligated to relate his empirical data to his normative concerns (p. 210). While normative decisions enter his research at many points, including operationalization of variables like democratic socialization, this commitment culminates in his concluding chapter with recommendations to psychologists, educators, and policy makers. These recommendations include such controversial measures as evaluations of teachers based on achievement tests given to students before and after they have studied with a particular teacher. Unfortunately some of the recommendations are not closely linked to the data reported in the book. For example, policy makers are told that more funds must be made available for public education. Yet the first part of the book shows that higher expenditures in one district failed to lead to superior performance on the part of its students and did not produce the markedly distinctive staff one would have expected.

In conclusion one must note Merelman's tremendous interdisciplinary command of the literature. He carefully relates to his own work pertinent research from psychology, sociology, and education. In addition, perhaps because of the largely negative results of the survey reported in this book, some of the most interesting and useful material is Merelman's description of the dynamics of education and teaching in a large U.S. city. His understanding of administrator and teacher motivations and his insights into district educational politics alone make this book well worth reading.

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Kennedy Justice. By Victor S. Navasky (N.Y.: Atheneum, 1971. Pp. xx, 482. \$10.00.)

It is difficult for the reader of the daily newspapers to get the sense that presidencies are actually about the politics of capturing—or learning to be captured by—the routines of government, and then making the government run. It is doubly difficult for the student of the Kennedy Presidency to get this sense, for reasons

that are well enough known. We can be grateful to Victor Navasky for not being so beguiled by the Kennedy glamor as to have produced yet another misleading and redundant tribute to Camelot. Instead he has given us a detailed chronicle and serious assessment of the way in which Robert Kennedy operated the Justice Department—the best discussion I know of that deals with national politics from the perspective of a cabinet officer.

Navasky's study is generally instructive precisely because, as he points out, the Kennedy Attorney Generalship must constitute a limiting case; surely no head of a department has ever enjoyed better or more assured access to and support from a President than Robert Kennedy did from his brother. Thus Kennedy was, as Navasky says, the maximum Attorney General, at least so far as constraints and difficulties from above are concerned.

Yet, as we rapidly discover, this is by no means enough to assure maximum performance on all fronts. Rather than a clearcut hierarchical Justice Department, in which the maximum Attorney General automatically gets the outcomes he wants, Navasky (courtesy of Max Weber) proposes the metaphor of a Justice Department which embodies an interaction among three giant forces: the code of the Kennedys, legitimized by charisma, activist, result-oriented, red-tape cutting, and loyal to persons; the code of the Ivy League Gentleman, embodied in Kennedy's top appointees (Burke Marshall, Nicholas Katzenbach, Byron White, Louis Oberdorfer, *et. al.*), legitimized by tradition, proceduralist, oriented to civil liberties, limited government, due process, federalism, and letting the chips fall; and the code of the FBI, legitimized by the existence of files, secretive, routine-oriented, self-protective, and loyal to habit.

The first and in some respects the most significant part of the book describes the way in which Robert Kennedy came to terms with J. Edgar Hoover, the maximum bureau chief. Since the FBI employs almost half of the people in the Justice Department, the nature of this bargain was bound to have enormous impact upon what Kennedy and his appointees could do in their time in office. Navasky concludes that the FBI imposed considerable constraint upon Kennedy Justice, especially in the field of civil rights, where the Bureau was not quick to adapt its priorities to those of the Attorney General. Navasky suggests one highly plausible reason for Bureau-Department tension in this area. Increasingly in the early 1960s, leaders of the Department came to be-

lieve that it was necessary to ride herd on local law enforcement agencies throughout the south in order to provide minimal levels of protection to civil rights workers. Navasky surmises that part of the FBI's reluctance to participate in this effort arose from long-standing ties between the Bureau and local law enforcement agencies. To disrupt these ties in order to meet an extraordinary situation might have jeopardized the ongoing routines in which the Bureau was productively enmeshed with local people.

The identification, examination and diagnosis of dilemmas such as these are central to at least one branch of political science. Navasky shows how and why Attorney General Kennedy challenged, circumvented, and gave hostages to the FBI and to J. Edgar Hoover. This discussion contributes helpfully to our understanding, otherwise seriously deficient, of the politics of running the great departments of our national government.

Other sections of *Kennedy Justice* take up other facets of the Attorney General's world. Herewith, a small sample: A long section on federalism, slightly marred by an excess of second-guessing by Navasky, explores the role of the Justice Department in the integration of southern universities. Here, Navasky argues that the code of the Ivy League Gentleman, and the theory of federalism to which it gave rise, hemmed Kennedy in too much. Another chapter argues that the eagerness of the American Bar Association to legitimize its participation in the selection of federal judges, coupled with serious lapses in the Justice Department's attentiveness to the credentials of nominees, produced some very bad judicial appointments in the South. Navasky only takes up in detail, however, the handful of notorious cases. His legal training (though a practicing journalist, Navasky is a graduate of Yale Law School) might have led him to a more ambitious and more enlightening task: examining the performance of a larger sample of Kennedy judicial appointees in order to see if the Justice Department's alleged pattern of semi-indifference to the quality of the bench was sustained in areas other than the South, and for issues other than segregation of public facilities.

While Attorney General Kennedy's light touch with his bureaus is held to have been relatively ineffective in dealing with J. Edgar Hoover, Navasky thinks Kennedy struck just the right note with Archibald Cox, the distinguished and independent Solicitor General. Navasky gives a detailed and intriguing account of the complex negotiations in and around the Solicitor General's office surrounding the decision

to file an *amicus* brief for the government in one of the reapportionment cases. My view is that he is a little too sure that the outcome was the right one.

The *pièce de résistance* of the book is the last chapter, on the campaign to get James Hoffa, showing Kennedy Justice at its least inhibited, as the early chapters show it at its most inhibited. Navasky raises some proper questions about this project, and I think quite eloquently puts the case for a more constrained Attorney General to counterpoise the case he makes earlier, in regard to civil rights, for a less constrained one.

This is not a flawless book. It is a bit repetitious, and once in a while a bit smug. I found a few small inaccuracies in it. On the whole, however, I think *Kennedy Justice* is thoughtfully, artfully, and carefully done. There are a good many lessons in it for students of the craft of governing. Some of these are hard lessons, since they force us to consider what we can reasonably expect from leaders of government less advantageously placed than the maximum Attorney General.

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The Texas Political System. By Dan Nimmo and William E. Oden. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. 166. \$5.95.)

Texas Politics: An Introduction. By James Anderson, Richard Murray and Edward Farley. (New York: Harper & Row, Publishers, Inc., 1971. Pp. 327. \$3.95.)

Whether the slim volume by Nimmo and Oden finds favor with today's political scientist may depend upon whether the reader regards systems analysis as *the legitimate* frame of reference. This book is assertedly "the first application of a systems approach to the study of any state government" (blurb). The supports and demands of Texas politics (inputs) and the conversion of stress-producing demands into authoritative allocations of benefits and deprivations (outputs) are to be identified and compared with input-output data from other state political systems in the United States. Systems analysis functions as a fairly clearcut paradigmatic framework which facilitates the perception of political phenomena. The emphasis is on identification and analysis of the uniformities of data and functions. Systems analysis performs very well the duty of identification, but it is not yet clear that it facilitates superior handling of problems such as measurement, causal

inference, and policy determination, or that it informs about situations and behaviors in a manner which contributes to accuracy in prediction or perception in postdiction.

Nimmo and Oden tell us that beneath the formalities of Texas participatory democracy lies not so much a pluralistic democracy as an elitist reality. They agree with Daniel J. Elazar, *American Federalism*, that the Texas political culture is an amalgam of traditionalistic (a paternalistic and elitist conception of government) and individualistic (government perceived as a marketplace in which policies emerge from the bargaining of individuals and groups acting out of self-interest) rather than a *moralistic* political culture (in which government is perceived as existing in order to promote and maintain a shared public interest—the good society).

Where in this culture do political *demands* arise? According to Nimmo and Oden, they come from strong interest groups which are in the main conservative and grounded in the business community: the oil industry, natural gas, beer, railroads, insurance, trucking, consumer-finance, banking, the Farm Bureau, the engineering lobby, and certain religious bodies. The less prosperous are more "subjects" than "citizens."

Political power resides with a set of loose and shifting coalitions among pressure groups representing the affluent. These groups and their lobbyists grew up with Texas. Economic development preceded or accompanied development of the political structure and shaped that development in its own image. The Texas Constitution of 1876 insulates the decision-makers from popular control. The majority of the adult population are acquiescent or indifferent. The pressure system, say the authors, is the least studied and least understood of all Texas political subsystems, yet it is the "prime filter through which all demands to which policy makers respond must pass" (p. 105).

The authors devote little attention to the often bloody election battles between conservatives and liberals, or to the colorful personalities and political high jinks often featured in these donnybrooks. This neglect is obviously dictated by the emphasis on comparative systems analysis and by the brevity of the work. I cannot resist suggesting, however, that in Texas the demands and supports mobilized by the efforts of tough-skinned conservatives and tenderhearted liberals owe something to individual leadership capacities which deserve attention.

More crucially, the authors apparently conclude that elections and candidates are rather

irrelevant in Texas (except perhaps for the legislature) because pressure groups representing the upper economic classes dominate state policies. They also emphasize that the power of the governor is relatively slight. This may be so, but the authors need more evidence to support their findings than is offered in this book. This omission is not entirely the fault of Nimmo and Oden. They point to the dearth of basic data on Texas politics. Lamentably absent are studies on pressure groups and policies, legislative roll calls, identification of the demographic profiles of diverse voting blocs, licensing and other policies of bureaus and commissions, and other important political reference points.

The sketchy outline of the political policy picture in Texas which emerges here is intriguing, but the limitations impose upon the work an exceptionally tentative quality. Unfortunately, there are simply too few conclusions which are not hedged in with qualifications as to the inadequacy of supporting evidence to permit confident comparisons with other states. What is found here is a silhouette, made elegant by tasteful style and by what the authors call a "sophisticated tool of social inquiry."

In sharp contrast to the Nimmo and Oden work, the book by Anderson, Murray, and Farley appears to be typical of the James Bryce emphasis on "facts, facts, facts." The authors' preface affirms a desire to avoid discussion of "reform" and to minimize considerations of "policy." The authors describe in legalistic and structural terms the constitution, administration, judiciary, and local governments, and they use a situational-behavioral framework to discuss parties, voting, elections, interest groups, the legislature, the governor, the courts. A final chapter details the sources and amounts of Texas revenues and the budgetary allocations.

The volume employs quantitative measures in indicating population changes, voting proportions, political party patterns, and the use of the gubernatorial veto. On other occasions the style of the narrative is marked by the use of illustrative examples. Perhaps the best stylistic feature of the volume and its nearest approach to an overview is found in the series of cartoons. My favorite (evidently the publishers liked it too, because it is reproduced on the back cover) shows a bald, potbellied, cigar-smoking individual in conversation with another man. "Jim," says one of them, "suppose the people get a lobby?"

In one respect Anderson, Murray, and Farley offer evidence which amounts to a corrective not only of their own generalization but of one subscribed to by Nimmo and Oden as well

—the weakness of the governor. While affirming that he is a weak chief executive relative to the powers of most other state governors, the evidence strongly suggests that the Texas governor exerts a clear and frequent influence on policy, including legislative policy. Evidence shows that the prestige of the office and its occupant reinforces the very tangible power of the veto, including the power of the item veto on appropriations bills. The governor also frequently uses the power to call special sessions of the legislature. In a special session the legislature is constitutionally required to confine its attention to the subject matter specified in the call for the session (i.e., by the Governor). The importance of special sessions is enhanced because the legislature is constitutionally required to meet biennially. Demands of policy make necessary more frequent sessions.

In general I have been struck by the frequent appropriateness with which the data contained in this book might be used to support several major conclusions of the work by Nimmo and Oden. On the other hand, Anderson, Murray, and Farley avoid making these generalizations. Perhaps the confining modesty of their design (for an introductory level book) served as a kind of self-fulfilling prophecy.

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Executives in the American Political System.

By John C. Ries. (Belmont, California: Dickenson Publishing Company, Inc., 1969. Pp. 151. \$2.95, paper.)

Professor John C. Ries has begun the much needed and difficult task of building a theoretical framework within which the behavior of executives within the American political system may be studied, analyzed, and compared. Ries argues that the literature of the Presidency, governors, and local executives, although frequently rich in detail, adds up to no meaningful whole and that we lack an integrating framework with which to relate executives to political systems. Ries attempts the beginnings of a difficult integration by using relevant concepts from functional-structural analysis, group theory, organizational theory, and role theory. He constructs his framework by (1) identifying and analyzing the principal functions of the executive's role and the key elements within the political system which appear to affect and to structure that role; (2) developing a matrix of political subsystems within which executives may find themselves and evaluating the appropriateness of various executive actions within

each subsystem; and (3) identifying and interpreting the factors likely to produce transformations in the executive's role.

Ries states that the primary task of executives at all levels in the American political system is to maintain the system's dynamic equilibrium. To do this, executives must deal with stable demands which continue regardless of the incumbent, changing demands which flow from shifts in political ideology, advances in technology, situational factors such as natural disasters, new participants in the political system, and their own political ideologies and personalities.

The key elements which affect and structure the executive's capacity to perform this task are as follows: (1) the universal functions which the political systems must perform; (2) the political culture which underlies the distribution of power, the scope of governmental action, and the behavior of public officials; (3) the political structure through which power is distributed; (4) the groups within the political system which interact and contend with each other in order to achieve their goals; (5) the diverse outputs of governmental agencies at all levels; and (6) the political resources of the executive.

The core of Ries's work appears to be the matrix of subsystems within which most executives at the local and state governmental levels operate. Drawing upon the elements of the political system developed carefully throughout the first half of his book, Ries constructs his matrix using three major components: (1) performance categories which specify the principal claims made upon governments; (2) power structures which specify the distribution of power among leaders and followers and the degree of convergence of values among leaders; and (3) the executive's political resources. From this, Ries constructs fourteen subsystems and argues that within each subsystem, executives may be classified as weak or strong. Given these subsystems and the resources of the executives within them, it is possible to account for much of an executive's behavior and its appropriateness. It is the system rather than the executive which is the primary determinant, and system role analysis becomes the most useful approach for interpreting executive behavior. Ries summarizes this approach by grouping his fourteen subsystems into seven sets of subsystems and examining each in terms of its frequency, associated policy scopes, decision-making style, political acts, executive resources, and system roles. The system roles include (1) manager—applying skill and expertise; (2)

budget-cutter; (3) broker—serving as a go-between; (4) negotiator—participating in the outcome; (5) figurehead—being manipulated by others; and (6) mobilizer—bringing new participants into the process.

Ries does not attempt to place the Presidency within his subsystem matrix because of the lack of knowledge about the distribution of power within the national political system and differences in policy scopes among the national and the state and local governmental levels. He does, however, attempt to identify the roles generally available to the President and concludes that because of the existence of coalitions which seek to use the President, the dominant roles are figurehead and broker and that a negotiator role exists as a derivation of these two dominant roles. The technical nature of national policy scopes also creates a managerial role, and a conservative coalition imposes a budget-cutting role. Except for a "honeymoon" period following a lopsided electoral victory, the mobilizer role is virtually denied the President because of the existence of powerful coalitions which would be threatened by bringing new participants into the political arena.

The executive's role may be changed or transformed by (1) systemic factors which produce changes in values and power distributions within the system; (2) situational factors which are unpredictable and have consequences for political systems; and (3) the personality of the incumbent. Of the three, the least seems to be known about personality, which may be one of the greatest forces for innovation. Executives then operate at a focal point which forces them to contribute to the stability of the political system but also enables them to influence the rate of and direction of innovation within the system. Actual role transformation, however, is seldom dramatic and typically incremental, because stability throughout the political system is normally functional for most participants.

In attempting to construct a framework within which the role of the executive can be interpreted, the author has been forced to handle a wide variety of social science concepts and deal with virtually the total political system. This is an enormous task, but there seems no realistic alternative if we are to develop a more systematic understanding of the American executive and his role. Professor Ries's work has provided us with an imaginative framework for interpreting existing knowledge about the executive and a basis for additional theory building and empirical research.

LLOYD A. ROWE

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What Price Vigilance? The Burdens of National Defense. By Bruce M. Russett. (New Haven and London: Yale University Press, 1970. Pp. xii, 261. \$7.50.)

This admirable work is modern social science at its best. It is concerned with a problem of enormous importance on which may depend the whole future of mankind. This is the problem of what I have called the "war industry," that is, that segment of the activity of society which produces what is purchased with national military expenditures. The book makes no pretense at an exhaustive treatment of the subject: it concentrates heavily on what perhaps is the most significant aspect—what determines the overall size of the military sector? There is only incidental discussion of how, within this overall expenditure, resources are allocated to different forces, different weapons, and so on. The approach is quantitative and statistical, using for the most part, however, the relatively simple techniques of rank-order correlation. It uses these techniques with modesty and restraint, and the author is fully aware of their limitations. One does, however, have a sense of the frontier of knowledge being pushed forward, and even the little advance into the enormous complexity of the social system is very cheering.

The work opens with a general discussion of the problem. Military expenditures in the United States have not only exhibited a "ratchet effect," in the sense that after every war they do not return to their previous relative level, but since the Second World War they have remained at a level and order of magnitude that is ten or twenty times greater, as a proportion of the economy, than what was regarded as "normal" in all previous American history. This is a major quantitative change in American society with enormous consequences not only for American citizens but for the whole world. The author puts forward tentative hypotheses to account for this momentous change.

The second chapter analyzes the roll call votes in the Senate in two years—1961–62 and 1967–68, with a scaling technique designed to create statistically emergent, rather than previously imposed, categories relevant to national defense. Three of these categories emerge for the earlier year and four for the later. In both years, however, the major category is National Defense itself. What comes out is a fairly clear hawk-dove spectrum of both senates, which is however not very closely related to other clusters of voting behavior, such as aerospace, gun control, or even arms

sales abroad. A few maps would have enlivened this chapter greatly. In fact, I ventured to draw one myself. It shows up what I have called the "infertile crescent," from the point of view of the doves—the great crescent of hawkishness running through the mountains and Great Plains states to the South, from North Dakota through Wyoming, Colorado, and Texas, to South Carolina.

The third chapter relates defense expenditure by states to the corresponding senatorial voting behavior. The relation is quite low, with the conclusion that on the whole hawkishness is not much related to narrow economic considerations, but rather to general ideological viewpoint. There is no relation at all between hawkishness and defense contracts by states, mainly because most defense contracts go to the dovish states of the Northeast, Middle West, and Far West. There is some relationship, however, with military personnel by states, suggesting that the "permanent armed forces" are more important politically than are fly-by-night defense contracts. There are very interesting suggestions, not followed up much, about tradeoffs between the defense expenditure and civil rights.

The fourth chapter is an attempt to apply the theory of public goods to the problem of alliances. The theory of public goods, as developed by Samuelson, and especially Mancur Olson and Zeckhauser, suggests that the rich will usually end up paying a larger proportion of their income for public goods than the poor will. In the case of alliances, this principle would be reflected in a positive relationship between the GNP and the proportion of the GNP devoted to defense. For NATO and, in later years, for the Warsaw Pact, this hypothesis works out fairly well, for the Latin American countries not too well, and for the Arab League not at all. These seem like interesting and significant conclusions, which would not emerge without the quantitative and theoretical analysis.

The fifth chapter on "The Opportunity Costs of American Defense" is a careful economic analysis of what goes down when military expenditure goes up, what goes up when military expenditure goes down. The method here is simple correlation using various proportions of the GNP as the variable. The conclusions are interesting; for instance, education emerges as an important victim of defense. The methodology is somewhat open to criticism. Using the GNP as a base neglects unemployment, which could be taken care of if the Gross Capacity Product rather than the Gross National Product were used as the "100 per cent." The influ-

ence of the Second World War, also, is so large that it tends to distort the correlations and, though this is recognized sometimes by making the correlations nonlinear, it might have been better to treat the period after the Second World War separately. The next chapter extends this analysis to some other countries, particularly Canada, France, and Britain, with useful comparative results. The work concludes with a brief chapter on the military in American life, which, however, is sketchy and not up to the quality of the rest of the book. There is an appendix by Peter A. Busch on "Mathematical Models of Arms Races" which is interesting in itself, but perhaps would have been better published elsewhere. As this reviewer's work is not even mentioned in it, it is hard for him not to feel that the work is incomplete.

One general criticism might be made of the theoretical framework of the book. It assumes implicitly that the international system is in the phase of "unstable peace," as I have called it (in "Research on Peace," *Science Journal* [England] 5A [October, 1969] 53-59); and there is no suggestion that part of the changes, in the 1960s especially, may be the result of the system's moving in the direction of the phase boundary that leads into stable peace. Consequently, there is no treatment of what might be called the "integrative aspects" of the international system, the slow growth or decline of which has a great deal to do with creating the framework for the system of arms races and deterrence. It can well be argued, however, that this topic is well beyond the scope of the book, and this criticism in no way diminishes the substantial accomplishment of this work within its limited field. One hopes the author will go on to fresh triumphs.

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The Political Imperative: The Corporate Character of Unions. By Gus Tyler. (New York: The Macmillan Company, 1969. Pp. 294. \$7.95.)

The Labor Revolution. By Gus Tyler (New York: The Viking Press, 1967. Pp. 271. \$6.50.)

Gus Tyler, a foremost student of American labor, is a highly successful union leader who has served as Assistant President and Education Director of the International Ladies Garment Workers' Union. In both *The Political Imperative* and *The Labor Revolution*, Tyler marshals a lifetime of experience as a participant observer. His wide-ranging concern is in-

deed refreshing in this era of overspecialization. While both books are written for the general student of organized labor, each contains much of specific interest for political scientists.

In *The Political Imperative* Tyler tells us that he relies not only on his three decades of experience in the labor movement but also draws upon anthropology, ethology, theology, biology, economics, history, psychology, sociology, and cooperative life. He is true to his word; moreover, he also makes use of jurisprudence, semantics, genetics, child behavior, and philosophy in his analysis.

At the outset Tyler posits his main concerns:

First, will there continue to be a labor movement? . . . what is the role of organized labor in government? Finally, what are the aspirations and ambitions of trade union leaders—democrats or bureaucrats? (p. 1)

The chapter on "Universality of Unions" represents a trenchant historical analysis. Tyler adduces strong evidence to indicate that, ". . . employees, like slaves—are among the last to organize. And among workers, the very poorest and the most illiterate are usually the least likely to organize" (p. 39).

The student of American political parties will find a first-rate analysis of the broker-mediator function of American political parties. Tyler fully understands both the nature of coalition politics and the decentralized operation of American political parties. Furthermore, he provides perceptive insight into the operations of labor politics in New York State. There is a limitation, however. In supporting the theme of nonpartisan politics, Tyler seems to underrate both labor's strong attachments for the Democratic party and its outright hostilities for the Republican party outside of New York state. He overlooks industrial states like Indiana, where legislative votes on the controversial "right-to-work" law always found more than 90 per cent of the Republicans lining up for "right to work," while a like number of Democrats took an opposite stance. Tyler does not demonstrate that his analysis is applicable outside of New York state.

One marvels that a man who can perceptively apply Federalist #10 to his analysis of interest groups, can fail to mention *The American Voter*, *Voting*, or the other landmark works when analyzing union-member voting behavior. Nonetheless, Tyler offers remarkably accurate conclusions based apparently upon a lifetime in labor politics rather than upon the data reported in the voting studies.

In his final chapter, Tyler addresses himself

to a significant problem of democratic theory—striking a balance between the liberties of individual members and the collective requirements of the union. He incisively analyzes the differences and similarities between governmental and various nongovernmental institutions. In addition, he ably discusses the problems of union democracy and calls for a union leadership with the power to lead—that is, to represent the group as well as to protect it. He also attacks those leaders who forget that sovereignty ultimately belongs to the members and argues that too often the leaders equate personal good with the good of the membership. More significant than his actual conclusion is the careful manner in which Tyler examines the case both for and against union democracy.

The book does have a few minor shortcomings—reference to a questionable “herd instinct” and an occasional misplaced analogy (business and labor as siblings engaged in rivalry for the respect of the parent—the social order). Nevertheless, on balance *The Political Imperative* is well worth reading.

The Labor Revolution is a highly optimistic book. Although many are concerned that organized labor can at best engage in a holding action, Tyler forecasts a highly roseate future. The optimism rests upon a number of factors. First, while recognizing that the idealism of the 1930's which held sway in the Northeast and the Midwest is no longer present, he sees the South and the Southwest as the battlegrounds of the 1970's.

Tyler states:

The South is a twentieth-century fact, resting on a nineteenth-century economy, ruled by an eighteenth-century aristocracy . . . three developments will be of major importance to the labor movement. First, industrialization will provide the traditional base for unionization. Second, the Southern Negro, responding to his new self-image, is becoming active as a unionist and as a voter. Third, these factors, plus the trend in federal law and national opinion, are liberalizing Southern politics (pp. 10–11).

The general triumph of moderate southern candidates in 1970 certainly constitutes a liberalization of Southern politics. Whether this represents a trend or an aberration remains to be seen, since Negroes and labor are still grappling to achieve more than a tenuous coalition.

Another area of Tyler's optimism involves an alliance between young idealists and organized labor. Although the young people who worked for Eugene McCarthy had not fully blossomed when this book was written, the data fail to in-

dicate that they found common cause with labor as did their counterparts of the 1930's. My own surveys of students working for Eugene McCarthy in 1968 actually indicated their strong antipathy to organized labor—more intense even than their antagonism toward American business. The situation still holds. Activist youth apparently will not provide the infusion of idealistic leadership that Tyler seeks.

Tyler also presents a concise but effective history of the labor movement, an astute analysis of featherbedding as a cultural problem not unique to unions, and a discussion of automation and unemployment.

His objectivity is most open to question in the chapter on Negroes and the labor movement. Characterizing George Meany and the old AFL as champions of the Negro seems contrary to the exclusionary policies many building crafts have continuously followed. That labor may have finally come around to verbalizing a pro-civil rights position does not blur the reality of discriminatory practices that still pervade many individual unions.

Tyler believes that the rural legislator is the *bête noire* of progressive policies, and if his overrepresented strength is diminished, liberal legislation will likely take place. As a political realist, Tyler knows that in many instances reapportionment will bring an increase in the strength not of the inner city but of suburbia. He argues, however, that the suburbanite ought to be joined with his urban counterpart since both share the problems of water and air pollution, transportation, housing, and integration. He introduces little evidence, however, to convince us empirically that conservative suburbia will actually start electing activist liberals to the various state houses.

If Tyler can be faulted for being unrealistic on reapportionment, he can be lauded for his understanding of the political system in general. He demonstrates a high degree of political pragmatism in demonstrating the unfeasibility of labor's forming a third party.

The concluding chapter, “Politics in Labor,” provides insightful analyses of the conflict between the intellectual and the practitioner. Although Michels originally dealt with this problem, Tyler places it in its appropriate modern American setting. His insights on James Hoffa are also worth reading, as is his plea for more effective education within organized labor. He argues cogently that labor can strengthen its case with the public as well as develop competent leadership by emphasizing meaningful educational programs.

All students of labor politics should read this

book for its intellectual breadth, its predominant fairness, and its excellence of style.

MELVIN A. KAHN

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The Impact of the United States Supreme Court: Some Perspectives. By Stephen L. Wasby. (Homewood, Ill.: The Dorsey Press, 1970. Pp. 305. \$4.25.)

Since the celebrated case of *Marbury v. Madison*, decisions of the U.S. Supreme Court have produced lively discussion, vigorous controversy, and critical examination. Though scholars (and laymen) have engaged in doctrinal analysis and plotted doctrinal trends of court decisions, they have paid little attention to the impact of court decisions on the political process. For the most part, research efforts at systematic examination of the consequences of Supreme Court decisions have occurred only within the last two decades. Many of these efforts were sparked by precedent-shattering decisions of the 1950's and 1960's in such controversial areas as racial discrimination, church-state relations, and criminal law enforcement. These court actions, and the reactions to them, provided the raw material for accelerated interest and study of the Supreme Court from a political perspective. (See, for example, Theodore Becker's *The Impact of Supreme Court Decisions*, Oxford University Press, 1969.)

The volume under review here may well be characterized as a kind of "pause at the way-station" toward the development of "impact" theory. Professor Wasby's purpose is "to pull together and begin to integrate the extant materials on the impact of the United States Supreme Court" (p. vii) and to examine critically what has been done. As a result, he has produced a fairly readable and useful guide to future research in this area.

The volume is divided into two parts—the first focuses on questions essential to the development of a "theory of impact," and the other examines the impact of the Court's decisions in several substantive areas and political arenas. After an initial look at some examples of impact (attempts to overturn, evade, or comply with decisions; consequences for the immediate parties; effect on political and bureaucratic activity, etc.), Wasby strengthens the case for regarding the judicial system and more particularly the Supreme Court from a political perspective. Wasby suggests that such a perspective, as contradistinguished from the traditional constitutional-legal perspective, is essential to theory building in the area of the Court's impact.

Three other important problems examined initially are the conceptualization, the measurement, and the identification of relevant variables and their effect upon impact. Here Wasby considers the "boundaries" of such concepts as impact, compliance, noncompliance, effect, aftermath, and evasion. In doing so, he maintains that some of the difficulties in utilizing or "operationalizing" these concepts have led to a search for other concepts through which more meaningful analysis may result. Furthermore, he notes that measuring and isolating the impact of a decision presents some difficult questions. For example, is one to measure impact only by visible activity? Can impact be isolated as a causal factor? Does one measure merely the number affected by a decision? What time span should be considered?

Among the variables examined are those that relate to "characteristics of the case itself," such as the "closeness of the vote," the existence of a majority opinion or a plurality opinion, the statutory or constitutional grounding of the decision, and the degree of "completeness" or "ambiguity" in the disposition of the questions raised. Others relate to environmental factors, characteristics of particular decisions, followup efforts, community characteristics, and social-psychological factors.

Wasby concludes the first part of his volume with an examination of three frames of reference that might be utilized in the study of impact. They are: 1) the constitutional-legal framework, which emphasizes the courts as legal institutions operating through fixed rules and processes; 2) the political framework, which focuses on the activities of interest groups and emphasizes viewing the Supreme Court as a part of the political process; and 3) the framework of communications processes, which involves looking at the Supreme Court as a part of the communications network in which messages are transmitted to "attentive publics" and are reacted to. Because of their inherent limitations, each of these frames of reference contributes varying perspectives and brings both gains and losses for the researcher.

In the second part of the volume Professor Wasby applies his theoretical perspectives to several substantive issue areas and governmental and political arenas. Considering such issues as legislative reapportionment, obscenity, church-state relations, public school desegregation, criminal procedure, and economic legislation, he clearly illuminates for each issue the Supreme Court action taken and the consequences that have followed. Like many others, however, Wasby is lured into a disproportion-

ate concentration on the "hot" issues of the day—those involving civil liberties.

The political arenas on which he focuses attention include the Congress, the President, the federal bureaucracy, the lower courts, and state officials. Using several Supreme Court decisions, he illustrates the kinds of reactions and responses from these arenas. For example, were there Congressional attempts to amend the Constitution or to strip the Court of its appellate jurisdiction in specific areas, or did a state supreme court refuse to carry out the mandate?

Much of what he writes in Part II is descriptive and somewhat repetitive of materials presented in Part I or in the work of others. Literature chronicling what happened after the Court's decisions in each of the areas considered is plentiful in the work of journalists, lawyers, and social scientists. But what is most valuable in Wasby's work, and what distinguishes it from that of many others, is his emphasis on the impact of a decision on the actors in the various political arenas. Of particular merit is the attention Wasby has given to the lower courts—federal and state. In doing this, Wasby reflects an increasing effort among political scientists to overcome the "upper-court myth." He also suggests the need for greater attention in impact research to the role of state legislative and executive agencies.

The most important contribution of Part II, and perhaps of the entire volume, is the presentation of a series of hypotheses about where we are in the development of an impact theory and how our research must proceed. Wasby, properly cautious, warns that some hypotheses are not yet "tightly operationalized" and "lack precise delineation." But given the gaps in our knowledge of impact, testing these hypotheses empirically would be an excellent starting point.

TWILEY W. BARKER

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Federal Grants-in-Aid: Perspectives and Alternatives. By Deil S. Wright. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1968. Pp. 158. \$2.00.)

Much has been written in recent years on the topic of fiscal federalism. Libraries are replete with historical reviews (Elazar, Mosher), normative prognostications (Heller, Reuss), empirical inquiries (Sharkansky, Pogue), as well as some beginning efforts at comparative analysis (Break and May). But what is available for the beginning student of fiscal federalism, or one with a casual or peripheral interest in the subject? Here, with the exception of overview

chapters in a few textbooks, one finds a void. Deil S. Wright's little monograph offers considerable relief for this situation. His stated purpose is "to review the several systematic studies that exist in the field, to assess the overall effects of grant programs on what today is called intergovernmental relations, and to offer some thoughts about possible reforms" (p. 1). *Federal Grants-in-Aid* does this and more.

The utility of this work clearly depends on the extent of one's prior knowledge of the grant-in-aid system. The serious student will find little that is original in the chapters which outline the definition, legal precedents, historical development, and causal basis for today's grant-in-aid program. For the novice, these chapters set the debate about future adjustments in the American fiscal system in perspective.

Relying on the 1958 House report on the grant program (by the Fountain Committee), the author gives a careful, comprehensive listing of the philosophical, legal, fiscal, and administrative advantages and disadvantages of the present grant system. One wonders, however, if the Committee Report has not become somewhat dated, given the changes that have occurred since 1957 in the grant-in-aid programs, both in terms of the number of grants and the total amount of money involved in intergovernmental transfers. This period of great growth and change for the program has focused greater academic and public attention on the subject of intergovernmental finances and has also intensified the hopes and fears of many observers far beyond that evident in the testimony of fourteen years ago. Unfortunately, the data are generally interpreted as if very little change had occurred since the 1957 hearings. The research of this reviewer (as well as that of Derthick, Pogue, and Sundquist), suggests that a very different view of the grant-in-aid program would emerge if the Committee hearings were reconvened.

Even with this limitation, Professor Wright's analysis of the grant-in-aid program from the perspectives of the national, state, and local participants is the most useful part of the book. The analysis is based on data collected from a multiplicity of sources: the author's own roll-call analysis of the 86th and 87th Congresses; the 1957 House Subcommittee on Intergovernmental Relations' report, *Replies From State and Local Governments to Questionnaire on Intergovernmental Relations*; the Senate Subcommittee on Intergovernmental Relations' reports, *The Federal System as Seen by State and Local Officials* (1963) and *The Federal System*

as Seen by Federal Aid Officials (1965); earlier studies by the National Municipal League and the Council of State Governments; and several academic reports. While these accounts present real problems of incomparability of data, low response rates (eight per cent for part of the 1963 survey), and time-boundedness, the reader receives an interesting picture of fiscal federalism in operation. Illustrative is the discussion of the resistance to change by federal grant administrators because of what is termed recurring perceptual themes of "functionalism," "professionalism," "protectionism," and general "indifference"; or the somewhat surprising preoccupation of administrators with program politics; or the amazingly frequent points of communality among state and local officials.

The role of the states is examined with a rather traditional listing of the pluses and minuses that will determine their future. Not surprisingly, the author comes to the increasingly accepted optimistic projection for the middle tier of our federal system: "Our discussion of the debit and credit side of the ledger does suggest that the criticisms of state government have been overstated and the positive features have been understated. In other words, the states are stronger than people think" (p. 127). The author's intimate knowledge of state government notwithstanding, other writers, myself included, would argue that except perhaps for a select few of the states, such optimism is quite unfounded.

The monograph concludes with a summary of the methods of improving the administration of the current grant-in-aid system and suggested alternatives to existing distribution techniques. Included in the latter discussion are direct federal expenditures, federal tax reduction, tax credits, and tax sharing—more specifically, the Heller revenue sharing plan.

A review of the pros and cons of these alternatives leads the author to conclude that "revenue sharing comes off best of all the possible alternatives to grants-in-aid, with the unique tax credit proposal of the Advisory Commission running second" (p. 150). While the last two years have witnessed the emergence of new sophisticated and relevant arguments against revenue sharing, the author does give a reasonably balanced hearing to the debate as it was put forth during the 1964–68 period.

In conclusion, while some of the author's prescriptive and projective observations may be challenged, and, by the author's own admission, numerous data problems are unresolved, *Federal Grants-in-Aid* makes a real contribution

for the beginning student of fiscal federalism. It also contains a few meaningful insights for the more mature student as well. It is a work that gives a balance to the literature of fiscal federalism; as such, it is highly recommended.

PARRIS N. GLENDENING

University of Maryland

African Perspectives: Papers in the History, Politics, and Economics of Africa Presented to Thomas Hodgkin. Edited by Christopher Allen and R. W. Johnson. (Cambridge: Cambridge University Press, 1970. Pp. 439. \$17.50.)

The seventeen essays in this volume presented to Thomas Hodgkin on his sixtieth birthday are a fitting tribute to his pioneering work in African Studies and his influence on several generations of Africanists. His first book, *Nationalism in Colonial Africa* (1956), quickly became a classic, while his *African Political Parties* (1961) still remains the most useful global analysis of African party structures before the era of one-party states and military regimes. As an historian Hodgkin was also one of the first Europeans to present Africa from an African rather than a colonial perspective.

Generally of higher quality than those found in similar collections, the essays are scholarly, concrete, analytical, readable, and committed—traits which characterize Hodgkin's own work. Despite the variety of topics covered, the volume is cohesive because it reflects much of Hodgkin's main interests and concerns. Part I (Historical Perspectives) opens with four pieces on the Islamic factor in West Africa, followed by five essays on diverse religious responses to the colonial situation. Part II (Political and Economic Perspectives) has eight essays: the first four are on political science and political economy, while the last four are case studies in political change.

The first paper by Hunwick and the second paper by Wilks and Ferguson are best left to historians and Islamists for comment. Although aimed at historians, Jeff Holden's methodological critique of previous evaluations of Samory Touré has useful lessons for other researchers working in Africa. Holden warns against taking at face value colonial records and recommends caution in accepting the results of interviews with people who often tell the stranger-researcher what they think the government would like to hear rather than what they really think.

Three historical pieces with Islamic themes examine responses to colonialism in Guinea, Algeria, and Senegal. Marxist historian Jean Suret-Canale traces Touba's ties with its precol-

lonial, colonial, and postcolonial rulers and notes the constancy of the principles underlying these relationships. His discussion of the P.D.G.'s relations with this influential, religious center indicates that Guinea's ruling party, though pursuing a social revolution in the countryside, continues to respect Islamic traditions. Jamal M. Ahmed's study of the life and work of al-Ibrahimi (1889–1965) stresses the importance of Islam in the evolution of Algerian nationalist thinking. Ahmed's paper is especially interesting because it sheds light on the philosophical premises behind the policy of Arabization now being vigorously pursued by the Boumedienne regime. Donal Cruise O'Brien's short piece attempts to explain the rise of the Mouride Brotherhood in Senegal in terms of the religious *charisma* of its saintly founder, coupled with the more mundane organizational skills of the wordly Ibra Fall, one of Amadou Bamba's early followers.

Essays by Jack Goody and Sholto Cross analyze the emergence of messianic religious leaders in different colonial situations. Cross's biographical essay on Jeremiah Gondwe, a leader of the Watchtower Movement in Zambia, treats his career as a positive and creative response to colonialism. Goody's piece challenges the assumption that African religious revival movements can be explained adequately as results of the dislocations caused by colonialism and signs of resistance to colonial rule.

David Apter opens Part II with a paper in which he turns to Marx as a possible guide in his search for the elaboration of a framework for political science. Although highly suggestive, Apter's essay is too loosely constructed to make a successful synthesis of Marx's work and his own approach to political science. In contrast to Apter's speculative piece are Gavin Williams's and Ken Post's attempts to apply Marxian analysis to concrete cases. After presenting an interesting typology of an ideal-type neocolonial economy, Williams discusses social stratification in Western Nigeria using Weberian as well as Marxian concepts of class and social relations. Post uses Engel's conceptualization of ideology as false consciousness with great effectiveness to analyze the Jamaican Ethiopianism movement, which reached its peak in the 1930's.

The essays by Reginald Green and Ann Seidman are useful contributions to the study of African political economy. After describing the operations of foreign firms in Africa, Seidman offers several suggestions for maintaining national control over them. Green's long essay is a veritable primer on the political economy of

decolonization and a marked improvement over *Unity and Poverty: The Economics of Pan-Africanism* which he coauthored with Seidman. Identifying concentrated external dependence as the essence of neocolonialism, Green then examines several African economic strategies and puts forward Tanzania as a possible model for decolonization.

The four case studies concluding the collection support the editors' claim that microanalyses are more likely to facilitate understanding of the main political events and trends in Africa than those built around broad and often fuzzy concepts like "political development" or "national integration." R. W. Johnson's analysis of the successful suppression of the left-wing faction of the P.D.G. in Mamou tells us more about Guinean politics than scores of articles on the political doctrine of Sékou Touré. Basil Davidson traces the causes underlying the rise of revolutionary movements in Portuguese Africa in his contribution to the volume. Christopher Allen's detailed study of Gambian trade unionism provides a greater understanding of the fragility and weakness of African trade unions, while Martin Staniland, in his piece on nationalism and communal partisanship in Bongouanou, Ivory Coast, presents a clear picture of the political processes operating between center and periphery during the late colonial period.

Besides the high quality of the essays, what makes *African Perspectives* a stimulating and provocative volume is the authors' conscious efforts to challenge the conventional wisdom on their topics (even that established by Hodgkin himself on occasion) and to offer alternative methodologies and explanations. These features combine to make the collection a fine tribute to their mentor and a significant contribution to African Studies.

SHELDON GELLAR

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The Nordic Council: A Study of Scandinavian Regionalism. By Stanley V. Anderson (Seattle: University of Washington Press, 1967. Pp. 194. \$7.50.)

This is not a book about integration, but on the contrary a case study of nonintegration. The picture that emerges from its pages is one of a Scandinavian regionalism unable to make a place for itself, caught between narrower nationalisms and wider European regional movements.

If there ever were likely candidates for regional unification, a fair case can be made for the Nordic countries. They are, with a few ex-

ceptions, closely connected in an impressive number of ways—geographic proximity, language, race, religion, trade, history, and compatible political systems. They have had a record of successful functional cooperation, e.g., Scandinavian Airlines System, common negotiators in the Kennedy Round, a common labor market, and reciprocal social security systems. There are frequent contacts among fraternal political parties, civil servants, and cabinet ministers. In political style and culture they are considerably less alien to each other than, for instance, Mississippi is to Maine.

And yet, as Professor Anderson makes clear, these cultural and historical affinities are neither necessary nor sufficient conditions for integration. In the absence of a volitional unifying act, the consciousness of distinguishing characteristics has invariably turned out to be greater than the sum of shared values, and the economic advantages of EFTA or EEC have overridden the periodic attempts to create a Scandinavian common market.

Despite a growing literature on regionalism, the term remains unamenable to satisfactory definition. This difficulty may arise because there are many different kinds of regionalism, depending on the region in question or on the purpose of any particular regional arrangement. The definition which underlies this book is, in light of the subject matter, suitably unpretentious: "officially sanctioned continuous effort to increase regional harmony by peaceful means. Similarities are heightened and differences lessened" (pp. 8-9). This definition is clearly intended to be broader than those meanings that emphasize regional integration or defense, and in this respect the author's caution is not misplaced. With Denmark, Iceland, and Norway in NATO, Sweden is neutral by choice and Finland by necessity. Earlier attempts to form a common Nordic defense pact could not overcome these fundamental differences, and attitudes towards the desirability of EEC membership are widely divergent. Under these circumstances, the Scandinavian countries have not moved perceptibly in the direction of supranational organizations, but have instead chosen the path of cooperation ("peaceful change by persistent joint efforts to increase mutual advantage" p. 147.)

The chief organizational expression of Scandinavian regionalism has been the Nordic Council, and the bulk of the book is devoted to an extended description and analysis of the Council. In successive chapters, the author discusses the Scandinavian region; interparliamentary predecessors of the Nordic Council; its

organs, procedure, and relations with Scandinavian governments; common Nordic market negotiations; and concludes with some generalizations on the precise nature of Scandinavian regionalism and what we may extract from that experience. The appendices contain the Statute, Rules of Procedure, and Directions for Committee work of the Nordic Council (all annotated) along with the text of the Helsinki agreement and the Statute of the Nordic Inter-Parliamentary Union.

When this book was written, the Nordic Council had been in operation for about fourteen years, and its record in practice must have proved disappointing to any who had hoped that it might provide the nucleus for a United States of Scandinavia. Instead, it was to serve as little more than a sounding board of Parliamentary and public opinion on issues of inter-Scandinavian relations, an intergovernmental substitute for authoritative regional decision making. One sign of its weakness was the dominance of its Presidium and Secretariats, despite the prominence and continuity of service of the elected members. The tasks performed by the Nordic Council have been of a kind more appropriately performed by civil servants (preponderantly Swedish and Danish) than by politicians. The absence of dissent, arid discussions, excessive compromise, and reduction to levels "desired by the least-regionally minded member" created the impression that the Council was, to paraphrase one delegate, in danger of drowning in its own unanimity (p. 77). The Nordic Council in effect became a kind of pressure group—but one attended with a notable lack of success in penetrating either governments or publics. There is little doubt that the governments of the region are the real decision makers, and that a great many changes would have to be effected before the Nordic Council could acquire the authority it now lacks. (See especially pp. 106-108, which emphasize its present nonauthoritative status.)

The book is detailed, informative, and helpful to any serious understanding of the Nordic Council and inter-Scandinavian cooperation. It is commendably restrained on a topic susceptible to the distortion of integrationist sentiment. It is likely to disappoint those who are looking for prodigious leaps into deep theoretical waters, but this was not the intention of the author, and quite rightly in view of the realities of Nordic regionalism. The author is, however, somewhat more cautious in generalizing than he need have been, and one finds oneself wishing for more extended comment in those passages devoted to appraisal and generalization.

Some of the data could have been brought up past 1962 (e.g., Chapter IV). But, although there have been developments in Scandinavian regionalism since 1966, (e.g., the NORDEK negotiations and revival of the EEC question) none of these really impinges on the usefulness of the book. In a larger sense, it can serve as a reminder of the homely truth that not all regionalism need involve integration in the currently acceptable senses. Full-blown unification does not seem to have been entertained seriously as an objective, and its lack of attainment may not really amount to failure.

JOSEPH B. BOARD

Union College

Priming the German Economy. By John H. Backer. (Durham, N.C.: Duke University Press, 1971. Pp. 212. \$6.75.)

John Backer was a member of the American Military Government in the economic administration of the American zone of Germany. Among his assignments was the promotion of exports from Bavaria; as he proves in his book, he had an excellent vantage point from which to observe the achievements and failures of American economic policy. Subsequently, he served as a diplomatic officer in the Soviet Union but apparently never lost interest in his earlier experiences in military government; shortly after retirement from the foreign service he wrote *Priming the German Economy*. It should be emphasized that the book is by no means autobiographical—the reader will find the author in first person in only very few passages of the story—but there is little doubt that the author has fully used his valuable experience in the selection of materials and in their mature and objective treatment.

The book begins with a very clear account of the drafting of United States occupational policies in the middle of World War II. Roosevelt, Morgenthau, Hull, and Stimson all had a hand in fashioning these policies, and Backer shows how they in turn were influenced by military men and civil servants both in Washington and abroad. The result of their labors, the compromise codification in the Joint Chiefs of Staff Directive 1067 determined the overall Allied economic policy in vanquished Western Germany at least until July, 1947. The author is able to show what the administrators of this policy in the field were able to do under the clauses “to protect the safety and meet the needs of the occupying forces” and “to prevent starvation or such disease and unrest as would endanger these forces.” No other rationale of dealing with the German economy was permitted under

this directive, and one shudders to think what would have happened if these escape clauses had not been used fairly liberally.

With equal skill the author explains how the “thorny problem of reparations” was handled after World War II, quite differently from the post-World War I period. He carefully shows how the United States reparation position was arrived at and how it was changed in negotiations with the Russians. He also traces the relationship between the Allied reparation plans and industrial development in the western zones of Germany. There is a shorter but equally good account of the consequences of the monetary collapse on the German economy and the steps taken by the bizonal administration, and later the Germans, to deal with them. Finally, and here the author seems to have the longest experience, he tells of the efforts of the Joint Export-Import Agency to promote exports from the American and British zones, so that proceeds could be obtained which were used to pay for vitally needed imports. Anyone who has observed recent West German successes in the commercial field—less than 25 years later—and has seen German foreign exchange reserves exceed those of any other country, including the United States, can hardly believe the efforts that had to be undertaken by relatively few military and civilian administrators, both Allied and German, to achieve exports of just barely more than \$200 million for all of 1947! The story ends with the onset of the European Recovery Program and with the rise of West German agencies and personalities that take over the functions of military government. The author argues convincingly that at least twenty-four months could have been gained for the German economy if there had been a currency reform in the Summer of 1946 rather than in 1948 and if General Draper’s proposal to make 31 billion of industrial raw materials available could have been implemented. *Priming the German Economy* makes it amply plain why, mainly for domestic political reasons in the United States, these steps were not taken earlier.

To this reviewer there are three weaknesses in this otherwise excellent book. First, is it really necessary to reproduce so many documents relating to the occupational policies in full? In some instances exact quotation is essential; in others salient points and summary would do and render reading easier. Second, the author is apparently not as much at ease with economic theory as he is with political reality. For instance, the Keynes-Ohlin controversy over reparations in the interwar period is condensed

into one paragraph which, to put it kindly, should have been omitted. Lastly, although the author is extremely fair and generally tolerant of criticism of his own positions, he makes an exception in the fledgling opposition to American economic policies by German politicians in 1947. While few would rise to defend the Halmssen Report, Backer's criticism of Dr. Johannes Semler is not sufficiently supported by the author's argument. Maybe Semler was excessively political in his criticism of American occupational policies and in his questioning of United States motives; but was this not the kind of stuff necessary for the establishment of a new political system and an economic democracy in West Germany?

F. A. BREIER

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Agricultural Production in Communist China 1949-1965. By Kang Chao. (Madison: The University of Wisconsin Press, 1970. Pp. 357. \$15.00.)

Specialists interested in policy making in socialist countries governed by Communist parties will find this study informative, stimulating, and useful for general comparative purposes. Since few economic studies of socialist systems relate economic performance to organizational change and policy making, this work is of special importance as an exercise in the political economy of socialist countries.

Professor Chao leads off with an excellent summary of controversy within the Chinese Communist Party about how to collectivize pygmy farms using primitive technology. The leadership, he tells us, agreed upon the necessity of violent land reform but split upon the issues of when and how to collectivize family farms. The technocrat faction, represented by Liu Shao-ch'i and Teng Tzu-hui, argued for a longer transition period under private ownership of land before collectivization, at least until China could begin to produce enough farm machinery. The Maoist faction urged that collectivization immediately follow land reform, arguing that the longer the 'rich farmer' mentality survived amongst the peasantry, the more difficult it would be to socialize agriculture later on. Debate between these factions continued and tensions mounted until the Cultural Revolution resolved the conflict in favor of Mao's leadership and policies. The progress of collectivization through the 1950's and early 1960's, traced against this background of agrarian policy dispute with great skill by the author, makes for a fascinating account of how party discord influenced overall agrarian policy.

Part Two, consisting of five chapters, provides the best account so far in English of Chinese Communist efforts to supply agriculture with new technology and capital. The author and his associates have culled from an impressively large collection of periodicals and technical studies a wealth of information and data on progress of mechanizing farm production, developing irrigation facilities, introducing new cropping systems and seeds, and applying new chemical fertilizer. Have administrative efforts under collectivization produced a technological revolution? Not yet. While some gains were achieved by irrigating more land, cropping land more intensively, and developing some new, higher yield seed varieties, basic errors in introducing new technology to agriculture have caused irreparable harm to farmers and production: the wrong crop systems have been introduced; new plows have been misused; numerous water conservation schemes have caused a greater incidence of floods rather than providing farmers with a stable supply of water.

These examples strongly indicate that the party leadership prematurely emphasized mechanization (p. 116) and hastily introduced new seeds without testing against local conditions (p. 172). The leadership still has not recognized that a first step transformation of agriculture requires a biological revolution based upon the diffusion of new, high-yield seed varieties suitable for local conditions. This policy is difficult to carry out because administrators must be patient and await the results of the technicians and scientists involved in such projects. The party leadership has clearly demonstrated that other priorities come first, and that seed research is too slow and time consuming a strategy for raising farm production. As long as the leadership persists in this belief, it is very unlikely that sustained and rapidly rising yields for foodgrains and industrial crops can occur.

The final section considers agricultural production performance under collectivization. Professor Chao and his associates estimated provincial sown area for foodgrains from data collected from newspapers and periodicals. Their estimate of foodgrain sown area for 1949 approximates that computed by John L. Buck, which is considerably higher than the Communist estimate (p. 207). Chao next adjusted provincial crop yield data in order to multiply by sown area to obtain foodgrain supply estimates. It is not clear how he adjusted certain official yield figures and whether or not he used a weighted average of provincial foodgrain yield to multiply by sown area. The correct proce-

ture would have been to determine the percentage of sown area by crop, a very difficult task, and to multiply each amount by the respective crop yield. His final statistical results show that between 1952 and 1957, foodgrain production increased at an annual growth rate of 2.3 per cent, roughly equivalent to population growth. Chao's estimate stands below official claims but above the estimates of western economists such as Liu and Dawson.

Chao also attempts to determine the productivity of the combined contribution of labor, draft animal, and tractor power, irrigation, and fertilizer inputs to foodgrain production. As this exercise applies to only six years, involves very imprecise measurement of each input's change, and assumes a very questionable weighting procedure of each input's contributing share, his findings call for considerable caution and skepticism. His derived index input of productivity shows a decline of 1.7 per cent per year between 1952 and 1957. Such a finding implies that the growth rate of each input's supply had to increase rather rapidly to enable foodgrain production to increase at 2.3 per cent per annum. Chao fails to carry through in this exercise to show the required increase of each input to produce this growth rate while input productivity declined. One final critical comment: the author's statistical estimates would appear more convincing and valid had he presented them as statistical ranges with error estimates. If economists continue to estimate production performance in Communist China, as they will do, they should present their results as statistical ranges with some error estimate to inform the reader how wide of the mark the estimates conceivably are. Their findings will then carry greater credibility and elicit more respect.

To sum up, this study combines a careful and clear discussion of statistical inference and estimation with policy controversy and organizational change to give the best descriptive and analytic account of agricultural change in Communist China yet written. Chao's findings that foodgrain and technical crop production barely matched population change certainly appears plausible and valid. This reviewer regards such a performance as rather remarkable given the drastic organizational changes and upheaval which have affected traditional family farming. This performance, however, probably attests more to the experience and capability of the Chinese farmer than to the new organizations within which the farmer now must work and live. This fine study makes it abundantly clear that unless the policy of introducing new technology to farming is changed, Chinese agricul-

ture cannot experience the 'green revolution' now taking place in various parts of Asia and long experienced by Japan and Taiwan. This work should find its way onto the shelf of all specialists interested in economic development and political change in socialist systems.

RAMON H. MYERS

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Economic Development in the Soviet Union.

By Stanley M. Cohn. (Lexington: D. C. Heath & Co., 1970. Pp. 135. \$10.00.)

This is a monograph with many virtues, but here and there it loses its way. The author is at his best when he is handling figures and explaining their meaning. The chapter entitled "Analysis of the Soviet growth model" is full of useful comparative data, and the author seeks in a very interesting way to identify the factors which contributed to Soviet growth performance, among them: natural resources, education, migration from village to town, investment priorities. Perhaps there is one presentational problem of a statistical kind, which deserves a few pages of explanation. Soviet growth indices, especially in the period 1928-37, are highly sensitive to price weights. Thus using Bergson's recomputations, Soviet labor productivity during these years increased either by 1.8 per cent or by 8.0 per cent per year, depending on the weights used. The first figure is poor by international standards, the second is highly creditable. The reasons for the disparity lie, of course, not in any deficiency in Bergson's statistics which Cohn uses, but in the drastic structural and price changes of those years. Surely the reader needs to be told this in detail. And is it not a little misleading to give, in historical comparisons, a single figure for all countries except Russia, as if they were not sensitive to price weights too? Alexander Gerschenkron is quoted several times in his capacity as economic historian, and quite rightly so, but Gerschenkron was also the author of an admirable study which pointed to the extraordinarily high price sensitivity of the index of machinery output in the United States in the period 1899-1939. Cohn might well reply that detailed technical examination of index numbers is beyond the scope of this short work, and it is. But one would wish for a brief discussion of such matters all the same, because they are germane to the proper understanding of the figures.

A rather different criticism concerns the presentation of sources. Where Soviet figures or data exist, it seems only right to quote the Soviet original, or explain why—as in the case of

indices of GNP—it is necessary to use the best available western recomputation. But if an actual Soviet-published figure is used, as for instance the number of men in the Soviet armed forces in 1950, then it is slightly odd to find Bergson cited as a source. After all, Bergson presumably reproduced the figure cited in a Krushchev speech; he did not compute it. It seems even more odd of Cohn to cite a paper of Michael Kaser's on economics of education as a source for the rate of growth of Japanese GNP. Surely Cohn would not allow his own graduate students to do this! Needless to say, the reviewer wishes to imply no disrespect to either Bergson or Kaser; he would be just as critical of anyone who cited one of his own works as a source for the date of the death of Stalin, say.

There are some slips in the historical section. Thus it is not the case that "the Fifth Five-year plan (1950–55) was never published." It was presented to the 19th party congress by Malenkov in Stalin's presence. Nor did Trotsky expect the World's workers to be impressed by a "model of a genuine and successful socialist state" in the USSR, since he denied the possibility of socialism in one country.

Yet the book contains many valuable observations and statistical analysis of a high order. Its defects seem to me due primarily to uncertainty in the author's own mind as to its purpose and its readership.

A. NOVE

University of Glasgow

West African Chiefs: Their Changing Status Under Colonial Rule and Independence. By Michael Crowder and Obaro Ikime. (New York: Africana Publishing Corporation and the University of Ife Press. Pp. 453. \$18.00.)

Historians of colonial and postcolonial Africa might do well to recall the "debate" between Alexis de Tocqueville and Edmund Burke over the French Revolution; for in Tocqueville's analysis of the *ancien régime* and in Burke's *Reflections* one may find accounted for, in terms of national political experience, those divergent ideas and practices that characterized the two nations' colonial empires in Africa and that still deeply influence the political life of "anglophone" and "francophone" regions of the continent. Burke's implication that France in 1789 might well have had a British-type 1688 revolution instead is decisively answered by Tocqueville's demonstration that on the eve of the revolution, France had long since been irrevocably committed to bureaucratic central-

ism. Britain, meanwhile, remained attached to those "customary" institutions that were to develop into what is called, with a particularly British resonance, "local government."

Thus it happened that, while local circumstances sometimes led to similar results on both sides of an Anglo-French border, the men who administered British colonial Africa generally cherished gentry ideals (even when not of gentry origin), while their French counterparts, as Pierre Alexandre notes in his contribution to this volume ("Chieftaincy in French West Africa") were usually republican, which is to say universalist, egalitarian, and anticlerical in sentiment. Their attitudes toward chieftaincy were correspondingly different (though again local circumstances might produce prudential convergences). The French chief was generally an appointed *fonctionnaire*, his British counterpart more often a local ruler with some claim to traditional legitimacy. British administration concerned itself far more with local government (which incorporated chiefs), with indigenous courts and law, and with the mastery of local languages, while the French emphasized administrative efficiency and the diffusion of the French language. There is currently in vogue a revisionist view that imperialism in Africa was essentially the same everywhere, but while it is useful to point out similarities where they existed, the revisionist argument has, I think, been overdone, as the studies collected in this volume show. The editors seem, in their introduction, to agree, and they point to the consequent differences in the fate of chiefs under independence in French and British areas. Britain and France were different in the nineteenth century, and their colonial policies and practices reflected the difference. As Winston Churchill remarked in another connection, "it will take many ingenious books to raise this issue to the dignity of a historical controversy."

This volume presents papers delivered at a symposium held in 1968 at the Institute of African Studies of the University of Ife in Nigeria, and it is to be welcomed as the first major publication of that University's Press. Its strength is the series of detailed accounts of chieftaincy under colonial and postcolonial conditions in various parts of formerly French and British West Africa. Jean Suret-Canale provides an excellent discussion of Fouta-Djalou, and J. Lombard gives a fine treatment of the Bariba of Dahomey. I. A. Asiawaju compares two Yoruba states: Meko and Ketu, one under French, the other under British rule—incidentally bringing out very nicely the Anglo-French difference. Elliott Skinner deals very

skillfully with the Mossi, whose ruler, the Moho Naba, was the most durable of African monarchs to fall under French rule, and the one to whom the French perhaps conceded most during the colonial period. He was also shrewd enough to steer through the intricacies of immediate pre- and postindependence politics and emerge with what Skinner feels is a reasonable chance for survival.

Discussions of formerly British territories include John Smith's on the relationship between British officers and chiefs in Northern Nigeria, John Paden's on Kano, Ronald Cohen's on Bornu, J. A. Atanda's on Oyo, E. A. Ayandele's on Ijebu, and S. A. Akintoye's on the Ekiti. All these are in Northern and Western Nigeria, where traditional rulerships were strong. Something like the same situation prevailed in Benin, described by Philip A. Igbafe, and among the Itsekiri, about which Obaro Ikime writes. In Eastern Nigeria, on the other hand, the policy of "indirect rule," applied by Lord Lugard in the North, and later extended to the West, ran into difficulties, for the Ibo and other peoples of that region did not possess strong rulers of the sort found in the North and West. G. I. Jones's account of the effort to create chiefs there does much to clear up earlier confusions on the subject.

The volume includes several discussions of chieftaincy in Ghana and one in Sierra Leone: in Northern Ghana by Phyllis Ferguson and Ivor Wilks, in the "foreign quarter" (*Zongo*) of Kumasi by Enid Schildkrout, in Larteh by David Brokenshaw, and among the Limba by Ruth Finnegan and David J. Murray. Perhaps the best of these is Brokenshaw's analysis of the "resilient" chieftaincy in Larteh.

I would have appreciated more attention than the volume gives to indigenous law and courts, a subject which I think might have thrown further light on the Anglo-French difference. British officials were very concerned with these matters, while French ones *appear* to have ignored or suppressed them. Furthermore, the discussions in the seminar (assuming that the published volume reflects them accurately) might have benefited from a somewhat less exclusively *West* African focus. The notion that "indirect rule" through indigenous political institutions originated with Lugard's administration in Northern Nigeria would not have occurred to anyone familiar with *East* Africa, or, indeed, to anyone attentive to Lugard's career. Margery Perham, in her biography of Lugard, makes it clear that his prior experience in Uganda was crucial to his formulation of the idea (if not the term), and he had still earlier

served in India, where the princely states provided a model. Actually, the idea is probably as old as imperialism itself. I also miss any reference to the very considerable literature on central and southern Africa that addresses the very questions that preoccupy the authors of this volume. One appreciates the authors' concern to develop the Anglo-French comparison within West Africa, but it would be a pity if African Studies were to break up into self-contained regional blocs.

LLOYD A. FALLERS

University of Chicago

The Diary of a Diplomat in Russia, 1917-1918.

By Louis de Robien. (New York: Praeger, 1970. Pp. 318. \$8.50).

Civil War in South Russia, 1918.

By Peter Kenz. (Berkeley: University of California, 1971. Pp. 351. \$10.00.)

The Victors' Dilemma: Allied Intervention in the Russian Civil War, 1917-1920.

By John Silverlight. (New York: Weybright and Talley, 1970. Pp. 392. \$10.00.)

The Russian Revolution occupies a central place in the history of the twentieth century, though not so important as the one in which Soviet leaders and scholars have enshrined it. Similarly, the role various Western states and Japan played in the Russian Civil War has also been important, although not so great as that assigned to it by Soviet politics and Soviet mythology. Both the revolution and allied intervention have attracted enormous attention, not only from scholars and politicians but also from novelists and film-makers. During the past twenty-five years, as Soviet-American relations have emerged as a crucial factor in international politics, these historical developments have created almost an industry among scholars and would-be scholars.

These volumes represent three different kinds of contributions to this search for knowledge and understanding. Count Louis de Robien was a young Breton lawyer who learned Russian at the School of Oriental Languages in Paris and who was assigned to a junior post in St. Petersburg early in 1914, when he was only 26. He began his diary in March, 1917, with the coming of the first revolution of that year, and he continued it through 1918, with one long interruption. The diary was published originally in French in 1967, almost a decade after de Robien's death, which followed a lifetime of service in the Quai d'Orsay, climaxed by appointments as Ambassador to Romania and as Chief

of Personnel at the Quai d'Orsay. Unfortunately, the English translation provides no introduction or footnotes to aid the reader, and the volume lacks a map of Petersburg-Petrograd, which would have been most useful for both the scholar and the ordinary reader. Moreover, *The Diary of a Diplomat* has serious shortcomings as a source and as a guide to the activities, mood, and temper of 1917-1918. De Robien was a junior officer and one with profound aristocratic views, which made understanding the Russian scene very difficult for him. He was not a skilled observer, and the information he provides is often fuzzy or unclear. However, his diary does provide significant insights concerning the allied diplomatic community and concerning life in Russia's capital during a time of incredible confusion and agony. He saw quite clearly that the Bolsheviks were likely to triumph because they alone pledged immediate peace and because the Russian will to fight was absolutely dead. His descriptions of demonstrations, of street fighting, of the effects of the transport crisis (although one could get Black Sea oysters in some restaurants in the summer of 1917), and of the qualities and views of the aristocrats whom he knew are all valuable. Above all, de Robien on occasion showed remarkable insights concerning probable developments, handicapped as he was by his political views.

Professor Peter Kenéz, of the University of California at Santa Cruz, is a young foreign-born, American-educated scholar whose volume undoubtedly represents the revision of a good Ph.D. thesis. His book is therefore a case study, a monograph on the beginnings of the Russian Civil War in the South. It is especially valuable for its explanation of the failures of the Volunteer Army and of the Whites in general: their glaring disunity; the absence of effective leadership; the conflict between their dreams of "a Russia Great, United, and Indivisible" and the goals of the national minorities; the organizational abilities of the Bolsheviks; and the lack of political sense shown by the Volunteer Army generals. Indeed, the Kenéz study illuminates the need for a study of the military leadership of the Czarist forces. What kind of professional and general education had it received? Why were the generals so utterly lacking in political sense?

Kenéz provides no bibliographical essay or annotated bibliography, and the bibliography has curious omissions. The extensive use he made of the Hoover Library and of the Russian and East European Archives at Columbia University demonstrates how valuable these re-

sources are and what a significant contribution its collectors have made. He makes no reference to Soviet sources, although he has studied in the Soviet Union.

In summary, *Civil War in South Russia, 1918* is an interesting and valuable book, a first volume of which a young scholar can be proud. At the same time, perhaps because the subject itself is so vast and involves so many other contemporary developments, the volume contains several serious flaws. It devotes insufficient attention to the fighting itself and to factors such as supplies. It is not clear about the significant role Germany's changing position played. Above all, it tries to isolate the Civil War in the South from the rest of the canvas, including the climactic year on the Western Front and the policies of Russia's allies.

Silverlight's *The Victors' Dilemma* satisfactorily discusses the incredible problems these allies faced in trying to comprehend and deal with the Russian revolutions, the Bolsheviks (whom we understand with difficulty even now), the various national minorities seeking to obtain their independence, the various other anti-Bolshevik groups, and the general confusion within Russia; it also deals well with the crises in the West caused by the Germans' final unsuccessful thrust for victory in 1918, their sudden collapse, the end of the war, the flood of problems revealed by peace, and the political pressures within and among each of the victorious powers. His fair-minded, lucid description is a very considerable achievement and no doubt represents in part the skills Silverlight has acquired as assistant editor of the *British Observer*. Above all, he succeeds in enabling the reader to understand the extraordinary complexities of the issues with which the allied leaders had to deal, at a time when the entire world was in disarray. The information they could obtain on developments within Russia was scanty, tardy, and unreliable. Often, intelligent decisions, based upon a sound understanding of the national interests of the states involved, were upset or reversed by quirks or flukes. Silverlight argues, with some justification, that the decisive factor was the action in the summer of 1918, of the Czech troops, eager to leave Russia to fight for the creation of an independent Czechoslovakia and convinced that the Bolsheviks were seeking to block them. One of the many ironies in this genuine tragedy of errors was that the war ended and Czechoslovakia became a free state before the Czech troops could have arrived, even if the Bolsheviks had sought to assist them.

This volume relies heavily on the most recent

and most able academic studies of this tangled period, upon the memoirs of British statesmen in particular, and especially, upon British Cabinet Papers, which were opened to the public in 1966. It uses no Soviet sources, undoubtedly because Silverlight does not read Russian. Its maps are unclear and not especially useful. It was also written for educated Englishmen who possess more knowledge and understanding of British politics than do Americans and other foreigners. But it is an excellent general volume which should increase our understanding of the problems statesmen faced fifty years ago and face still today.

ROBERT F. BYRNES

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Economic Reforms in Eastern Europe. By Michael Gamarnikow. (Detroit: Wayne State University Press, 1968. Pp. 202. \$8.95.)

In all political systems there is a definite relationship between economic and political developments, but nowhere is the relationship as close as in communist states. Since the days of Stalin, almost all communist leaders (Marshall Tito of Yugoslavia is the exception) have maintained a tight grip on the economies of their countries through strict control over the central planning apparatus. In this way the communist leaders have determined investment priorities, resource allocation, and the proportional development of the economy—invariably favoring heavy industry and defense over agriculture and consumer goods. By the mid-'sixties this system of "command economy" had achieved some notable successes in the development of heavy industry. But by that time the communist leaders of the Soviet Union and Eastern Europe also realized that their economies were in serious trouble. The capital-output ratio (the amount of capital needed to produce a given unit of output) was increasing at an alarming rate, labor productivity remained distressingly low, and piles of shoddy and unsalable consumer goods had begun to accumulate in state warehouses. The situation was particularly acute in the nations of Eastern Europe, which lack the abundance of raw materials that has enabled the Soviet Union to "muddle through" its economic difficulties. It was as a result of these economic pressures that the communist leaders of Eastern Europe, in many cases reluctantly, began to experiment with reforms of their command economies, and it is this process of experimentation in the years 1964–1967 which Michael Gamarnikow describes in *Economic Reforms in Eastern Europe*.

Of greatest interest to political scientists in this very useful book is Gamarnikow's description of the political obstacles encountered by the advocates of economic reform, who were primarily economists. Although the pattern of economic reform differed from country to country (Gamarnikow provides an excellent comparative treatment of the reforms), almost all reformers agreed on the following three points: decentralization of the economy by giving greater autonomy in decision making to individual factories; the establishment of profit rather than gross output as the criterion for judging the effectiveness of a factory; and the introduction of a more rational price system related to scarcity costs and based on the needs of the market, in place of the existing administratively determined system of prices.

There was a great deal of political opposition to reforms of this type. In the first place, the central planning apparatus was very reluctant to reduce its power by granting the individual factories more autonomy. Secondly, many party leaders were fearful of losing control over the economy if too much initiative were left in the hands of the factory managers. In addition, since the allocation of scarce economic resources is one of the party's major powers, many communists were reluctant to give up the "communist pork-barrel," in which they were able to construct huge enterprises in politically desirable areas—irrespective of the economic rationality of such investment decisions. Another source of opposition came from those individuals, often with good party connections, who stood to lose their jobs as a result of the reforms. These included not only members of the central planning apparatus, but also factory managers who owed their jobs more to political loyalty than to economic ability. Indeed, such individuals, good at carrying out routine orders from above in the system of command economy, proved unwilling to take on the additional responsibilities and challenges which would be necessitated by the reforms.

Underlying the whole debate about economic reform in Eastern Europe has been the question of whether genuine economic reform, which would mean the establishment of a pluralistic system of autonomous economic enterprises guided by signals from the market rather than orders from the party-controlled central planning apparatus, would eventually lead to the democratization of the East European communist states. According to Marxist theory, the economic "base" determines the political "superstructure," and, while the reverse has hitherto been the case in communist practice, many

of the economic reformers, and Gamarnikow himself, seem to feel that such a process of democratization is inevitable. While I am not quite so sanguine, it is clear that many of the communist leaders of Eastern Europe have been preoccupied with just such a concern. The end result has been that the Eastern European communist leaders, with the exception of Janós Kadar of Hungary (and of course Tito) have been endeavoring to allow just enough economic reform to make their existing systems of command economy work more efficiently, but not so much as to jeopardize their monopoly of political power. This economic "juggling act" is a very difficult one, and, as the fall of Gomulka demonstrated, the success of such halfway measures is by no means assured.

In presenting these issues in a comparative way, and in a manner easily intelligible to political scientists, Mr. Gamarnikow has done a service to students of Eastern European affairs. It is hoped that he will soon update his book by adding a discussion of the effect of the Soviet invasion of Czechoslovakia on the process of economic reform in Eastern Europe and in the Soviet Union itself.

ROBERT OWEN FREEDMAN

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Colonialism in Africa 1870-1960. Volume Two: The History and Politics of Colonialism 1914-1960. Edited by L. H. Gann and Peter Duignan. (Cambridge, England: Cambridge University Press, 1970. Pp. 563. \$17.50.)

This is one of those reviews that tell you what the book is not, as well as occasionally discussing what it is. Works that attempt grand summations of forces and events are fair game for such an approach. The present work is nothing less than "the second volume in a five-volume collaborative history of the colonial era in Africa" (front flap blurb). The editors define their project in the first two volumes as "essentially a work of history . . . designed to interpret the underlying motivation of what has often been called 'the New Imperialism'" (p. ix). Succeeding volumes will deal with the social impact of imperialism on the peoples of Africa, with economic forces, and with bibliographic and archival sources.

Other academicians can decide to what extent the aspirations of their respective fraternities are fulfilled. Despite equal billing for the politics of colonialism, political scientists will be disappointed. "Who gets what, when, and how" seems largely a public and official affair.

Two chapters out of fourteen concentrate on black African responses to colonial rule. Immanuel Wallerstein notes important but not basic social changes at the macro level, while Martin Kilson presents evidence—largely from Ghana—of the rise of new skill groups at the micro level. There is no chapter on trade unions, rural associations, or political parties, despite the fact that political scientists have devoted just about all their research efforts in Africa on these matters in the past fifteen years. Crawford Young's chapter on decolonization emphasizes Metropolitan politics, a vantage point that leads to overestimating the possibilities for controlling events once political challenges get under way. The historian A. A. Boahen, in the chapter entitled "Conclusion," notes the glaring omission of any treatment of African parties between 1944 and 1960 (p. 517). Is it significant that Boahen is the only black African contributor? Since the editors saw fit to include an "Epilogue," which in part disputes Boahen's view, the absence of more extended discussion in the body of the book is even more inexplicable.

Continuing in this vein, what else "ought" political scientists to expect? For one thing, a new look at the connections between foreign policy and domestic politics—including pressure groups within and outside government—would be welcome. Gann and Duignan are not unaware of the importance of this subject. They just don't think it is worth a whole section. Another general theme, suggested by the dialectic of the construction of an imperial order and its overthrow, is the interplay of "allocating values" in the field, in territorial headquarters and in the metropolitan capital. In perpetuating the convention of confining discussion of colonial policy to one Metropole at a time (omitting also, by the way, the experience of Spain and Italy), the editors miss an opportunity to review comparatively what we might call "command and control" problems, as well as a derived issue, intrabureaucratic politicking. Michael Crowder's chapter on "The White Chiefs" makes a start that suggests this line, but it is all too brief.

Finally, except for potshots at a vulgar economic interpretation of political events, the editors relegate mining and commerce to a rather recessive role. Perhaps the fourth volume will make up this loss. Yet inclusion of these forces in the economics volume perhaps prejudices the controversy. Colonial rule as a system of power—the political economy of colonialism—seems weighty enough to risk substantial treatment. In

addition, a serious Marxist interpretation of colonialism and revolution might spell out the purported dangers of a doctrinal approach, and provide some raw meat for argument.

The editors make some good suggestions for further research, most of which have political import: the origins of African socialism, cooperation between groups in the black diaspora and African political movements, the influence of neo-traditional symbols on political rhetoric and opinion, and biographies of African political leaders. Possibly, Gann and Duignan think more work has to be done on these subjects before contributions can be invited. My guess is that chapters on these subjects at present could be just as illuminating as the rather thin and Anglocentric contribution on "Military and Police Forces" or the establishment-oriented chapter on "Missionary and Humanitarian Interests."

It appears the editors want their audience to include more than scholars and Africanists, but the style of discussion is exceedingly broad, even for the general reader. It ranges from a kind of undergraduate lecture by Hubert Deschamps ("France in Black Africa and Madagascar between 1920 and 1945"), through a stimulating sketch of greater research possibilities by Roger Anstey ("Belgian Rule in the Congo and the Aspirations of the Evolvee Class"), to Kilson's data- and citation-crammed paper fit for a professional gathering. To call the contents "uneven" is an understatement.

More significant, however, is the lack of a sustained perspective or of the opposite—a series of reasonable and enlightening alternative perspectives with which to view the enormous changes wrought by colonialism. Most of the contributors seem unaware of one another's papers, although the attentive reader can find some similar issues with which to create a dialogue. Gann and Duignan themselves, however, do have a perspective, prominent in their "Introduction," their "Epilogue," and their chapter, "Changing Patterns of a White Elite." They see colonialism in the light of historical forces at work on a worldwide scale, forces that involved beneficent motives, processes, and outcomes. Unfortunately their approach is barely reflected in other contributions.

Put rather bluntly, Gann and Duignan are telling us that white interests in Africa were, for the most part, not exploitative and not racist. They believe scholars ought to be brave enough to examine their naive, liberal, reformist biases. It is difficult to predict the overall impact of their multi-volume effort, but it is clear that Gann and Duignan have staked a

claim as academic revisionists. (I might add "right-wing," but that would place me in the same doctrinaire bag as many of my fellow academicians.) Gann and Duignan make a useful analytical contribution, although their chapter on the white elite may strike some people as bordering on apologetics. They want scholars to examine policies and the consequences of policies in the light of possible alternatives, seeing actions and outcomes in terms of least-worst options. In addition, they emphasize the two-edged nature of sociopolitical relationships: for instance, at some point dependence can have advantages. The editors make the intriguing suggestion that we view colonialism as a facet in the expansion of "the frontier" around the world. They believe race relations in Africa under colonial rule were no worse than elsewhere and in some cases a good deal better.

Readers might be more confident in their own evaluation of this position if other contributors tackled the issues raised by Gann and Duignan. They might agree that it is time "to strike a balance between the Eurocentric and the Afrocentric approach" (p. ix), if a few more chapters were clearly Afrocentric. They might also feel less uneasy about the whole argument if contrasting views did not invariably set up straw Marxists to knock down, as I fear Gann and Duignan do. Some readers may get the feeling that redressing the scholarly balance fits into some greater design of anticommunist struggle.

This is hardly the definitive work on colonialism in Africa. But it carries within itself the seeds of a worthwhile dispute.

HARVEY GLICKMAN

Haverford College

Self-Determination and History in the Third World. By David C. Gordon. (Princeton: Princeton University Press, 1971. Pp. 219. \$7.50.)

In the attack upon colonialism it has with good reason come to be accepted both by political activists of the third world and by commentators from many camps and countries that the most grievous injury inflicted by colonialism has been the spread among the colonial peoples themselves of the conviction that they are inferior branches of mankind. The counterpart of this proposition was, of course, the belief among the white, European-descended peoples that their superiority entitled them to dominate the remainder of mankind and to determine the rules by which the great game of world intercourse should be played. The fullest

realization of these propositions came in the colonial countries where the presumption was that the rulers acted while the ruled obediently responded.

With the rise of third-world nationalism and the speeding up of decolonization this older relationship became intolerable, and the new rising forces demanded recognition of an equal status and dignity for their peoples. Everywhere as peoples in the modern age have laid claim to their national identity and destiny they have rewritten their history in such fashion as to justify their claim in the light of their distinguished past. On the contemporary scene the most striking case is that of black Africa, striking primarily because of the denial by white historians, who commanded the heights of historiography, and by laymen as well, that Africa had any history other than that imposed by the Europeans. The recent past has seen a rush to fill this supposed vacuum and to demonstrate that Africa had a precolonial history no less worthy of note than that of other parts of the world. In particular the so-called Dar es Salaam school has striven to establish that Africans were not merely passive objects of European initiatives but were from the outset actors in their own right and that the nationalist movements which came to the fore after World War II had firm roots in the early African resistance to European encroachment.

It is this theme, with a concentration of attention on the Middle East and the Arab world, which is elaborated in *Self-Determination and History in the Third World*. In it the author, who is a professor of history at the American University of Beirut, sets out to examine the uses and abuses of history by and on behalf of peoples who are asserting their right to run their own affairs and speak for themselves. The self-determination with which he is essentially concerned is not the legal and political right of "all peoples" to get out from under colonialism—a right that the United Nations has so insistently proclaimed—but rather the search for their own historic souls by peoples who have been excluded from history by Western imperialism.

In the opening paragraph the author distinguishes between two kinds of history. One is that collective memory of a people of its past experiences, heroes, and great deeds which becomes a basis for its sense of identity—"an accumulation of myths, illusions, symbols" (p. 3). The other is the critical, scientific study of scholars for whom history's validity is determined by objective criteria. History as utilitarian myth, serving the collective interest, and

history as objective science obviously overlap at a number of points, but however fuzzy the boundary between the two, it is the first type with which Professor Gordon is primarily concerned in this book. Within this type he further distinguishes three different approaches to history of which he makes much use: the approach of the "futurist," who tends to turn his back on history, that of the "apologist," who seeks a return to a specific past tradition, and that of the "resurrectionalist," who looks to a progressive future based on a reinvigorated past.

Sympathetic to the efforts of peoples to free themselves from "the paralyzing myths of the colonizer, if only through countermyths" (p. 191), Professor Gordon also warns of the dangers of a new counter-racialism, citing Paul Valéry's judgment of history as "the most dangerous product evolved from the chemistry of the intellect," making nations "bitter, arrogant, insufferable, and vain" (p. 179). Of the peoples with whom he deals, the author regards the Tunisians as the most "mature" (his own quotation marks) in view of their readiness, following Bourguiba's lead, to acknowledge a broad Mediterranean heritage which enables them to draw upon Rome, Carthage, and France as well as Islam and Arabia.

Two items may be briefly and somewhat arbitrarily selected from the wealth of material the book contains, which, it deserves to be mentioned, includes significant comparative comment on India, Africa, and American black nationalist movements.

Myths not infrequently contradict each other—as the author puts it, one man's myth is another man's poison—and he cites in illustration the conflict between Pan-Arabism and the nationalism of particular Arab peoples. This is peculiarly the case in Africa where any pursuit in depth of national myths means in most instances the singling out of tribes which at least potentially threaten the unity of the incipient nation. Second, the resurrection of the past of the third world to an impressive extent has been a product of the labors of western archaeologists, anthropologists, historians, and colonial administrators. The myths and symbols on which the third world draws are only in part, and in a number of countries in small part, the contribution of the people of those countries, although increasingly they are taking over the investigation and presentation of their own heritage.

This is a book which is no doubt of special interest to those concerned with the Middle East, but it is written with style and grace and

it develops a theme of universal interest, including inquiry into no less a question than whether history needs to be true.

RUPERT EMERSON

Harvard University

The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy. By David Green. (Chicago: Quadrangle Books, 1971. Pp. 370. \$10.00.)

The Quest for Change in Latin America: Sources for a Twentieth Century Analysis. Edited by W. Raymond Duncan & James Nelson Goodsell. (New York: Oxford University Press, 1970. Pp. 562. \$12.50.)

In his very readable, well documented work, David Green challenges the generally held view that Franklin Delano Roosevelt's Good Neighbor Policy toward Latin America was a notable success, and the view held by some that the postwar decline in hemispheric unity could be attributed to the abandonment of the economic aid policies of Roosevelt's administration. The Good Neighbor Policy was a success for some years, Green agrees, but he is convinced that over the long run, it proved to be a failure. Indeed, he claims, it was largely because succeeding administrations followed President Roosevelt's economic aid policies that relations between the United States and Latin America have deteriorated to their low point today.

By the time Roosevelt proclaimed the Good Neighbor Policy in 1933, depression nationalism in Latin America had already reached full blast; in fact, revolutionary nationalism was aflame in many of the countries. And although the national movements differed in many respects, all of them had one common concern: their goal could be achieved only if their nations were able to gain full control of their own economic resources, many of which were owned and controlled by foreigners, especially Americans.

Yet ignoring the ominous portents, the Roosevelt administration almost from the beginning took a strongly negative attitude toward rising Latin American nationalism. The administration was certainly not opposed to the development of Latin American resources, for the United States depended upon and profited from their development. Nor was the administration rigidly committed to defending the property rights of Americans in the region. Rather, Green believes, administration policy-makers for the most part "felt that they, rather than the revolutionary nationalists, had a more realistic

and sensible perception as to how Latin American resources development ought to proceed" (Preface, ix).

This negative attitude seemed to give way to a more positive one when President Roosevelt announced a "New Approach" in 1940. Referring to Latin America, the President stated, "Give them a share. They think they are just as good as we are, and many of them are" (p. 38). The New Approach at first seemed promising, for it recognized the need both of American private enterprise and of Latin American societal growth. But World War II served to maintain and even strengthen United States influence over the nature and direction of Latin American economic development.

Green's work, utilizing many unpublished as well as published materials and also interviews with prominent Americans and Latin Americans who had a hand in the policy making, concludes with 1948. But a brief epilogue of events to date forcefully states his conclusions regarding the long-term effects of the Good Neighbor Policy. Over the long run, he is convinced, the Good Neighbor Policy was a failure. For the establishment by the Truman administration of the Inter-American system in 1947 and 1948 in effect formalized United States supervision and control of Latin American economic development. Also, the policy-makers, besides failing to convince Latin Americans that the Good Neighbor Policy was a policy of institutionalized benevolence, more importantly failed to dissipate or obviate the need for rampant Latin American revolutionary nationalism.

As the United States became more involved directly or indirectly in pushing through its policies, it became obvious that the Good Neighbor Policy, stressing a closed hemisphere in an Open World, had resulted in a plan for containment of Latin America. Ironically, the policy in time even equated revolutionary nationalism in Latin America with the supposedly dangerous threat of communism.

Moreover, the Good Neighbor Policy was largely responsible for the political instability prevalent in Latin America. For while economic control remained frozen in the hands of foreigners, "political control of local government machinery in Latin America passed swiftly from hand to hand among various local groups, all competing for the wealth and power which could be theirs through 'cooperation with the United States on an inter-governmental basis'" (p. 295).

In essence, Green contends, the United States did not want neighboring allies who were

"dependable and strong" but allies who were "dependable and weak" (p. 296). In explaining this stance, Green's commendable work should be a choice morsel for policy-makers and others to chew on. It is hoped that the State Department has some dog-eared copies it is putting to good use.

A Quest for Change in Latin America, edited by W. Raymond Duncan & James Nelson Goodsell, attempts to provide authoritative sources for an in-depth understanding of the ceaseless, militant striving for change that pervades Latin America today: political change, economic change, social change. Changes in every realm of the developing society that will enable it to utilize its full potential, realize its deep aspirations, and attain its desired stature in the world.

Of the 89 well selected readings, some are documents, such as Articles 27 and 123 of Mexico's Constitution of 1917, and the Objectives of Bolivia's Agrarian Law promulgated in August, 1953. But most of them are the speeches and writings of influential political spokesmen who played a prominent role in the development of their own countries or of Latin America as a whole.

The tone of rising, militant nationalism is explicit or implicit in most of the selections. Almost half of the volume (three of the seven sections) is devoted to nationalism in three nations—Mexico's Revolution of 1910, Cuba's Revolution of 1959, and Chile's "Revolution in Liberty" of 1964. The various spokesmen and writers give vent to their ideas and feelings on a wide variety of controversial issues, such as the North American threat, the European threat, anticlericalism, land reform, and Yankee capitalism. A brief introductory note preceding each reading gives the setting and helps to interpret it.

Yet whatever the subject of a particular speech, article, or document, the underlying idea is how to achieve change. For as the editors state,

Since the opening of the century, there has been a growing acceptance by Latin American leaders of the idea that man makes and is, at least in part, responsible for his own society. That idea is the common denominator of the sources brought together in this book. The acceptance of the idea brought about a willingness to change that could well be compared to the radical nature of the Copernican Revolution (Preface, vii).

The many ideas expressed in the volume are a yeasty brew and will probably stimulate the student of Latin American politics to probe more deeply into the philosophies and contribu-

tions of the various leaders. Especially interesting are the great differences of opinion expressed by the leaders on significant issues. What should be the national goals, and which should receive priority? What are the best means of attaining them? Just how fast should the rate of change be? And perhaps most important of all, should needed change be accomplished by revolution or by reform?

This stimulating collection of readings, many now available for the first time in English, is a most worthwhile contribution to a realistic understanding of what Latin Americans want, why they want it, and why they will eventually get it.

ALEXANDER EDELMANN

University of Nebraska

The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents. Edited by Sven Groennings, E. W. Kelley, and Michael Leiserson. (New York: Holt, Rinehart and Winston, 1970. Pp. 489. \$9.95.)

It has been nearly a decade since William Riker first demonstrated in *The Theory of Political Coalitions* (New Haven, Conn.: Yale University Press, 1962) the theoretical leverage that a focus on coalitions can offer to the study of politics. For many studies in the volume under review, Riker's rigorous development and fruitful application of formal methods to the study of coalition behavior serves as a theoretical benchmark. Now, however, "the theory" in the title of Riker's book has become "theoretical perspectives" in the title of *The Study of Coalition Behavior*, aptly reflecting the diffuseness of coalition theory today. Unfortunately, the editors of this volume have not brought much coherence to these perspectives and have been derelict even in failing to publish an index, which makes it very difficult to trace common themes through the volume's twenty-five chapters.

More serious problems arise, however, in the "basic definitions and assumptions" they posit, which do more to confuse than clarify issues. By stating what they do *not* assume at the outset, the editors admirably fulfill their purpose of providing "as much variety as possible" (p. 3)—and giving almost total indirection to what follows. Lest this theoretical leeway turn into theoretical license, however, we are told that coalitions "are seen as events in a process" (p. 7). Whatever *this* means, we can at least ruminate on the proposition that this notion of coalition "is neither static nor conventional" (p. 7). Of course, conventional thoughts about a

field "still in its infancy" (p. 3) would be unnatural.

When the editors finally get down to basic definitions (at least those which are italicized), they first eliminate from consideration *coalition situations* that produce "maximum satisfaction for all the actors" (p. 7). I think this is an unfortunate restriction, because it not only excludes the study of conditions that lead to Pareto-optimal outcomes but also seems to exclude the study of public goods. Such goods by definition are available to all members of a group and provide satisfaction which is maximal in the sense that no individual could purchase more only for himself.

As Riker has given major impetus to the study of coalition behavior in conflictual (i.e., predominantly zero-sum) environments, Mancur Olson in his *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965), has done so in environments not assumed to be conflictual. I think Olson's study and the work of others in the area of public goods genuinely address how and under what circumstances coalitions form, and they do not deserve to be excluded from studies of coalition behavior. (It should be noted that in eliminating conflict from the environment, Olson does not assume it out of politics but instead postulates it in the form of the "free-rider" problem, which occurs when individual members of a group do not contribute to the purchase of a public good from which all benefit.) To be sure, the editors may limit their domain of inquiry to whatever they please, but from the several objections raised by contributors in this volume to Riker's zero-sum assumption, a number would seem to agree with the position taken here about the utility of other assumptions about the nature of the environment in which coalitions form.

Contrary to the claims of the editors, many of the contributors very definitely do not subscribe to their definition of a *coalition* as "two or more actors in a coalition situation who have communicated and agreed to coordinate their actions" (p. 7). It is simply beyond belief that the scores and even hundreds of members of legislatures, political parties, and other elite groups considered in the twelve case studies—if these members are taken to be the actors in the above definition—have communicated with each other, much less agreed to coordinate their actions. To avoid limiting studies of coalition behavior to small groups, I would suggest that a more fruitful definition of a coalition might be "those actors or sets of actors with complementary interests who join together to

form a group that exacts some payment (not necessarily monetary) from its members."

I have dwelt on the editors' introductory remarks because (1) it is impossible in the space provided to review all the contributions, and (2) the direction (or lack of it) which the editors give to such a volume is crucial in evaluating its contents. Since the contributors to this sundry collection do not seem to have adhered to what common terminology and assumptions the editors did offer, I shall evaluate the contributions for which space remains independently of the guidelines the editors claimed to have imposed.

Roughly the first half of this volume is devoted to empirical case studies, several of which are excellent. Especially impressive are the systematic compilation and analysis of quantitative data on legislative and electoral coalitions in West Germany since World War II by Peter Merkl, on electoral coalitions in the 1951 and 1956 parliamentary elections in the Fourth French Republic by Howard Rosenthal, and among factions of the Liberal-Democratic Party in Japan since 1955 by Michael Leiserson. In the case of the latter two contributions, I found most enlightening Rosenthal's use of prior and posterior estimates in testing propositions about the size of coalitions, and Leiserson's candid discussion about the potential shortcomings of a focus on coalitions. Some useful refinements in Riker's theory are suggested by Phyllis Peterson's test of the size principle using local-election voting data from São Paulo, Brazil, and by Gerald Pomper's imaginative historical analysis of coalition-formation processes in the American Constitutional Convention of 1787.

In offering qualifications to Riker's size principle, several contributors to this volume show how other factors, like the desire to minimize ideological distance or the number of members in a coalition, may improve the predictive power of his theory and may also lead to other predictions, such as what the composition of coalitions and the distribution of payoffs (e.g., cabinet posts) among coalition members (e.g., political parties) will be. Unfortunately, most of these other factors adduced to explain coalition outcomes are not derived from the assumptions of a model and hence have a largely *ad hoc* flavor. An exception is Abraham De Swaan's contribution, but even his careful if informal derivation of consequences from a set of assumptions has a built-in circularity: the ideological compatibility of actors (derived from an ideological scale along which he assumes actors can be ordered) is determined

from preferences that presumably are in part based on previous party alignments, which in turn are used to postdict what ideologically compatible coalitions will form.

If ideology vitiates the operation of the size principle by precluding actors of dissimilar beliefs from joining the same coalition, it might be argued at the same time that ideological cleavages increase the perfectness of information, and thereby the applicability of the size principle, by making choices more predictable. Clearly, such plausible yet divergent claims made for the effects produced by ideology on the size principle leave its status as an explanatory variable unsettled. This ambiguity vividly illustrates the kinds of logical problems that can be created by the introduction of new "explanatory" factors in a model. These factors may lead to good predictions but, at best, confused explanations.

In some cases it is not clear what distinguishes "cases from four continents" in the first half of the book from "theoretical and methodological perspectives" in the second half of the book, which also includes reports (like De Swaan's contribution) on empirical findings. Logical and empirical analysis merge in Dina Zinnes's argument that Riker's theoretical assumptions are empirically untenable when applied to the study of balance-of-power systems. This argument is hard to evaluate, principally because it is so difficult to determine whether the assumptions of a theoretical model like Riker's are "true." Not only is the usefulness of a model often quite independent of the realism of its assumptions, but in testing a theory it is generally more productive to test the consequences of a model's assumptions rather than the assumptions themselves. More to the point, however, is that Zinnes's contention is based on empirical observations; it does not really come to grips with Riker's logical demonstration that there is an internal inconsistency in the balance-of-power rules of Morton Kaplan. If one accepts Kaplan's rules as descriptive of balance-of-power systems, then one must accept Riker's conclusion that such systems are inherently unstable.

For political scientists interested in the insights other disciplines may offer to the study of coalition behavior, the review articles on game theory by Leiserson, on economic bargaining and utility theory by E. W. Kelley, and on sociopsychological theories and research by Jerome Chertkoff are informative, as are some of the articles I have not discussed. Although I can commend several individual pieces in this volume, the collection as a whole suffers from

a lack of editorial integration and logical organization, the latter I'm afraid being in part a function of the present state of the art.

STEVEN J. BRAMS

New York University

Internal Colonialism and Structural Change in Colombia. Edited by A. Eugene Havens and William L. Flinn. (New York: Praeger Publishers, 1970. Pp. 252. \$15.00.)

Like many before them—from Marx to the originators of project Camelot—Havens and Flinn are concerned with the potential for revolutionary change. One of the editors' two "central assumptions" is that an examination of Colombia, a country with many variations, will allow the formulation of insights about change that can be applied to the Latin American region as a whole.

The other "central assumption" sets the tone for much of what follows: "It is the editors' contention that institutional change must take place before economic growth will be self-sustaining in Latin America; that change in the economic structure is a necessary condition for societal development, since economic relations directly affect other social relations." It is clear from later passages that the type of institutional change the authors see as necessary is not evolutionary, since institutional change is equated with "structural change"—an "alteration in the organizational arrangements of a total society." Thus, Colombia needs a thorough-going revolution.

A predictive note is added to the prescriptive orientation: "Eventually such a revolutionary change will occur and it will be the result of a certain set of processes and of organized activity." The reader is promised a documentation of the social changes occurring in Colombia and the accompanying pressures on the system as well as an analysis of the social processes leading to revolutionary change (p. 16).

This is a Herculean task—especially for an edited book only 250 pages long—and it is not surprising that the editors fall short of the mark. In the summary chapter, in which the analysis is to take place, eleven descriptive statements are offered, and of the eleven, six at most deal with *changing* conditions: a decline in economic growth indicators for major sectors of the urban, industrial economy relative to earlier periods; an "increasing man-land ratio" in the rural areas and heavy urban migration; "constant or declining employment opportunities"; normlessness on the part of economically marginal groups; a growing gap between the standard of living of large and small farm-

ers and the production of latifundia in new areas of colonization. But there is no attempt to integrate such findings into an "analysis of the social process" preceding the inevitable structural change. In fact, some of the findings themselves are either questionable (e.g., there is no convincing systematic evidence for the "normlessness" proposition) or require statistics to establish the magnitude of the problem (e.g., standard of living data).

Although "internal colonialism" appears in the title, it is not given extensive treatment in the body of the work—in fact it is rarely mentioned. Rather, it also is an "assumption" and "is not subjected to direct empirical examination." We are instructed that the "validity of the assumption can only be inferred from the analysis that follows." But given their definition of internal colonialism—an arrangement characterized by the presence of a small, dominant group of landowning capitalists versus a "large, subjected mass" with little or no mobility or participation—and the kinds of materials presented in the volume, such an inference is difficult to make. In fact, one of the chapters dealing with the occupational mobility of middle groupings (Chapter 13), tends to confuse the reader: Where do the middle segments fit into the two-class frame?

At least two additional disharmonies exist between the Havens-Flinn prescriptive macro-sociological orientation and the materials in the body of the book. First, many of the data are based on microstudies and hence make generalization difficult (see Chapter 6: "The Power Structure in a Shantytown" and Chapter 7: "Urban Lower Class Voter Participation and Political Attitudes"); this problem, of course is one that many of us have faced, with varying degrees of success.

Second and more significantly, some of the chapter materials, while prescriptive, are incongruous with the editors' deterministic, revolutionary frame. The two chapters by Dale Adams and Gustavo Jiménez's chapter on the Church best illustrate this. In Chapter 8 ("The Land-Tenure System: Antecedents and Problems") Adams acknowledges that governmental efforts are being made to "further accelerate changes" in the land tenure system and that the structural (politico-legal) obstacle is only one of many. He modifies the two-class latifundia/minifundia dichotomy by noting many different types of each and maintains that the importance of the land concentration problem has been overstated. In Chapter 11 ("Leadership, Education and Agricultural Programs") Adams notes that one of the evolutionary reforms he

emphasizes has been accomplished in "at least one rural area" (p. 195). Nowhere does he indicate a necessity for (or sympathy with) total revolution. Finally, Jiménez's materials confirm the Church as mainly a status-quo institution, but are not set in the context of revolutionary determinism.

If the book suffers from problems at the "design" level, the individual chapters provide some valuable insights, especially into the situation of the disadvantaged members of Colombian society. Harlan Davis concludes that the scarcity of modern public services in the countryside is due to the absence of "effective mechanisms to press the changes" and that poverty is partly responsible. But another cause of the situation is the "lack of autonomy and decision-making authority at the local level" (p. 82).

Joseph Thome elucidates well the problems caused by title insecurity—inability to obtain credit and problems in the transfer of land are two of the most serious. Havens and Flinn had concluded in a previous chapter that the subsistence farmer "receives little, if any, assistance from the introduction of new farming techniques" and that this "is partially due to the subsistence farmer's perceived inability to tolerate the risks involved if the new practice should fail" (p. 43). When these observations are added to those of Thome, it is not difficult to appreciate the adverse conditions experienced by the small farmer.

In the Havens-Flinn chapter on education (among the best documented) it is likewise easily concluded that access to rural instruction, especially at the secondary level, is limited to a small minority of school-age children. The authors attribute limited opportunities in education to "structural blocks" but fail to show why it is simply not the result of a paucity of national financial resources.

In his chapter on the "Occupational Mobility of the Middle Strata" Rodrigo Parra sets out two major aims: to analyze the "role these [middle strata] groups have played in changing the occupational structure of Colombia" and to "examine the manner in which Colombian society has influenced the development of the new middle strata" (pp. 215–216). That he does not make these analyses the focal point of his conclusions is obvious even on superficial reading. Parra does, however, confirm for Colombia what others have pointed out and analyzed for Latin America as a whole: that urbanization is not accompanied by proportionately increasing industrialization and that great increases in tertiary occupations are a result.

But as an entity, this book is disappointing.

By starting out prescriptively, it runs the risk of a double defeat. First, a more objective "scientific" orientation, the most sound basis for prescription, is discouraged. Thus, Adams prescribes more people with rural backgrounds in agricultural leadership programs without first trying to establish whether in the group he studied there were indeed significant differences in the behavior and attitudes of rural versus urban agricultural leaders. Second, the authors of prescriptive analyses are usually striving for policy impact. True, if the revolution comes, this book could conceivably play a part. But since Latin American nationalists and political influentials of varied stripes seem increasingly less willing to follow the advice of foreigners, the political effectiveness of this volume is likely to be minimal.

REID R. READING

University of Pittsburgh

Communists and Their Law: A Search for the Common Core of the Legal Systems of the Marxian Socialist States. By John N. Hazard. (Chicago: University of Chicago Press, 1969. Pp. 560. \$8.75.)

Professor Hazard has undertaken the gargantuan task of comparing the legal systems of the fourteen countries of the world that are ruled by Communist Parties, namely, the Soviet Union, its six fellow-members of the Warsaw Pact, Yugoslavia, China, North Korea, North Vietnam, the Mongolian People's Republic, Albania, and Cuba. The purpose of the comparison is to discover the basic common elements in "the family of socialist legal systems."

The starting-point is the 1957 Declaration of the Twelve Communist Parties in Power, which claims to lay down "basic laws applicable in all countries embarking upon a socialist course" (p. 6). These are (1) leadership of the Marxist-Leninist party, (2) state ownership of the basic means of production, (3) gradual Socialist reconstruction of agriculture, (4) planned economy, (5) a Socialist revolution in ideology and culture, (6) friendship among nationalities, (7) defense against external and internal enemies, and (8) proletarian internationalism. Professor Hazard asserts that the legal systems of the fourteen countries under consideration are designed to achieve these goals. He recognizes that "there is room for considerable variation in the choice of means . . . and the choices of the fourteen Marxian socialist states have been far from uniform . . ." (p. 8). Nevertheless, he believes that there is a "common core" and that the fourteen legal systems belong to the same "family."

To test this proposition, various basic aspects of the socialist legal systems are examined in turn: Constitutional and administrative provisions affecting the position of the leadership at the higher and intermediate levels; concepts of the nature of law and its relationship to the masses of people; land ownership and utilization; state ownership of production enterprises; individual rights of property, contract, and inheritance; family law, tort law, criminal law, and others. The method of examining each aspect is, first, to focus on the Soviet legal system,

since that is the inspiration for the others within the family. Materials taken primarily from the Polish, Yugoslav, Czechoslovak, and Chinese systems are then introduced, along with lesser quantities of data from others within the family, to provide the basis for generalizations about the common core (p. xii).

The author has his greatest difficulty in fitting China and Yugoslavia into this scheme. He states that they are at opposite ends of the spectrum of socialist law (p. 8), but again and again their legal systems appear to fall entirely outside his criteria. He seems to be on firmest ground in asserting that all fourteen states have systems of public law that rest on the foundation of a one-party system (p. 27), and that preservation of the leadership of the Party is "a cardinal point of the 'style' of the Marxian socialist system, if not *the* cardinal point . . ." (p. 33). Yet even here qualifications are needed. Stalin replaced party rule by the rule of the secret police, and apparently Mao replaced it at one time by the rule of the army (cf. p. 65). Moreover, it is doubtful that party leadership means the same thing in the Soviet Union, China, and Yugoslavia.

In other areas the diversities are even greater, so that what are described as the same broad goals or principles—"state ownership of the means of production," "planned economy," "collective use of the land," etc.—are only the same if they are viewed in isolation from the very different kinds of legal institutions used to implement them. The principle of "state ownership," for example, may be said to exist in two countries with different operative concepts of ownership, but then we are talking about two principles, not one. By analogy, one would hesitate to speak of "private ownership" and "market economy" as common principles of two countries with such diverse legal systems as, say, the United States and Pakistan. Is there a "family of capitalist legal systems"?

The difficulty is compounded by Professor Hazard's demonstration that in the area of pri-

vate law—individual rights of contract, property, and inheritance, family law, etc.—the Marxian socialist states have returned, for the most part, to traditional (i.e., non-Marxian) doctrines; and further, that the emphasis upon legal flexibility, simplicity, and popularity in the early stages of socialist development have been superseded everywhere—except in China—by an emphasis upon stability, formalism, and professionalism. Thus the principal distinguishing features of the socialist legal systems are said to be confined to certain areas of public law. Yet there is hardly a legal situation—in any system—in which public law and private law are not closely interrelated.

It is submitted that a quite different picture of socialist law would emerge if instead of comparing the legal systems of fourteen states, one were to compare the legal systems of two—say the USSR and China, or the USSR and Yugoslavia, or the USSR and Poland or Czechoslovakia. Then one would have to take greater account of historical factors as well as of political circumstances, social and economic conditions, etc. The “Marxian” features would then be seen in a better perspective.

This is not to say that the search for the distinctive elements shared by all Communist legal systems is not worth undertaking. On the contrary, we can only be grateful to Professor Hazard, the dean of Soviet legal studies in the United States, for turning his abundant energies to the task he set for himself. His book is one that every student of Communism should read, even though he may come away, as this reviewer did, with more questions than answers.

HAROLD J. BERMAN

Harvard University

God's Englishman: Oliver Cromwell and the English Revolution. By Christopher Hill. (New York: The Dial Press, 1970. Pp. 324. \$7.95.)

The Character of the Good Ruler: A Study of Puritan Political Ideas in New England, 1630–1730. By T. H. Breen. (New Haven: Yale University Press, 1970. Pp. 301. \$10.00.)

Here are two studies in the erosion of religious zeal and the secularization of political life. Of the two, *God's Englishman* is by far the more interesting, not only because it deals with greater men and more important events. Christopher Hill has in fact written an excellent biography of Cromwell. But the special value of his book for students of politics lies in the way he fixes on, worries about, and analyzes

the English Thermidor; that is, the rightward drift of the revolution after the execution of the king. Cromwell, who had pressed for the king's death (“I tell you, we will cut off his head with the crown on it”), is at the same time the great English Thermidorean. Though the return of England's “natural rulers” is not something he plotted, as some of the Levellers thought, the tale of that return might well be told in terms of his hesitations, enthusiasms, and disillusionments. It might also be told in terms of those objective conditions that set limits to what any revolutionary can achieve. Hill tells both these stories: his book combines conventional biography and Marxian analysis, and while he has no new research to report and no new theory of the revolutionary leader, the combination brings us quickly to the heart of the dilemmas such a leader always and perhaps necessarily faces.

“Underlying all the disputes between Oliver Cromwell, the Levellers, and his parliaments was one fundamental problem: the problem of the electorate” (p. 206). It is, one might say of a less “constitutionalist” polity than England, the problem of a social base. Neither the political nor the religious radicals (and Cromwell was allied with both at different times in his career) had the support of a sufficient number of Englishmen. There was never a majority for democracy, toleration, or godliness in the seventeenth century, either among the propertied classes or among the mass of the people. For all his sympathy with the Levellers, Hill sees clearly enough that had they seized power, they would have “faced the necessity of holding the rest of the population down” (p. 207). The point is equally true of the Puritan “saints” of the Barebones Parliament, who could not rule without the army, and of course, it is true of the officers themselves, the very embodiment of what Andrew Marvell called “the forced power.” Hill does not mean to say that England's “natural rulers” could govern the country without violence, but they required a great deal less of it than any alternative group, and ultimately any government which did not intend systematically to destroy them, had to make its peace and admit them to power.

The crucial moment for Cromwell, Hill rightly argues, was not the defeat of the Levellers at Burford, but the dissolution of the Barebones Parliament. God's people, and not the people simply, were always the basis of his political faith, but God's people were at war with one another and with the propertied classes, and these were not wars in which Cromwell found any providential guidance. After 1653,

he no longer dreamed of the triumph of the saints: "he saw himself as a constable whose task was to prevent Englishmen from flying at one another's throats" (p. 143). His it was, finally, to defend law and order—and property. It is the same task that John Locke only a few years later described as the central and virtually the sole function of government.

T. H. Breen's book traces a hundred-year-long movement "from piety to property" in the writings of New England Puritans. He is, then, concerned with developments directly analogous to those which took place in England but, because of the heightened tension and accelerated pace of revolutionary politics, in a much briefer time span. The analogy is not one that he discusses, however; nor does he have much to say about the movement as a general phenomenon; nor, again, is he given to speculation as to its possible causes. His is a resolutely conventional history; it is always careful and it is highly competent; but it must appear to social scientists as a record of missed opportunities. Though he retells some of the political history of New England and grants that, in this case at least, "theory seemed to joggle along behind practice," he is content mostly to joggle along behind theory. And so he offers us no insights into the complex and interconnected character of the secularization process.

Indeed, it is hard to make out from this book just what secularization is: what precisely is it that survives, what is it that disappears or is transformed in religious thought? More concretely, when a study of Puritan ideas ends with a consideration of the Franklin brothers' *New England Courant*, is its author pointing to the death of Puritanism or to its survival in strange, distorted, partial, in a word, *secularized* forms? Weber thought one needed Puritanism to account for Benjamin Franklin's economic thought, but is the faith of John Winthrop central to an understanding of *Cato's Letters*? Perhaps it is, but Breen does not tell us that it is, or why. Only once does he address himself to this sort of question—at the end of his fifth chapter in a summary statement that is worth quoting at length:

New Englanders, Breen writes, "intermixed the language of property and morality, as men did not in England; and because of this blending, colonial attitudes about government acquired a unique character. The citizens came to see any invasion of private estates as sinful acts prompted by the magistrate's impiety; and during the American Revolution the mingling of material and spiritual values led Puritans to condemn taxes levied by the idle,

sumptuous rulers of Britain as both arbitrary and immoral (p. 202).

That is an important point, and a book which argued it at length and in detail (and with the necessary comparisons) would be an important book. It is not the book Breen has written.

MICHAEL WALZER

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The Politics of Reform in Peru: The Aprista and Other Mass Parties of Latin America.

By Grant Hilliker. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 201. \$10.00.)

Prior to the mid-1960s, few monographs dealing with Peruvian politics had appeared in English. Then in 1965 came Payne's *Labor and Politics in Peru*, followed in 1969 by Larson and Bergman's *Social Stratification in Peru* and Astiz's *Pressure Groups and Power Elites in Peruvian Politics*, and in 1970 by the English translation of Bourricaud's work of four years earlier, *Power and Society in Contemporary Peru*. Mr. Hilliker's study of the Peruvian Aprista Party—the subject of this review—draws deeply on all of these, as well as on much of the now considerable literature in Spanish.

The Peruvian Aprista Party has been chosen as an example of a "mass reform party" (p. 11), as well as one of the earliest parties of the so-called "Democratic Left" in Latin America. Chapter VIII specifically includes comparative information on the Bolivian National Revolutionary Movement (*Movimiento Nacional Revolucionario*) and the Venezuelan Democratic Action (*Acción Democrática*), two other mass reform parties; but the body of Hilliker's study focuses on the Peruvian Aprista Party or APRA (*Alianza Popular Revolucionaria Americana*). After two introductory chapters, the reader is presented with chapters on Aprista party history, party organization, tactics, and the party's involvement with the agrarian reform issue. In most of these chapters, the author attempts to relate the past activity of the APRA to the process of "national development" in Peru.

The Politics of Reform in Peru was written by a Foreign Service Officer whose work reflects the ideology and rhetoric of the New Frontier and the early Alliance for Progress. As a self-described "twentieth century liberal" (p. 187), Mr. Hilliker quite openly expresses his preference for the non-Marxist "Democratic Left" in Latin America and for the politics of reform rather than the politics of more

radical change. Somewhat unfortunately, at times he allows himself to get bogged down in rather vague discussions of "development" without ever succinctly defining what he means by the term. A far more serious omission is his failure to show how the concept of development really helps to improve the reader's understanding of the APRA. While discussions of the "politics of development" have become quite fashionable in recent years, such discussions ought to be reserved for cases in which they have something special to add.

This reviewer finds Mr. Hilliker's characterization of the Peruvian APRA as a "reform party" a debatable view. Little overt mention is made of the apparent conservatism that has affected APRA in recent years. Despite its reformist program of 1931, which never has been formally abandoned, the party has not shown itself through its actions to be greatly concerned with current social reform. The support given by the APRA to the conservative government of Manuel Prado between 1956 and 1962, and the party's parliamentary coalition from 1963 to 1968 with the right-wing National Odrista Union (*Unión Nacional Odrísta*) of ex-dictator Manuel Odría could be interpreted by an unfriendly critic as political opportunism. The strategies employed by APRA since 1956 might be justified by the party's need to accommodate the extremely hostile conservative elements in the military which previously had blocked APRA's way to the key office of the Presidency; but such an explanation will find doubters both in Peru and in the United States.

On a more positive note, Mr. Hilliker should be complimented for his diligence in using the available secondary literature on Peru. While not every reader will agree with all of the author's interpretations of this literature, such differences of opinion probably are inevitable in any study. The author's narration of the APRA's most recent periods of legality (1945-1948 and 1956 to the present) represents a highly plausible account of the political events of those years as they have affected the party.

The useful summary of the literature found in *The Politics of Reform* well might have been supplemented by more original data than is found in the text. The limited interviewing that was done adds little to the material now available elsewhere. In particular, this reviewer would have liked to have seen an analysis of the internal workings of the party and how the divisions in the top leadership have reflected

the party's various choices of tactics in the past. The chapter on organization (Chapter IV) really only describes in broad generalities the organizational framework of the party. Nothing is said about the leadership except to mention the position of party founder and *Jefe*, Haya de la Torre, now in his seventies. What of the men below him who have had an important say in day-to-day operations of the party during Haya's prolonged stays abroad? What about the party's future? No attempt has been made to describe how the military regime now in power may be undercutting APRA's base of support in the Aprista "solid North" with the *junta's* own agrarian reform program.

In summary, Grant Hilliker has written a book describing the APRA Party, with considerable attention to its past attempts at winning power in Peru. The material added in the penultimate chapter on two other "mass reform parties" places the Peruvian data in a limited comparative focus. If the reader seeks a competent analysis of the political history of the APRA, he should be happy with Hilliker's book; if the same reader also seeks new data on the party or a provocative analytical framework, he will be disappointed.

EDWARD C. EPSTEIN

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The Cameroon Federation: Political Integration in a Fragmentary Society. By Willard R. Johnson. (Princeton: Princeton University Press, 1970. Pp. 426. \$12.50.)

Early in 1961, citizens of the Trust Territory of the Southern Cameroons backed into one of the oddest federations on the contemporary political map. The resultant Federal Republic of Cameroon exhibits all the usual splinterings of tropical African states: ethnic diversity; contrasts between "bush" villages and glittering capital; juxtaposition of traditional subsistence farming, plantation agriculture, and major extractive industry; arid steppe and rain-soaked forest. Even beyond these contrasts, Cameroon adds others. Alone among tropical African states, it boasts two official tongues, three former colonial overlords, and a federal union that resulted to a substantial extent from decisions of the United Nations General Assembly.

With only a decade of independent and federated existence, Cameroon can scarcely claim to be a mature political union. The integration with which this study is concerned is one of process, not one of conclusive result. Professor Johnson aptly deems Cameroon a "fragmentary" society—not fragmented, since its current

structures are building toward unity. The harmonization achieved to date, however, has levied its main toll upon the former Southern Cameroons (now West Cameroon), whose political processes gradually are being integrated with—or, perhaps more accurately, into—those of formerly French-administered East Cameroon.

Professor Johnson sees integration as “the process whereby political interaction becomes more systemic.” I stumbled over this wording. Why not “more systematic”? Since a system is by definition total and englobing, an action can become only systemic, not “more” systemic. This quibble points to a broader problem. The weaknesses in Professor Johnson’s treatment stem not from his detailed understanding of a fascinatingly complex society, but from what I am tempted to call the unintegrated study of political integration. Is political integration most fruitfully conceptualized as a process? Or as a goal, reached more or less completely? Should the scholar’s perspective be fixed upon a would-be “integrated” system that has yet to come into being? Professor Johnson prefers this last possibility, since his theoretical focus is upon interdependency. Such interdependency is now only germinating in Cameroon.

The theoretical framework of *The Cameroon Federation* incorporates complementary and conflictful political actions (most notably the pursuit of political objectives on the basis of common rules), conjunctive and disjunctive political actions (“weaving behaviors of each citizen into the fabric of national political life”), and the mix of complementarity and conflict within, and among, the various political processes of the federation. The factors thus are complex, varied, and at times confusing. Professor Johnson shifts from individuals to institutions to data on economic interaction, with transitions that are occasionally jagged. What emerges clearly are the facts that one of the two federal partners has borne the brunt of adjustment; that an “integrative advance” has occurred within a decade as the result of implementing a Federal constitution giving restricted authority to the two federated states; and that unification had been given little serious attention by either partner prior to the United Nations-mandated plebiscite.

Professor Johnson rebuts the usual factors that scholars of political union ascribe to successful integration (however “successful” or “integration” may be defined). Ethnic affinities were insufficient to constitute a basis for national political awareness and a coherent political process. The dim memory of German ad-

ministration, though central to the rhetoric of “Kamerunian” nationalism, did not provide other than a verbal foundation for a “real nation.” Language, education, and religion differed markedly in the British- and French-ruled sectors. No manifest economic advantages seemed likely to accrue from unification. Even a “pan-Kamerun” organization to press for unification did not emerge prior to the plebiscite, because most political leaders in Cameroon saw reunification as a tactical maneuver, not a broad strategy.

In point of fact, the formation of the Federal Republic of Cameroon resulted in substantial measure from the wording adopted by the General Assembly for the 1961 plebiscite. Union was imposed upon the inhabitants of the British-administered Trust Territory, who could choose between association with Nigeria or federation with the then-Republic of Cameroon—and could not select the separate independence favored by many. It was, in Professor Johnson’s words, the “autonomous power of the mediating agency” that made the Federation’s birth possible.

Given the limited extent of prefederation integrative ties, the “integrative advance” is indeed impressive. But it has been an advance felt largely by the smaller, weaker partner in the new Federal Republic. The distinctive role of the United Nations would seem to have few parallels in the annals of political union. Hence, generalizations from the Cameroon example must be drawn with caution. Professor Johnson is appropriately chary about sweeping statements. He concludes with a remark applicable to all splintered, fragmentary African states: the apparatus of state can be built more rapidly than the appurtenances of nation. Given time and wisdom, there is an “even chance” that the “One Cameroon Nation” will prove that a Cameroonian political system has emerged.

CLAUDE E. WELCH, JR.

SUNY at Buffalo

A Year Is Eight Months. By Journalist M. Introduction by Tad Szulc. (Garden City, N.Y.: Doubleday & Co., Inc. Anchor Books Edition, 1971. Pp. xi, 260. \$1.95, paper.)

The Czechoslovak Experiment, 1968–1969. By Ivan Sviták. (New York and London: Columbia University Press, 1971. Pp. 243. \$10.95.)

The great awakening in the political life of Czechoslovakia and its tragic aftermath was one of the larger issues of 1968 on the world

political scene; it produced drama and even ecstasy; it also produced important lessons in the dynamics of international politics of our time. Judged by the latest outcome of events, the bold and ambitious undertaking to freshen the springs of Czechoslovakia's life and to bring about needed reforms is no longer a live issue. The perception and interpretation of the 1968 events, however, is a subject worthy of reflection and analysis.

Writing with the flair and commitment of dedicated reformers, and the insight of knowledgeable participant-observers, Journalist M and Ivan Sviták present to the English-reading audience two differing Czech interpretations of the political currents in their native country.

M's opinions and views are less critical of Czechoslovakia's leaders in 1968 and more approving of the main streams of action. Sviták, on the other hand, from the beginning to the end of the windy Prague Spring shows consistently anti-establishment views, harsh judgments, impatience with the country's leaders, and a strong preference for coping fast and radically with a wide range of social and political issues.

M's book illustrates the dilemmas of reformers working in a rapidly changing political climate, urging moderation, and looking at the political landscape with a certain degree of prudent optimism. Sviták's volume illustrates the dilemmas of political dissidence.

The author of *A Year Is Eight Months* (originally published in German as *Die Kontrollierte Revolution*) is identified in an introductory note by Tad Szulc as "a true-believer Communist" and "a highly active and influential member of the Prague newspaper fraternity" during the Dubcek era. His book is an informative and a very readable work portraying well the unprecedented political activity in Czechoslovakia of 1968. He takes us to the swiftly changing political stage to see real action and to appreciate the importance of the tragic element in the affairs of nations, and he leaves us with the message that hope goes on.

In M's interpretation, three main causes of tension and conflict gave rise to a revolutionary change: economic stagnation, nationalist grievances of the Slovaks, and the suppressed rights of the citizens. The policy of repression could not arrest the progressive decay of the whole system. Under Antonin Novotny's tutelage, the Communist Party was leading the country into a blind alley. Hence, the upper echelons of Party bureaucracy approved the plot to unseat Novotny and to end his regime of personal power. M describes the ideological ferment, the

maneuvering of the Party leadership and its apparatus, the political breakthrough against dogmatism and conservatism, and the emergence of Alexander Dubcek's "socialism with a human face." But the ferment gave rise to confusion and disorientation among Party members. The disarray was reflected in the district Party conferences and the meetings of the higher echelons. Three strong and influential groups formed within the Party, each with interests and outlook of its own: the reformers, arguing for a new, progressive political and economic model that would become an alternative to bureaucratic socialism; the anti-reformation bloc; and the Dubcek center group.

In the struggle over the question of changing the whole structure of power, the communication media became the main vehicle for advancing the cause of the reformers. The public airing of differences among Party factions and their infighting deepened the political crisis and weakened the Party's ability to lead and mobilize the people.

Further complicating the factional struggle among the several power elites was their interaction with the Soviet power elite. The turning point came with the publication of the Warsaw letter, signed by the Party leaders of the U.S.S.R., Poland, East Germany, Bulgaria, and Hungary, and addressed to the Czechoslovak Party Central Committee. The letter closed with an ultimatum to mobilize immediately and to fight decisively against all threats to the foundations of the Communist regime in Czechoslovakia. In a resolute reply, Czechoslovakia's leaders revealed the depth of their conflict with the Warsaw Five. The reply unified the Party and brought it great popular support. At the end of July came the fiasco of the meeting with the Soviet Party Presidium in Cierna, and three weeks later the military intervention by the Warsaw Five.

M relates how the Party placed itself in the forefront of the resistance and fought a despairing battle to preserve its political program of "socialism with a human face." But the conservative elements, whose fate seemed to be sealed just before the intervention, gradually regained control over the Party and provided the necessary ingredient which enabled the Soviet regime to accomplish successfully the main purpose of the military action.

Ivan Sviták, the author of *The Czechoslovak Experiment, 1968-1969*, identifies himself as a Marxist philosopher and socialist humanist. He was expelled from the Communist Party in 1964, and in the post-Dubcek period lost the position of a Fellow of the Institute of Philoso-

phy of the Czechoslovak Academy of Science and a lecturer in modern philosophy at Charles University, Prague, as well as his Czechoslovak citizenship.

Sviták states that all the documents presented in his volume were written for the radically democratic Czech intelligentsia, especially the leftist democratic students. With the exception of the integrating Prologue, his essays have all been previously published, most in Czech newspapers during the Prague Spring. The book has an excellent selection of political cartoons, a welcome contrast to the earnest style of Sviták's materials.

Sviták argues that the Czechoslovak experiment was only the first stage of a democratic revolution, an overture to a radical structural change. In the first half of 1968, the goal of a complete transformation of the system, advocated by the radically democratic elements of the intelligentsia and the university students, did not have a wide backing among the main strata of the society. A broad-based revolutionary movement came into existence just before the Soviet military intervention. Sviták comments that the interpretation of the Chinese press, which called the experiment a factional struggle between revisionist power elites, is closer to the truth than the Western and Soviet interpretations.

Early in the Dubcek period, the author published his main theses supporting the movement for structural reform and civil rights. He justified his crusade against the bureaucratic dictatorship as a struggle against the perversion of the humanistic aims of Karl Marx's socialism. Marx, the messenger of freedom, provided no theoretical support for requiring the Communist Party to take the leading role. Sviták warned that "it must be clearly understood that we support the program of the new team, not their personalities, and that their maximum program is our minimum one" (p. 25); he also warned that "the slogan of 'democratization' is an improvisation born out of abnormal circumstances involving personality conflicts within the Politburo" (p. 26). He urged that no one who was a member of the National Assembly in the spring of 1968 should be elected and that the whole power structure should be liquidated and replaced by a more pluralistic political system, more sensitive to human needs and rights.

Sviták was aware of the danger arising from the endeavor to emancipate Czechoslovakia fully from the Soviet political model and to begin the process of transformation of the system as "an exceptionally important preview of social processes which would take place in the

Soviet Union in the seventies" (p. 37). In an article written early in August, 1968, he analyzed the power rationale of Soviet intervention and concluded that "the overriding reasons for intervening in the internal affairs of Czechoslovakia rest on ideological and prestige considerations" (p. 132). The decisive reason was the well-founded fear of infecting other socialist countries with the ideas of democracy: "The Soviet leaders are willing to tolerate, in Eastern Europe, any kind of benevolent dictatorship but not political freedom based on respect of fundamental human rights" (p. 133). In his opinion, a more moderate course might have deferred but would not have prevented U.S.S.R.'s military action to restore "the elitist version of 'Panzer-Marxism'" (p. 212).

According to Sviták, the right course was the radicals' attempt to accelerate the internal democratization of the regime; there was no way of preventing the military intervention except by accepting the risk of war:

The Czechoslovak experiment foundered on its own paradoxes—on the bizarre role assumed by the Communist Party, which tried to act in the guise of a democratic organization, on the naive belief of the power elite that its adversaries could be drowned peacefully in a wave of popular enthusiasm for democracy, and on the clash between the illusion of Marxist humanism and the reality of mechanized divisions. . . . The Czechoslovak experiment foundered on its effort to accomplish the impossible: the Europeanization of Russia and the humanization of Soviet Communism (pp. 214–215).

Will there again be opportunities to sponsor programs of political renewal in Czechoslovakia when the international climate is more favorable than in 1968? Sviták, like M, has not given up hope. By giving divergent viewpoints on the critical policy issues, the two volumes make a fascinating contribution to the understanding of the rise and fall of reform movements in Central and Eastern Europe.

JOSEPH HAJDA

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The Jews in Soviet Russia Since 1917. Edited by Lionel Kochan, with an introduction by Leonard Schapiro. (London: Oxford University Press, for the Institute of Jewish Affairs, 1970. Pp. 357. \$7.95.)

Considering the large contribution scholars of Russian Jewish ancestry have made both to the social sciences and to East European area studies it is rather surprising that so little detailed research has been devoted to the Russian Jews as a social group. The student of Soviet nationalities can turn to an array of mono-

graphs dealing with almost every facet of contemporary or past development of the Ukrainians; materials on the Armenians and the Baltic nationalities are nearly as comprehensive. In contrast, treatment of the Jews of the Russian Empire and the Soviet Union has tended to take the form of polemics, descriptive history, or literary criticism.

The present volume is a partial remedy for this situation. While the symposium includes distinguished Americans like Maurice Friedberg, Professor of Slavic Languages at Indiana University, and William Korey of B'nai B'rith, the sponsor and a large majority of the contributors are British. In their treatment of "The Jewish Population: Demographic Trends and Occupational Patterns," the renowned economist Alec Nove and his Glaswegian colleague J. A. Newth provide the best behavioral analysis of any aspect of the Russian Jewish collectivity. Most of the articles, however, employ literary or descriptive approaches rather than behavioral science methodology. Consequently, these articles provide material for conceptual analysis rather than analysis itself. The service they render is, however, by no means negligible, for anyone (like the present reviewer) not of the Russian Jewish culture has had enormous difficulty even following its terminology and institutional complexities. Thus the articles on "Soviet Jewry," "Hebrew Literature in the U.S.S.R.," "Yiddish Literature in the U.S.S.R.," and "Jewish Religion in the Soviet Union," apart from the intrinsic interest of their subjects, will probably be most useful to political scientists as guides to using literature as source material.

Friedberg's two articles on the Jewish component in Soviet literature, on the other hand, provide more directly utilizable information. While the author approaches his subject in a literary manner, his findings can readily be related to the conceptual framework of social science. For example, Friedberg's analysis of successive periods of treatment of Jewish themes in Soviet literature suggests a general hypothesis on the relation of a highly centralized mobilization regime to an established minority culture. First Jewish characters appeared frequently and sympathetically, but were effectively drained of specifically Jewish tradition and cultural values; later (in the postwar Stalinist period) Jewish characters and themes nearly disappeared from Soviet literature, except in the veiled, pejorative form of "homeless cosmopolitans"; most recently (as in Andrei Siniavsky's works) Jews reappear in non-Jewish dissident literature as the symbol of rejection

of the inhumane values of the regime.

Equally useful, from a different point of view, is "The Jews in Russia at the Outbreak of the Revolution." While S. Ettinger gives essentially a descriptive history, he also provides invaluable statistical data which up to now have been very difficult to obtain. Using primarily older Russian-language sources, Ettinger analyzes the natural increase, internal and external migration, and occupational distribution of Russian Jews in the two decades preceding the Revolution; while not resolving all uncertainties, his analysis enables one to derive concrete indicators of the transition from a traditional diaspora to one which was modernizing at a very rapid rate.

Nove's and Newth's conceptually sophisticated statistical treatment of the Soviet period is much more detailed than Ettinger's. Relying mainly on intensive analysis of Soviet data, the authors are able to show, with a high degree of probability, the dimensions of war losses, migration from the "Pale" to internal Russian areas, changes in age and sex ratios, and occupational distribution. They convincingly argue that there is little evidence that the declining proportions of Jews in general higher education and the skilled professions indicate discrimination, although the authors are careful to point out that this conclusion does not preclude the inference (supported by nonquantitative evidence in other articles) that Jewish quotas may exist in elite occupations and training institutes. Nove and Newth are even able to throw some light on the difficult question of intermarriage. Here, however, their study is handicapped by a common feature of nearly all of the articles in the symposium: Composition preceded publication by several years. As a result, Nove and Newth did not have access to the detailed, revealing Jewish demographic information which has appeared in N. V. Kurman and I. V. Lebidinskii, *Naselenie Bol'shogo Sotsialisticheskogo Goroda* (1968, on Kharkov) and A. I. Kholmogorov, *Internatsional'nye Cherty Sovetskikh Natsii* (1970, on Latvia).

The volume does contain a more up-to-date article, "After the Six-Day War," by Zev Katz. But, like preceding articles in this volume dealing with political decisions ("The Biro-Bidzhan Project" by C. Abramsky, "Soviet Jewry in the Second World War" by R. Ainsztein, and "Antisemitism in Soviet Russia" by B. D. Weinryb), Katz's treatment documents the Soviet regime's anti-Semitic and anti-Israel policies without really analyzing the reasons for such policies. The political scientist will find this lack of analysis in depth disappointing, but

it is perhaps too much to expect a volume of this nature to present a particular theoretical interpretation which would presuppose, in effect, a general theory of the nature of Soviet politics. As the editor points out, "the intent of this book has been historical" (p. 337). In a few places, however, greater clarity even in the chronological presentation is needed. For example, Weinryb overlooks the very significant fact that the most atrocious act in the purge of Yiddish cultural figures, the official murder of Solomon Mikhoels, occurred in early 1948 *before* Israel was established, and that, conversely, the antic cosmopolitan campaign became vehement only after Andrei Zhdanov's death.

For the earlier period, Jacob Miller presents an excellent, though largely descriptive, survey of the evolution of Leninist-Stalinist theories about the Jews, and Korey provides a comprehensive discussion of the legal status of the Jews. Taken together with the other excellent articles of this volume, their work will enormously facilitate comparative studies on ethnic groups in the U.S.S.R.

JOHN A. ARMSTRONG

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Legislatures in Developmental Perspective. Ed. by Allan Kornberg and Lloyd D. Musolf. (Durham, North Carolina: Duke University Press, 1970. Pp. 590. \$15.00.)

This is an impressive symposium which is mistitled, or at least the contributors do not agree on what the title signifies. For some of them the focus is on whatever relationship exists between legislatures and development (economic, political or both). For others the central issue seems to be whether legislatures can be perceived within a developmental, in this case powerless to powerful, continuum. Additionally, agreement is lacking on what legislatures are or do. On the one hand the contributors do not cavil at the assertion that it is "principally through the enactment of statutes that the modern [Western] legislature has derived its saliency" (p. 7). On the other hand Robert A. Packenham, who conducted a survey of functions performed by these institutions, concludes that "most [Western legislatures] do not allocate values, or at least do not have this as their principal function. Other functions . . . [presumably non-legislative, but ambiguity arises] seem to be more important" (p. 522).

We are thus confronted with a monumental muddle. Do legislatures legislate ("enact statutes," "allocate values")? A tentative "maybe" would seem to be the accepted answer. Should

these "institutions"—bundles of interests, assemblies of representatives, etc.—legislate? Again, the response appears to be less than clear. It really depends upon what other goals are given precedence. Legislatures, especially if they are nonrepresentative by virtue of exclusiveness or a skewed electoral system, may impede rather than encourage political or economic development.

It is all very curious, and for me at least, highly instructive and encouraging. Let me explain. First and foremost, the symposium reflects a revival of serious scholarly research on legislatures. Much has been learned about the U.S. Congress and British House of Commons, but even about them, gaps in our understanding remain. Anyone who wishes to do further research in this area would do well to study Musolf and Riggs' chapter on "Comparative Legislative Studies and Services: Notes for a Program."

Kornberg and Musolf begin the volume with an introductory essay providing the background for the symposium and a précis of the contributors' major findings. Chapters 2 through 9 are essays on legislatures of selected countries (U.K., Canada, U.S.A., India, Chile, Lebanon, Kenya, and Philippines), representing a broad geographic mix as well as "developed" and "developing," and "parliamentary," "congressional," and "hybrid" models. Despite the undoubtedly prodigious efforts of the editors, there is an inevitable divergence of style and mode of analysis which can be readily explained by the disparity of basic knowledge concerning these institutions and their inmates. Compare, for example, Crick's mature reflections on the British Parliament with Agor's heavily footnoted protestations concerning the "highly developed" character of the Chilean Senate; or Kornberg's severe recriminations regarding the Canadian Parliament with Stauffer's comments concerning the Philippine Congress's vitality "in keeping democratic politics alive." Common criteria or expectations appear to be lacking, and a rather pernicious condescension is apparent: One can evidently criticize well-established legislatures and freely discuss their shortcomings, especially the perceived chasm between expectations and performance; but it is apparently not quite proper to find fault with similar institutions in less hospitable climes; nay, their performance must be extolled.

Fortunately, Packenham provides healthily abrasive constraints on the occasionally uncritical enthusiasm for legislatures and their possi-

ble roles in political and economic development. Basically his message is that our knowledge of legislatures within the context of specific domestic politics, and of the relationship between improving legislature services (research and administrative staff) and political development is as yet too slight to allow us to rush in with projects to strengthen legislative bodies:

I have not written this paper to argue that under no circumstances should a program of technical assistance for legislative services be forthcoming. I have tried to suggest that these circumstances will rarely be found, and no such program should be undertaken until the kinds of questions raised here have been confronted and more satisfactorily answered (p. 582).

Such self-restraint is laudable, especially since championing such a program might well provide an A.I.D.-funded pot of gold at the end of the rainbow.

The symposium contains four analytic essays. Two deal with problems of professional staffs for legislatures in general and for committees, with the experience of the American Congress used as the principal data base. In the third essay John Grumm seeks to determine the "structural determinants of legislative output," with American state legislatures used as the data base. Finally, Malcolm Jewell assesses the utility of role analysis in probing "Attitudinal Determinants of Legislative Behavior," drawing also on American experience. Jewell's conclusion that role analysis "is a flexible technique, tested in previous research, and appropriate in comparative analysis" is a bit worrisome if for no other reason than that it implies an uncritical acceptance of particular research methodologies which may well be culture-bound.

Nonetheless, as noted at the outset, this symposium is a worthwhile contribution to the study of legislatures. Americanists will find it useful in reopening questions they might have considered as having been settled long ago. Comparativists will gain from it renewed challenges to answer questions that may be extraordinarily difficult to research. All this is to the good.

HANS H. BAERWALD

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Essays on Gandhian Politics; The Rowlatt Satyagraha of 1919. Edited by R. Kumar. (Oxford: The Clarendon Press, 1971. Pp. 347. \$10.50.)

The Rowlatt Satyagraha marked an impor-

tant stage in the growth of Indian nationalism: it propelled Gandhi into dominant national leadership and ushered in an era of mass politics. The Rowlatt Bills, so called after Sir Sidney Rowlatt who headed a government committee to investigate seditious activities in India, were drafted to confer upon the Government of India extraordinary powers to deal with "anarchical and revolutionary crime." Gandhi launched the first nationwide satyagraha movement in April 1919 with the immediate objective of forcing the withdrawal of the one enacted Bill and preventing the passage of the second by the Legislative Council. His long-range purpose was not only to mobilize popular support against British rule but also to create in Indians the self-reliance necessary for sustaining, as well as achieving, self-rule.

Nine of the ten essays published here were first presented at a symposium held at the Australian National University in 1966, and they reflect the expanding Australian interest in the study of modern India. From these essays emerges a picture of the political conditions in India during 1919. They explore such questions as the nature of the social and economic groups which responded to Gandhi's initiative, and the local discontents which Gandhi channeled into a movement of protest against British rule; and they assess the qualities of the primary and secondary leadership of the movement in different parts of the country.

The final essay by D. A. Low originally appeared in the *Journal of Asian Studies* (February 1966) and is addressed to the first non-cooperation movement which began a year after the Rowlatt Satyagraha. Dr. Low's study, together with others in this volume, adds weight to his opinion that the most fruitful advances toward an understanding of the processes by which India and Pakistan achieved their independence may well be made through the study of "particular episodes and particular themes" (p. 298). Dr. Low focuses upon government opposition to the nationalist movement and makes the point that with increasing interest in the movement itself there may be some danger that the changing strategies and tactics of the government response could be neglected.

This book is rich in descriptive historical detail of political action, both within the movement and between satyagrahis and government forces. The well documented essays on the action as it developed in the cities of Bombay (J. Masselos), Delhi (D. W. Ferrell) and Lahore (R. Kumar) and in the Central Provinces and

Berar (D. E. U. Baker) and Gujarat (K. L. Gillion) add substantially to an understanding of the complexities of the movement. Each also provides an analysis of the respective political and social backgrounds into which satyagraha was introduced.

In his introductory essay, Dr. Kumar observes that the Rowlatt Satyagraha illustrates Gandhi's ability to draw together individuals belonging to different castes, communities, and religions and that "what immediately strikes the attention of the social historian is the conspicuous absence of the language of class in the appeals which Gandhi issued in connection with the Satyagraha . . ." (pp. 12-13). Dr. Ferrell concludes that the "primary significance of the Rowlatt Satyagraha in Delhi was that, given an appropriate issue, it was possible to transform a heterogeneous political community into a homogeneous political society" (p. 235).

As Dr. Masselos notes, Gandhi considered the technique of satyagraha "not only the most effective method of protest against the Rowlatt Bills . . . but also it was the only method of preventing the outbreak of violence." Gandhi sought to provide an outlet for the passions of young Indians, passions which otherwise would, he wrote, manifest themselves in the form of "the powers of vengeance and the doctrine of the little Bengal Cult of Violence" (p. 174). Gandhi was astonished at the intensity and extent of the movement's support. With the outbreak of violence in several places, Gandhi suspended the movement and went on a penitential fast. H. F. Owen critically evaluates Gandhi's leadership. He examines the new problems of organization which faced Gandhi in a nationwide campaign and examines the difficulties of exercising control and restraint over newly mobilized social groups in circumstances of agitational political activity. His essay contributes to an understanding of what was, in strictly Gandhian terms, a failure of satyagraha. Detailed critical evaluations of the secondary leadership are presented in the several essays on city and regional activities.

Two background essays treat respectively traditional influences upon Gandhi and his earlier development of satyagraha. A. L. Basham concludes that Gandhi was more successful than earlier reformers in giving non-Indian ideas a thoroughly Indian character by building on the foundation of his own tradition and relating new modes of approach to earlier doctrines and concepts. As Gandhi himself had pointed out, there were only two major planks

in his platform which had no prototypes in India's past: the dignity of manual labor and the emancipation of women.

P. H. M. van den Dungen's penetrating essay challenges the view that the Rowlatt Satyagraha marked the turning point for Gandhi. He explains why Gandhi's position and leadership of this movement did not represent a sudden shift of loyalty from that reflected in his readiness, in 1918, to recruit for the army. Recruitment was part of his program to restore Indian manhood. Gandhi had, in fact, advocated the inauguration of satyagraha (to secure amendments in the Montagu-Chelmsford reforms) at the same time that he pressed for recruitment. Dr. van den Dungen goes to the heart of the matter in explaining central but often overlooked elements of satyagraha. He touches upon "the corollary of the ethics of nonembarrassment—helping the opponent in a time of trouble in order to secure a sympathetic response . . ." (p. 61). He analyzes Gandhi's development of satyagraha in South Africa and shows that the Gandhi of the years before 1905 "could not and would not have launched a movement like the Rowlatt Satyagraha of 1919," but that the Gandhi who emerged at the end of 1909 "was not only able to do so but was eager to use satyagraha and not least in the struggle for swaraj" (p. 59). Dr. van den Dungen's contribution serves to counter the argument (made here by Dr. Low) that Gandhi failed to appreciate the moderates. Closer alliances with the moderates might well have served immediate political purposes on several notable occasions. But Gandhi never lost sight of his ultimate objective which centered upon the further development of satyagraha, its use in situations of conflict, and the realization of the social, economic and psychological effects it promised for individuals throughout India. The Rowlatt Bills provided the occasion to replace violent means with satyagraha and to exercise what was, as van den Dungen establishes, an old policy. "Gandhi had been ready since at least 1909" (p. 63).

An American scholar (Dr. Bernard Kohn) has recently urged that social historians devise a more sophisticated model of the functioning of Indian society in the twentieth century and that they ask "what ties, what identifications, what symbols do [Indians] call upon and respond to in what circumstances?" This volume contributes to an understanding of the complex circumstances in which Indians found themselves in 1919 and illuminates the many ways in which Indians bound themselves to one an-

other and gave expression to those ties. It suffers less than do most collections of essays from overlapping exposition and contradicting argument. The book is a valuable contribution not only to the continuing study of "Gandhian politics" but also to modern Indian political history.

JOAN V. BONDURANT

University of the Pacific

Elites in South Asia. Edited by Edmund Leach and S. N. Mukherjee. (Cambridge: Cambridge University Press, 1970. Pp. xiv, 266. \$12.50.)

It has become a cliché in the reviewing of symposia that the quality of the contributions is "uneven." But what is a reviewer for a political science journal to say if practically none of the authors has shown the relevance of his research, however scholarly it may be, to the general topic designated by the title of the volume? If all were historians, one might simply acknowledge that our sister discipline has different criteria of significance and accept their wealth of factual data and cautious interpretations at face value. But the editors tell us that the seminar for which these papers were originally prepared was interdisciplinary, including a sociologist (Shils), an economist (Bagchi), and three political scientists (Bernstorff, Dalton, and Gray) as well. In these circumstances it seems insufficient for Leach and Mukherjee simply to enter the modest disclaimer that "the level of communication was not always very high" and that "for most of our authors, theory has been of minor relevance." Was it not their obligation as convenors to stipulate the terms of reference, such as the definition of elite, for the participants so as to ensure some degree of comparability of results? True, they have struggled *post hoc* in the introduction to set the book's theme into the context of European political sociology: Pareto, Mosca, and Marx. They find as a "common parameter" that all the papers but Spear's are concerned with the British influence on the Indian elite. With the dubious exception, however, of C. Wright Mills, they have ignored the whole North American controversy represented by Lasswell, Dahl, Bachrach, and Bottomore about the definition of elites and the measurement of power. Certainly it no longer suffices to "let the contributors decide for themselves what they mean" by the basic conceptual tool of the book. They conclude that "for most [of them] . . . the word elite implies no more than 'the men at the top'" (p. x) when, by their own admis-

sion, what most of the groups studied have in common is precisely that they were *not* at the top: they were colonial subjects! At most, they qualify as subelites or as a potential ruling class.

These conceptual criticisms aside, what raw materials do the eleven authors provide the student of elites for eventual generalization? The editors have remarked pessimistically that their contributors have not shown that anything is generally true about elites in South Asia but that they have demonstrated quite a number of things which are *not* true. The chapters can be classified, according to the source of power they deal with, into four categories: (1) the traditional landed aristocracy (Percival Spear on "The Mughal Mansabdari System," Eric Stokes's "Traditional Elites in the Great Rebellion of 1857," and Hugh Gray's "The Landed Gentry of Telengana"); (2) the modern commercial class (Ashin Das Gupta's "The Merchants of Surat," S. N. Mukherjee's own "Class, Caste and Politics in Calcutta, 1815-1838," Christine Dobbin's "Competing Elites in Bombay City Politics in the Mid-Nineteenth Century," and Amiya Kumar Bagchi's "European and Indian Entrepreneurship in India 1900-30"); (3) the modern intellectuals (Gordon Johnson's "Chitpavan Brahmins and Politics in Western India in the Late Nineteenth and Early Twentieth Centuries," Dennis Dalton's "M. N. Roy and Radical Humanism," and Edward Shils's "The Academic Profession in India"); and (4) the postindependence political class (represented by Dagmar Bernstorff's "Candidates for the 1967 General Elections in Hyderabad, Andhra Pradesh," and by Gray's paper on Telengana cited above.)

Only Spear and Bernstorff, two of the three authors dealing with pre- and postcolonial elites, explicitly raise broader analytical questions about an elite: its composition (caste and religious), its functional skills, its recruitment (including adaptability and circulation), its method of self-management (implying its cohesiveness) and the reasons for its decline or collapse. The chief impression one gets of the composition of these groups, whether truly elite or not, is the extent to which (at least among Hindus) high ritual caste status initially predetermined the chances of an individual's entering the new modern classes of India despite the introduction of achievement criteria. Thus Mukherjee, after denying that caste had any part in the selection of the *bhadralok* (gentlemen) of Bengal, admits that the top three castes (*Brahmins*, *Baidyas*, and *Kayasthas*)

formed the bulk of them because they traditionally had a monopoly of the literary and administrative skills. Unlike the Muslims, they were ready to adapt them to modern requirements. Once a greater rate of circulation of elites did set in, whole subcastes (*jati*), upwardly mobile in wealth through collaboration with the colonial rulers, used the new resource to raise their supposedly immutable religious status by the now familiar process of Sanskritization. Gray's *Shudra* "gentry," for instance, claimed *Kshatriya* (warrior) status.

The much maligned commercial elites have been rather neglected by political historians and analysts of India, more recently, perhaps, because of a Marxist bias against them. The four articles I have categorized under this heading are a healthy corrective. They make one wonder if "socialism" is actually a cloak for *Brahmin* bureaucrats and communists to dispossess upstart *Vaishya* businessmen. If so, this process may provide a rationale for including in the book Dalton's Radical Humanist, Roy, as well as Shils's academics. While "elitist in temper," both would seem otherwise misplaced in a work on elites or even subelites. In Bombay (Dobbin) more than Calcutta (Mukherjee) it appears that the first intellectuals sprang in part from that very commercial class which their ideological heirs now seek to destroy.

Beyond the foregoing classification of Indian elites, one can ask about their cohesiveness. Presumably a high degree of organization must be characteristic of a group which aspires to elite membership. But these case studies picture intense competition within as well as between communities: orthodox vs. modernizers, commercial land buyers vs. traditional landowners, colonial collaborators vs. rebels. It was only a part of each subcaste, religious sect, or functional group which rose in power and status during the modern era.

Finally, Spear has asked of his materials a question which few of his coauthors have tried to answer: How does an elite retain what power it has, or how does it decline? Each group discussed in this volume is portrayed either during its rise or at its height. Yet we know from beyond the confines of the brief periods surveyed that all except the two postindependence cases were doomed subsequently to fall from political elitehood: Spear's *mansabdars*, Stokes's *talukdars*, Das Gupta's Surat merchants, Dobbin's Bombay intelligentsia, certainly Mukherjee's and Dalton's Bengali *bhadralok*, Johnson's Chitpavans who "abdicated from politics" into the professions, and Shils's academics. Perhaps what is needed next in the study of elites is an international seminar to

create a sequel to this one, investigating how elites decline and others succeed them in South Asia, a subject difficult for upwardly mobile American scholars to comprehend.

THEODORE P. WRIGHT, JR.

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Venceremos Brigade: Young Americans Sharing the Life and Work of Revolutionary Cuba. By The Venceremos Brigade, edited by Sandra Levinson and Carol Brightman. (New York: Simon and Schuster, 1971. Pp. 406. \$3.95.)

Does Fidel Eat More Than Your Father?: Conversations in Cuba. By Barry Reckord. (New York: Praeger Publishers, 1971. Pp. 191. \$5.95.)

A number of Cubans ought to share in the royalties of these two books for having contributed at least as much to the writing as the authors themselves. Both the participants in the Venceremos Brigades and the black Jamaican playwright Barry Reckord looked to Cuba for inspiration. The Venceremos members considered the Cuban political and social structures and the development of "new Socialist man" as goals toward which the revolutionary movement in America should work. Reckord sought to document social, economic and spiritual progress in Cuba—and the price of that progress—measuring conditions in Cuba against those in his own Caribbean homeland.

In their relationship to the material, however, the authors differ considerably. The members of the Brigades considered Cuba not so much a model to be imitated, but rather a revolutionary setting in which they could test their mettle, their capacity to function collectively, and their commitment to work toward radical change in America. Their objective was to participate as fully as possible in the Cuban Revolution by cutting sugar cane and by trying to live and work as revolutionary men and women. Barry Reckord, in contrast, displayed no such wish to participate in the Revolution. As an intellectual alienated from his own country—he lives in London, not in Jamaica—he undoubtedly would have been unwilling to live as a Cuban. Nonetheless, he did attempt to provide a format through which citizens of Cuba could speak for themselves.

Venceremos Brigade is written by members of two of the four groups of Americans who have gone to Cuba to cut sugar cane or to work in agriculture. It is edited with a long introduction by Sandra Levinson and Carol Brightman, both of whom have first-hand knowledge of Cuba (Brightman participated in the Brigade).

The book is divided into eight sections, discussing preparations, work, leadership and discipline, the third world, women, revolutionary culture, internationalism, and "the aftermath." Materials include excerpts from the diaries, letters, speeches, essays, interviews, poems and pictures of the "book collective"; thirty-five writers, ten photographers, and an unspecified number of Cubans (including Fidel Castro) contributed.

The editors' and writers' hope was that out of the Cuban experience would emerge a greater sense of self, an appreciation for the need to work together, and a stronger commitment to and respect for the potential of the American people. The editors' introduction explains the formulation and organization of the Brigades, and provides a useful and thoughtful analysis of the Brigades' importance to the American left. "What happened to the *brigadistas* in Cuba . . . gives us more insight into the strengths and weaknesses of our movement than any single activity in the United States" (p. 16). The primary objective in putting together the book was to share this insight rather than to analyze Cuba. In fact, the sections which treat conditions and problems in Cuba are unconvincing, since the Brigade members became familiar with only a small and unrepresentative sector of Cuban life.

While the Americans expressed joy and appreciation for the chance to live and work in Cuba, the Cuban experience challenged their perception of themselves as revolutionaries. The dominant theme in the book, and the subject of the perceptive analysis in the Introduction, is the group's failure to act collectively. Competitiveness, male chauvinism, the inability to deal with race problems, and the tendency to think of revolution in terms of personal acts ("an extension of good old frontier individualism" [p. 30]) frustrated the well-intentioned Americans and confused the Cubans. The Cubans tried to help the Americans to deal with these issues, but they could neither fully understand nor cope with the group's divisions.

The improbable title—*Does Fidel Eat More than Your Father?*—masks a serious book which explores many dimensions of Cuban life. We soon find out from a group of Cuban children that Fidel does not necessarily eat more than father, or that if he does it is for good reason, or maybe in fact his problem is that he eats too much. There are, after all, many perspectives on the Revolution, and there are both willing and unwilling revolutionaries. After an initial chapter devoted to his overall impressions, Reckord has organized his conversations

around three themes: liberty, equality, and economic development. Considering these as the objectives of any revolutionary process, he seeks to measure the Cuban Revolution against them. A final chapter on Cuba and the United States contains a few concluding remarks by the author and long sections of a speech by Fidel.

In Reckord's book, Cuba is represented to the readers through the words of as wide a variety of *Habaneros* as he could find to interview: blacks and whites, middle class and lower class, intellectuals and bureaucrats, men and women, old people and youth. The urban scene is thoroughly represented, but impressions of rural Cuba, which has been the major focus of revolutionary efforts, are missing. Cuban opinions are recorded, presumably verbatim, occasionally interrupted and given direction by the author's observations and his interminable probing questions.

Reckord's tone throughout the book, but especially in the early chapters, is cynical. He dislikes much of what he sees in Cuba, reports a fair number of horror stories, and quotes extensively from people few of us would consider to be pleasant as neighbors. Pessimistically he considers most Cubans to be "consumers" of the revolution, rather than its creators. The commitment of Cubans to voluntary work in agriculture and to community service disturbs more than it impresses the author, and it leads him to conclude that "there is not much private virtue, only collective virtue" in Cuba.

Nevertheless, on the all-important measures of liberty and equality, Reckord judges that the Cuban revolution comes out quite well. Traditional civil liberties, to be sure, he finds lacking but he is impressed by the degree to which young Cubans are informed, involved, and enthusiastic participants in national development. In his own country the same young people would be ignorant, poor, and apathetic. In the section on equality, he effectively describes revolutionary shortcomings, such as lingering personal racism despite the elimination of institutional racism (being black, Reckord is especially sensitive to the situation of black Cubans), persistent bureaucratic privilege, and the structural inadequacy of increased worker participation. Nonetheless, Cuban egalitarianism excites his imagination and admiration: Socialism, he says, does not result in "dreary equality because when the dirty work is shared all around, then everybody will have the time and the mind to be creative."

In his treatment of the theme of economic development, the author's sympathies seem to be with Fidel rather than with the foreign ex-

perts whom Reckord describes as continually telling the Cuban leader that his priorities are wrong because they are expensive. Reckord dramatizes the dilemma of socialist economics with an account of British experts and Cuban cattle. Fidel, unable to trust foreign advice, makes his own decisions by instinct, and Reckord tells us why this is so.

While neither of these two books represents conventional academic scholarship, both studies are interesting and useful. Just as it is valuable to study Cuban society itself because it is perhaps the most exciting and controversial socio-political experiment in the Western Hemisphere, so too is it important to understand the direct impact of the Cuban experience on the youth of our own country and on representatives of the Third World.

PATRICIA W. FAGEN

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Marxist Theory and Indian Communism. By Charles P. Loomis and Joan Huber Rytina. (East Lansing: Michigan State University Press, 1970. Pp. 148. \$7.00.)

This book discusses Marx's contribution to social theory and its political application in India in terms of conceptual categories for structural-functional analysis. These theoretical categories, derived from such sociologists as Tönnies, Sorokin, and Parsons, formed the basis of the "Processually Articulated System" (PAS) model first put forth by Professor Loomis in *Social Systems: Essays on Persistence and Change* (1961) as an aid to comparative analysis of social systems.

In the present work the authors claim that the PAS model facilitates a systematic analysis of social theories, such as Marxism, as well as an understanding of the problems encountered by Indian communists in adapting Marxism to local conditions. There is some justification in attempting to explain the social-system model of the Marxists in terms of the functionalist approach, since both models view economic development as a major concomitant of modernization. This reviewer doubts, however, whether the functionalist model provides "a set of neutral categories" (p. 5) by which to examine other models, as the authors claim. The Marxist and functionalist approaches—the arguments of Dahrendorf, Van den Berghe and others to the contrary—appear to be contrasting ways of perceiving political reality. The functionalists stress accommodation or consensus, put forth the concept of a political system in equilibrium as the ideal type, and view "political development" as the gradual elimination

of conflict; whereas the Marxists emphasize conflict as the essence of politics and the source of "political change." Comparative politics "revisionists" are currently putting forth social-systems models more closely approximating the latter view than the former. In the present work, the theoretical bias of the authors is clearly evident in their evaluation of Marxian social theory.

Marx is depicted as primarily a sociologist and only secondarily an economist in the sense that he attacked the classical economists for ignoring social realities but accepted without question most of their theories (pp. 29–30). Despite the solid, empirical basis of Marx's scholarship, his predictions have gone astray, the authors contend, because of his faulty epistemological assumptions which posit a uniformity between logic and empirical reality and assert a necessary correspondence between dialectical categories, (such as thesis and antithesis) on the one hand and historical stages (such as feudalism and capitalism) on the other. This claim to universal validity on behalf of the dialectic has contributed to the fusion of theory and practice in Marxist thought. As a result, it is argued, dialectical materialism is similar to pragmatism in that both claim the criterion of truth to be how things work out in practice, and "anyone with enough power can demonstrate the truth" of Marxist claims (p. 16).

The Marxist treatment of social processes is discussed under such structural-functional rubrics as communication, boundary maintenance, systemic linkage, institutionalization, socialization, and social control. In the process, familiar Marxian ideas are converted to neologisms. Thus, class-consciousness becomes "boundary maintenance mechanisms" (p. 70), and alliance with the national bourgeoisie, an instance of "systems linkage" (p. 71). Although an interesting intellectual exercise, this procedure does not appear to further our knowledge of Marxist theory or to help explain the problems encountered in attempting to apply this theory to Indian conditions. The crucial question in both cases is whether the PAS Model provides categories which are truly analytical in nature, rather than purely descriptive: Does it advance our understanding of empirical reality? On the evidence solely of the present study, the answer would have to be no.

On the crucial question of India's political future, this work does not shed any new light. Both Marxists and structural-functionalists have been able to identify certain preconditions for political mobilization, but have been less successful in predicting the various political

forms that mobilization will take. The discussion of Indian Marxism, moreover, is seriously flawed by a lack of familiarity with the subject at hand. For data on both communist developments and socioeconomic conditions in India the authors rely heavily on two secondary sources—Overstreet and Windmiller's *Communism in India*, published more than a decade ago, and Gunnar Myrdal's *Asian Drama: An Inquiry into the Poverty of Nations*. As a result, the treatment of Marxist thought in India is disappointing. Much relevant literature by Indian Marxists has been overlooked. Discussions of important issues are sketchy and not wholly accurate in all cases. For example, we are told that "Indian Marxists evaluated the potentials of various classes somewhat differently than did the orthodox Marxists" (p. 41), whereas, in fact, Indian communists are as divided on this issue as their counterparts elsewhere. The question was first debated at the Second Comintern Congress in 1920 and is today a bone of contention between Moscow and Peking.

In a later discussion of the special problems of communication for Marxists in multi-lingual India, the authors cite the controversy over the term "decolonization" at the Sixth Comintern Congress in 1928 as a case in point. The various interpretations attributed to the concept in the course of the debate, however, had nothing to do with the language problem in India, as the authors suggest (pp. 68–69), but were related to the internal power struggle within the Soviet party. Finally the continuing influence of Gandhi's ideas, especially on nonviolence, is viewed as a major obstacle to the spread of communism in India (p. 51). Gandhi's views are honored more often in the breach than in practice today. Certainly faith in nonviolence has not been noticeably evident. The real difficulties faced by Indian communists have more to do with: their lack of ideological and organizational unity, the smallness of India's labor force relative to its immense population, India's steady if not spectacular economic progress, the hostility of Communist China, and the existence of both a competent administrative apparatus and a party in power which is able to command the loyalty of wide segments of Indian society.

This volume contains two appendices. Appendix B is a brief summary of the PAS Model. Appendix A consists of two parts: the first, by Mohit Sen, a member of the National Council, Communist Party of India, is an attempt to defend dialectical materialism against the attacks of the authors; in the second, Dr. Gautam Mat-

hur, Chairman of the Department of Economics, Osmania University, Hyderabad, comments on the inapplicability of Marxian analysis to modern economics. This reduces the major portion of the book to slightly more than 100 pages—which, at \$7.00, is a rather expensive publication even by today's inflationary standards. Regardless of their theoretical disposition, students of political sociology and political theory, especially of Marxist thought, may well wish to acquaint themselves with this work. Students of Indian communism, however, would find the effort less rewarding.

JOHN PATRICK HAITHCOX

Carleton College

Bolivia: The Uncompleted Revolution. By James M. Malloy. (Pittsburgh: University of Pittsburgh Press, 1970. Pp. 396. \$11.95.)

This book on the Bolivian revolution is ideal in many ways. Its great length enables the author to spend about half the book on the events leading to the 1952 revolution, and the remainder on the governing accomplishments and political strategies of the *MNR* (*National Revolutionary Movement*) party to its deposition in 1964. Both parts of the book are equally good. Professor Malloy eschews chronological history for sharp analysis, continually asks the right questions, has excellent perspective with depth studies where needed, and never generalizes beyond his research findings.

The thoroughness of the research sets the tone for the whole book. The work is basically Professor Malloy's doctoral dissertation. To write it he spent fourteen months in Bolivia poring over books, articles, and newspapers on the revolution, and supplemented that with depth interviews from a number of prominent and secondary leaders. The writings on the Bolivian revolution by Robert Alexander, Richard Patch, Herbert Klein, and others are also utilized and interpreted, and the author has even gone to the extent of scrutinizing many of Professor Alexander's written interviews of Bolivian party leaders and labor officials.

Objectivity is another commendable attribute of the book. Professor Malloy is basically interested in understanding the Bolivian revolution and thus avoids praising or deploring it. His detachment thus enables him, for example, to analyze the *MNR* party as an actual political power structure instead of whitewashing it, as others have done. He is capable of dissecting the formation and dissolution of hard-boiled coalitions without being caustic and cynical. The reasons for actions are what fascinates the author, and he avoids moral judgments.

One can always ask whether a book of this type will have any value to those other than Latin American specialists. In this case the response would definitely be affirmative. Professor Malloy in the first part of the book not only tests but also refines certain theories of revolution, and in the second part he evaluates many of the *MNR* revolutionary activities within the framework of some of the general literature on political development, mobilization, and economic choice priorities. Fortunately, the author never distorts his research findings to prove a theory. Instead, he has given us a case study in depth about the implications of a revolution in an extremely underdeveloped country. The Bolivian experience is in many ways unique, but the author argues that revolutionary leaders in other primitive countries will be faced with similar kinds of problems. Herein lies the value of the book for comparative politics.

In the first part of the book Professor Malloy utilizes various ideas from Chalmers Johnson's *Revolutionary Change* to sharpen his evaluation of the causes of the Bolivian revolution. The ideas are taken not for the purpose of constructing a tightly operationalized research model. Instead the author feels that the ideas enable him to place the causes of the Bolivian revolution in much better perspective. The best example is Malloy's insistence that it was the economic stagnation of the whole tin economy by the late 1920's, and not the Chaco war, that led to multiple dysfunctions in Bolivia. One might take issue with this singular emphasis, but the author describes how the stagnation in tin led to elite infighting, the scarcity in prestigious jobs for the middle sectors dependent upon tin, worker discontent, and the economic deterioration of Bolivia. Malloy goes on to show how Bolivia was doubly hit by the effects of the depression and the fighting of the Chaco war in the 1930's. He analyzes these as two huge accelerators which aggravated the discontent, leading to the growth of many reform parties, the discontent among younger military officers, the outpouring of intellectual criticism, etc. Professor Malloy argues that the tin miners were probably the most embittered group during the 1936-1952 period since unsympathetic regimes deprived them of gains they had made under Busch and Villarroel. The author is certainly not the first one to handle deprivation theory, but he utilizes it in an insightful way.

Some criticisms of the first part can be made. The author argues convincingly that the tin magnates of Patiño, Hochschild, and Aramayo were perceived as La Rosca, a hated oligarchy,

and operated as an associational interest group on weak governments. But he does not specifically describe their ways of achieving influence. Also, in getting at the causes of the revolution, Malloy does not refer to Crane Brinton's very germane book, *Anatomy of A Revolution*, which deals with divided elites, the desertion of the intellectuals, economic incompetence, the misuse of force, etc. This is particularly unusual since Eldon Lanning thought Brinton stimulating enough to rigorously test his ideas in his dissertation *The Bolivian Revolution of 1952 and the Cuban Revolution of 1959: Case Studies of a Theory of Revolution* (unpublished doctoral dissertation, University of Virginia, 1965).

The second half of the book, covering Bolivia after 1952, carefully develops two interesting themes. The first is that Paz Estenssoro, Hernán Siles Zuazo, and other important *MNR* leaders were reluctant revolutionaries, who believed that the revolution should be controlled from above, and who preferred the creation of economic capital over the immediate distribution of the country's wealth to supporting revolutionary groups. Malloy develops this theme by recounting how the leaders continually conspired with disaffected military officers to come into power, and by showing how the leaders were not only somewhat conservative on the issue of nationalization of the mines and land reform, but also later moved into policies of stabilization and technocracy.

The second theme is that the *MNR* leaders, no matter how hard they tried, never really controlled factions within their own party, let alone their supporters such as the miners, the rural Indians, and regional bosses. Professor Malloy does his best writing on this theme. His analysis of the *MNR* right, the pragmatic nationalist center, and the left factions explains why they were so contentious. His excellent chapter on the rural Indians describes the uncontrolled land seizures of 1952 and 1953 and the great variety of decentralized rural sindicatos accountable only to local and regional bosses. His discussion of the miners' interests also emphasizes autonomous power and immediate consumption goals. Finally, the author's superb evaluation of the fragile political coalitions each president had to put together from 1952 to 1964 leaves one with the feeling that governing Bolivia was an almost impossible task. Events in Bolivia since 1964 have confirmed the author's conceptualization of the political system. The revolution has not been set back; groups gaining from the revolution

are still autonomous, fragmented, and decentralized; and the search for national authority is still nebulous at best.

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The Challenge of Nationhood: A Collection of Speeches and Writings. By Tom Mboya. (New York: Praeger Publishers, 1970. Pp. 278. \$7.50.)

Tom Mboya was one of the chief architects of Kenya's independence, its constitution, party apparatus, union organization and planning machinery. In the brief span of seventeen years Mboya rose to leadership of the trade union, nationalist, and pan-African movements and held a succession of key ministerial positions including Labor, Justice and Constitutional Affairs, and Economic Planning and Development. He was also chairman of the Economic Commission for Africa and the organizer of massive student airlifts for studies overseas.

This book was published a year after Mboya's tragic assassination. It is not a hastily mobilized effort to commemorate the passing of another fallen leader but a book Mboya had nearly finished before his death. His earlier book chronicled the justifications and actions leading to independence and the responsibilities this new status entailed. This posthumous work goes much farther, evaluating knowledge gleaned from an additional half decade of development experience.

Mboya's speeches and articles form a compendium of some of the most cogent arguments for nationalism, independence, planning, and the contributions of the new states and of arguments against tribalism, big-power competition, and foreign aid-trade practices. It is well enough integrated to be useful in courses on economic and political development as well as African politics.

The opening chapter of the book is an attempt to catalog the principal problems threatening the self-confidence and self-realization of the new African nations. Mboya deals summarily with a host of issues ranging from national interests, inherited institutions, rural development and one-party states, to women, youth, labor, the press, and the civil service. There are three following sections: "Kenya," "Pan-Africanism," and "Africa and the World."

Reading the selections, one is struck by their refreshing boldness and candor. In his addresses to newsmen, European farmers, students, professional societies, and to his parliamentary colleagues, Mboya moves directly to

the crucial issues, squarely meets contradictions and resistance, and deftly selects the most alarming, though reliable, statistics to make his point. A typical speech, for example, indicates that "industrial output for all of Africa except South Africa is less than that for Sweden alone and contributes only 14 percent of our national income . . ." (p. 197). At a Canadian conference, Mboya, in presenting evidence of the nutrition gap in developing countries, concludes that "these estimates imply that both today and in 1975, the cost of a food aid program to close the gap between social need and effective demand . . . is about 6.5 billion dollars or eight times the size of the present effort" (p. 257). In discussions of trade preferences or commodity prices, his grasp of technicalities and fundamentals is extraordinary for one whose only formal training beyond secondary school was a year or so at Ruskin studying trade unionism.

His intellectual sensitivity in other areas is keen as well. He explicitly recognizes tax policies as a tangible expression of political value allocation. He identifies political rhetoric and slogan-making as tactics appropriate to early national consolidation but of no utility in later transitional politics. On the subject of contemporary Afro-Americans who romanticize Africa, Mboya asserts,

. . . we live in mud and wattle huts and buy cheap Hong Kong fabrics not because it is a part of our culture, but because these are conditions imposed upon us by poverty and by limitations in technical, educational and other resources. White people often confused the symbols of our poverty with our culture. I would hope that black people would not make the same error (p. 228).

Mboya's definitions of the challenges of nationhood are terse and numerous, but he also includes a wealth of useful technical data and empirical observations, especially about such topics as foreign aid, planning, and economic development. His analyses do not comprise a development theory, though many of his statements (for example, his arguments for regional economies of scale) are useful adjuncts to such theories. Many points in the book are made didactically and are severely, though skillfully, simplified. Mboya was a man of practical affairs who saw it as his responsibility to chastise, encourage, and lecture those whose understanding and power were required to achieve the objectives he envisioned. The simplifications of such complex matters as Marxism, trade preferences, or African socialism are intended to educate the ignorant and misinformed among his own people as well as those outside Africa.

Some of Mboya's operating assumptions as reflected in this book deserve criticism. Two problems stand out. The first is his obsession with economism. In his discussion of African Socialism he cautions against excessive constitutional manipulation lest it put off foreign investors. In dealing with modern institutions and social change he tends to interpret them in the narrow terms of economic growth at the expense of political, administrative, and social considerations. Mboya is an excessively enthusiastic advocate of neo-Keynsian economics, the work-ethic, and the grand promises of technological shortcuts. Yet his very enthusiasm for these orientations in no small way explains his own capacity to escape the crises of confidence which he saw all about him.

The second criticism one might level at Mr. Mboya's position is his uncritical acceptance of the colonial and British simplification of the politics-administration dichotomy. Kenyan administrators often contend that politicians do not acknowledge that they have a vital role in the political process. Mboya's assumptions about administrative neutrality, delegation, and responsibility are not supported by what we know about the dynamics of controlled political change. This leads Mboya into a distorted concept of party organization as well, despite his own record as a successful party tactician. His practical resolution of the tensions between the civil servants and politicians was to make the party a personal instrument. As a consequence there were massive seepages of power to the executive and hence to the administration. These seepages eroded various legitimizing electoral, representative, and administrative principles; they left the party an intermittent and ineffective spokesman for diverse social interests. Responsibility for policy has subsequently become so ambiguous as to ensure confusion; and tribal competition for party positions inevitably found an area for expression in the ministerial apparatus instead.

The Challenge of Nationhood is an excellent introduction to the present crises of nation building. In addition it is a useful memorial to a man whom time may identify as one of the most able and effective political leaders in modern African history.

BRACK BROWN

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Résultats des élections belges entre 1847 et 1914. Bibliothèque de l'Institut Belge de Science Politique. New Series, no. 8. By W. Moine. (Brussels: The Institut, 1970. Pp. 200. \$3.50, paper.)

Of the three states—England, Sweden, and

Belgium—that have passed through what Stein Rokkan has called the "Western development toward equalitarian electoral democracy" in anything like a regular sequence, Belgium is the one whose development is by far the least well known. Yet Belgium is of interest for its own political history and for comparative studies: 140 years of electoral competition and parliamentary life unbroken except for the two German occupations; for half a century a two-party system less blurred than that of the British model; secrecy of the ballot introduced in the days of the taxpayers' suffrage; two decades of an intriguing system of plural suffrage, with a fourfold weighting system to temper the introduction of universal manhood suffrage; proportional representation since 1900; and for 70 years, until the 1965 elections, a basically three-party system, which makes election returns easier to handle than those of almost any other multiparty system.

The data on national elections since universal equal manhood suffrage in 1919 had been assembled, in publications to which I shall return. But there was almost nothing available by way of collected data for the elections between 1831 and 1892, and only bulletins of the Ministry of the Interior for the elections between 1894 and 1914. Now W. Moine (not M. Moyne, as cover and title-page of this errata-ridden book have it) gives us the results for the House of Representatives from 1847 to 1914, at the level of the electoral *arrondissement*, of which there were 41 until 1900, and 30 thereafter, with recapitulations at the level of the nine provinces and the kingdom as a whole. This covers all the elections before universal equal manhood suffrage except for the years 1831–1845, which it is reasonable to omit because of the uncertainty of assignment of candidates to parties in this period of Catholic-Liberal "unionism." Even for the later years, the author had to resolve many knotty problems of party assignment for candidates running alone and without party labels.

There is no substitute for the patient, detailed, and seldom adequately recognized work of the specialist, steeped in the problems and sources of time and place, who brings together the single-nation electoral data that one needs for further empirical and theoretical work. Moine worked almost alone, under the aegis of the eminent Professor De Meyer and the Center for Political Studies in the Flemish section of the University of Louvain. Not surprisingly, therefore, his work offers much less detail, fewer recapitulations, and less explanation, in a form much harder on the eyes, than the impressive *Atlas des élections belges, 1919–1954*,

by De Smet, Evalenko, and Fraeys. That work, which is not an "atlas," gave the House results by the 211 *cantons*, with recapitulations at *arrondissement*, provincial, and national levels, and by urban agglomerations and by language areas, along with succinct summaries of the issues of each election.

Moine's few explanatory pages are helpful for details which the election returns fail to disclose, such as the lack of contests in some constituencies, the results of runoff elections before 1900, and the distribution of seats between the two parties in a number of Liberal-Socialist joint slates after 1894. The author does not attempt to explain the changes in the system of representation which give Belgian political history much of its significance and color. Occasionally he offers a few bland words that appear to take for granted the outcomes of controversies that raged (and in some cases still rage) in Western politics. For example, he remarks (page 170) that it was "normal" that proportional representation be adopted in 1899. Why it was "normal" in Belgium, when it was excluded in France and Britain, both of which greatly influenced Belgian politics in other ways, is left without explanation. The decision of the Catholic Party leaders, with their absolute majority in both Houses of Parliament from 1884 to 1914, was a vital one, "normal" or not. It saved the Liberal Party from the near-elimination from Parliament which was to overtake British Liberals under a majority system. And it prevented a full confrontation between a predominantly Catholic Flanders and a predominantly Socialist industrial Wallonia.

Those who wish to study Belgian developments will find published data now, not only in Moine and in the De Smet, Evalenko, and Fraeys *Atlas*, but also in *Les Elections belges*, a volume by De Smet and Evalenko comparing the results of the 1950 and 1954 elections with social structure, trade union preferences, and very shaky figures on religious practice; in election results and analyses in a volume of the Belgian Institute of Political Science for the 1958 elections, and special issues of its journal *Res Publica* for the 1961, 1965, and 1968 elections, as well as current reporting and analyses of primaries and both municipal and national elections in the *Courriers Hebdomadaires* of the Centre de Recherche et d'Information Socio-Politiques (CRISP) since its foundation in 1958. The electoral laws are described by John Gilissen in *Le Régime représentatif en Belgique depuis 1790*. Specialists will want to see the criticisms of Gilissen in a review by H. T. Deschamps in the *Revue belge de Philologie et d'Histoire*, 1959, and those of *Les élections*

belges by Jean Stengers, with reply by Evalenko, in the *Revue de l'Université de Bruxelles*, 1958.

Wilfried Dewachter analyzes other steps in the electoral decision-making process: the choice of candidates, the issues, motivations and types of voting behavior, including preference votes, and consequences of the Belgian PR system in *De Wetgevende verkiezingen als proces van machtsverwerving in het belgisch politiek bestel* and in an article (in English) in *Res Publica*, no. 3 (1967). Frédéric Debuyst offers information and insight on the primary elections of the three major parties in *La Fonction parlementaire en Belgique: mécanismes d'accès et images*. There is an extensive bibliography of Belgian elections since 1831 by H. Lhoest-Offermann in *Res Publica*, no. 1 (1966). Dewachter has published an *Atlas* of the 1968 election results. But public opinion polls are just beginning to touch on party preferences.

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China: The Revolution Continued. By Jan Myrdal and Gun Kessle. (New York, Pantheon Books, 1970. Pp. 201, \$5.95)

Ideology and Practice: Evolution of Chinese Communism. By James Chieh Hsiung. (New York, Praeger, 1970. Pp. 300, \$4.50 paper)

These two works are welcome additions to the growing Western literature on contemporary China. Though drastically different in approach and coverage, both deal with the cultural revolution. The Myrdal book is an eyewitness account of the human drama of that revolution in a northwestern Chinese village. Hsiung's work interprets the outbreak of the revolution as a culmination of "the rising tension between ideology and practice" since 1949.

After eight years of absence, the Myrdals returned to Liu Ling, the village they had visited in 1962 and described in detail in their much acclaimed *Report From A Chinese Village*. The result is a sequel to their earlier work and the first comprehensive report by foreigners on the cultural revolution at the grass-roots level. Like their earlier work, this one provides a wealth of information about rural China which otherwise would be hard to come by, and it sheds light on the recent changes in Chinese villages. It covers a wide spectrum of topics: the cultural revolution and the role of the Red Guards; the Party and the evolving decision making and power structure in the village; the emerging pattern of finance and investment; the new remuneration system and the social security system; the reform in education and the rustication of urban

educated youths; the health care program, including the much publicized "barefoot doctor" and the new medical insurance; the evolving farming practice and diversification; the revival of indigenous small-scale industry; women, marriage (including the regime's attempt to promote delayed marriage), birth control, and many other subjects.

The Myrdals' new work is more than a piece of first-rate journalism; it is also a passionate apology and plea for open-minded understanding of the cultural revolution. They do not pretend to be neutral; they readily admit their sympathy for and enthusiasm about what they heard and saw in Liu Ling. While their indictment of the Soviet and the Western press's distortion and misinformation (deliberate or otherwise) about the cultural revolution is generally well taken, their eagerness to defend the new programs leads at times to excessive enthusiasm and idealization. While it is true that Mao characteristically outlines broad policy goals and general direction, and leaves the details for the low level cadres to work out, encouraging the "masses" to exercise discretion and initiative in order to maximize *yin-ti chih-yi* (getting the best out of each area), it is definitely not true that the "masses" actually make decisions on the basis of "the living study and application of Mao Tse-tung thought" (pp. 192-93). The "masses" are by no means free to reject the "general recommendations" issued by the center in the name of Chairman Mao. One wonders what would happen if the "masses" indeed turned down the recommendations. It is one thing to talk about the importance of Maoist thought in contemporary China; it is quite another to say that it is Maoist thought that is holding China together.

Like their earlier work, one of the greatest merits of the present one is the faithful recording of conversations with the villagers. Herein also lies its drawback. As in the earlier work, the peasants in the present volume talk very much the way the official press reads. It is virtually impossible to determine whether they are expressing their own views and feelings or simply parroting the official line which, thanks to the Party's relentless "ideological work," they have thoroughly internalized.

Hsiung in his book attempts an "interdisciplinary approach." He draws freely on the insights, perspectives, and analytical tools of various social sciences from cultural anthropology to linguistic analysis. The result is an eclectic but refreshing analysis which synthesizes the findings of others yet offers the author's own perceptive and original interpretations.

Hsiung's work is in three parts. Parts One and Two interpret the Chinese Communist ideology, mainly but not exclusively Mao's thought, in historic and comparative perspective. Part Three is a competent summary of the major trends since 1949: the ideological and policy directions; the growing tension between ideology and practice; the evolving leadership conflicts over a wide range of fundamental developmental issues and concrete policy measures which found their ultimate expression in the cultural revolution; the Maoists' aims and tactics in launching the cultural revolution; the twists and turns of that revolution; the subsequent reshaping of the Party, the polity, and society in the Maoist image; and the emerging approaches to the development inspired by Mao Tse-tung's thought.

Hsiung is at his best when dealing with the nature, role, and functions of ideology. He observes that in China "ideology is never conceived of as having emanated from any segment below; the ideology is always imparted by the center from above," and presumably transcends social classes. "In Communist China ideology . . . seeks to establish a new culture, a new way of life, through purposeful, collective means. It is all-inclusive. It offers society an apocalyptic vision, a spiritual body of concepts and vocabulary for communications, and a methodology" (p. 7). He finds striking similarities between traditional and contemporary China with respect to the role of the ruling ideology. "The content of the prescriptive system has changed, but its function has not" (p. 8). Marxism-Leninism-Maoism has replaced the Confucian ethos, taking over the latter's function as the "culmination of what we in the West term social conscience, morality, religious faith, and constitutional law" (p. 7). One might add that while the *literati* of the past upheld Confucianism in order to rationalize and preserve the status quo, the Communist Party of today uses its own ideology in an attempt to transform China into a socialist modernized nation state.

Hsiung offers refreshing interpretations of the goals of Mao's revolution, the content of Chinese Communist ideology, and particularly of Maoist thought. Since he covers a broad range of topics, one cannot do full justice to his work in a short review. My comments will be confined to two points on which the author believes he has something new to say. The first is his perceptive discussion of Mao's evolving conception of "class struggle"; the second his unelaborated analytical distinction between two types of leadership, ideological and organizational. Hsiung has largely succeeded in the first

task but, in my opinion, not done so well in the second.

According to him, class struggle has been a standard device employed by Mao to combat and correct the various cultural tendencies and traits he condemns (pp. 119–121). While persistently employing it as an instrument for social and cultural transformation, Mao has altered his ideas on the content and targets of class struggle over time. Until about 1960, his views were essentially in accord with orthodox Marxism; class struggle was “interclass, interpersonal, and outward-directed” against the class enemy, the “bourgeoisie,” “the exploiting and oppressing classes” and so on. Since the early 1960’s, it has been enlarged to “intra-class, intrapersonal, and inward-directed” (p. 209). “In the current usage, the term means the struggle within each person, irrespective of his class background, between proletarian thinking and remnant bourgeois thinking” (p. 122).

As for the second point, Hsiung’s analysis leaves something to be desired. In spite of his initial claim (see p. 9), Hsiung never really elaborates on the analytical distinction between the two types of leadership. Although his subsequent analysis seems to identify Mao as a practitioner of leadership by ideology and Liu Shao-ch’i as a practitioner of leadership by organization, he never defines the two key terms, much less pursues their theoretical significance. I concur with his taking Schurmann to task for subsuming ideology under organization, and I am also persuaded by his argument that Mao has had a penchant for ideological leadership (whatever that means) which makes him insist that the organization be an instrument in the service of the ideology. I am not, however, convinced by the suggestion that Liu is a man who relies primarily upon organization and cadres rather than ideological work to carry out sociopolitical transformation. For if Mao’s kind of leadership implies that the organization must be at the disposal of the ideology, then leadership by organization presumably implies that the organization may subvert or even transcend the ideology. To the extent that this is indeed what Hsiung means, one logical corollary of such an argument would be that Liu places organization above ideology. But in reality Liu was only employing the Party structure to achieve his own vision of a good socialist society, his ideology, which unfortunately deviated from Mao’s. Furthermore, Liu’s “On the Cultivation of A Communist,” to which Hsiung also refers, offers ample evidence that he was as concerned with “ideological work” and “mass line” as Mao; he also stressed the cadres’ commitment to the

Communist ideology and moral integrity, upon which all Chinese Communist leaders base their claim to leadership. There is undoubtedly genuine difference between Mao and Liu with respect to leadership style, but it is a matter of proportion and emphasis; Hsiung’s distinction between the two types of leadership is analytically ambiguous (at least to me). Insofar as such an analytical distinction can indeed be made, it must be more in the nature of Weberian “ideal type” with no empirical referents.

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The Green Shirts and the Others: A History of Fascism in Hungary and Rumania. By Nicholas M. Nagy-Talavera. (Stanford: Stanford University. Hoover Institution Press, 1970. Pp. 427. \$9.95.)

This is a highly readable and, in spite of its many shortcomings, a basically useful book on the evolution and characteristics of fascism in Hungary and Rumania. The author is a Hungarian of Transylvanian background, who experienced the horrors of fascism in his homeland and spent seven years (1949–1956) in Siberian labor camps. His experiences with both forms of totalitarianism, his mastery of a number of foreign languages, and his academic background (following his arrival in America, he received a Ph.D. from the University of California at Berkeley) make him uniquely competent to tackle this morbidly fascinating subject.

Unfortunately, the book is somewhat of a disappointment. While the author may be technically correct in his claim that it is the “first more or less comprehensive and comparative work (on the subject) in the English language,” much of the material has already been available to the readers in the West through the same major secondary sources on which the author tended to rely so heavily (e.g., Hilberg, Jaszi, Kállay, Lackó, Lévai, Macartney, and Roberts, to cite only a few of the major sources available in English). While it is useful in the sense that it brings under one cover the material pertaining to these two countries, it is quite unbalanced, for 6 of the 12 chapters (III through VIII) are devoted to Hungary and only 3 (IX through XI) to Rumania proper. The first two chapters provide a succinct historical background of the two nations and a review of the position of the Jews amidst them. The last chapter contains a comparative-analytical summary.

Although the book contains a wealth of useful information and a basically sound historical

interpretation concerning the genesis and manifestations of fascism in Hungary and Rumania, its value as a scholarly reference work is questionable. This is due primarily to the author's carelessness in linguistic matters, superficiality in the handling of data, and tendency to engage in undocumented, sweeping generalizations. Aside from the many orthographic mistakes, the Rumanian names and titles are misspelled throughout the text to the annoyance not only of the purist, but also the researcher interested in verifying his data. Many of the historical figures and authors are misidentified. To cite just a few of many such examples, in describing the attack launched by the Black Front, a terrorist group of the Arrow Cross Party, against a number of Jewish worshippers, the text gives (p. 141) the date of February 1939 while in the footnote it is identified as February 4, 1937. The author Róbert Major is mistakenly cited as József Major (p. 104). The massacres at Kamenets Podolsk are identified as having taken place in July 1941, whereas they actually took place on August 27-28, 1941.

Perhaps the greatest weakness of the book lies in the author's unsubstantiated sweeping generalizations. For example, the author states that the ultra-rightist Szeged idea and Gyula Gömbös' rise to power were aided by "Jewish financiers" (p. 88) and that fascist agitation in Hungary was Jewish-financed (p. 89), as was the Hungarian Party in Czechoslovakia during the 1930s (p. 148). Such statements leave the impression (though this is absolutely not the intention of the author) that the Jews were largely responsible for the development of fascism in Hungary. Evidence should have been provided showing the absolute and relative numbers of these so-called "Magyars of the Jewish faith" (*Izraelita-vallású magyarok*), and their proportion and relationship to the Jewish community. Many of the members of the dozen Jewish families, who, according to Nagy-Talavera owned or controlled the "mammoth enterprises that composed about 90 per cent of Hungarian industry and banking" (p. 148) were in fact converts to or born into the Christian faith. Many of his other claims, as for example that "Horthy never desired or believed in German victory" (p. 180), or that Szálasi, the Arrow Cross leader, opposed the destruction of the Jews "to the very end" (p. 202), or that the Hungarian Government had explicit warnings and knowledge about Auschwitz" (p. 203) are all interesting, but in the absence of evidence not very convincing. This lack of investigative effort also leads the author to make a series of declaratory judgments concerning

Nicolae Titulescu (p. 257), Sámuel Stern and the other members of the Jewish Council (p. 203 and 241), and Kasztner (p. 214-15). While the accusations relating to the former's corruption, and the latter's servility, spinelessness, and collaboration with the Germans may have some basis, the evidence is not supplied.

Nagy-Talavera properly devotes considerable space to the treatment of the "Jewish question," for without understanding it, one cannot possibly grasp the implications of the fascist phenomenon in these countries. He is rather superficial, however, in describing the historical background of the two Jewish communities (Chapter II) and basically inaccurate in his analysis of the era of the holocaust in Hungary. (The Rumanian Jewish community fortunately escaped the mass deportations to Auschwitz and other extermination camps.) His historical sketchiness may be due to his failure to exploit properly the vast Jewish historiography relating to the genealogy of these Jewish communities. His inaccuracy about the holocaust may derive from his reliance on a book by Lévai, the noted Hungarian journalist, which appeared as *Fekete Könyv* (Black Book) in 1946. While still a valuable source, it obviously could not anticipate the wealth of new archival and documentary materials that have been unearthed since its publication. These materials Nagy-Talavera has largely overlooked.

While the shortcomings of this book are many, so are its virtues. It includes a good chronological account of the fascist movements in Hungary and Rumania, skillfully tracing the similarities and differences between them to their divergent national-historical roots. It is particularly successful in demonstrating the basic identity of their inner core (ultranationalism and anti-Semitism) and the similarity of the social-political milieu in which they evolved and flourished. It is comforting to know that the faults of this book are easily correctable. This reviewer hopes that the author will manage to bring out a second, revised edition, for the important subject he tackles has long been neglected by serious scholars.

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Mongolia: A Profile. By Victor P. Petrov (New York: Praeger Publishers, 1970. Pp. vi, 179. \$7.50.)

This timely item in the Praeger Profile Series is coincidentally available for the Fiftieth Anniversary of the Mongolian People's Republic. It holds no surprises for those already familiar

with Mongolia, but it is a concise overview of the first communist nation in Asia and prototype of East European Russian satellites.

The book gives a good selection of the more important facts and figures about Mongolia. The book covers both past and present, but it offers little interpretation or analysis of the nation which is becoming increasingly more involved in world affairs. The first five background sections present an interesting historical summary but include a rather simplistic explanation of the rise of the great Mongol Empire—largely the old, unsympathetic, blood-and-thunder interpretation of Mongol expansion.

In discussing the epic of Mongolia's socialist revolution, Petrov emphasizes the invasion and counterinvasion of White and Red forces, but no mention is made of the popular discontent, the grinding debt of the Chinese merchants, the corruption of the Mongol elite, or the stagnation of the economy and the church—all of which were predisposing tendencies to revolution.

More informative are ten capsule treatments in separate chapters on such topics as government, economy, industry, agriculture, transportation and communications, the people, arts and recreation, vanishing customs, and foreign relations. Throughout the book Petrov correctly details how Russia has had the greatest influence of any foreign nation on modern Mongolia, but one wishes he had taken time to explore his complementary assertion that were it not for the Mongol invasion, the whole course of Russian history would have been entirely different (p. 5).

Throughout the book Petrov succeeds in impressing the reader with the rapid changes in Mongolia during the last fifty years. He notes the persistence of nationalism as well as a certain measure of independence from the Soviet patron. The study gives 1911 as the date of the first major penetration by foreign capital, but actually Chinese merchants dominated Mongolia economically long before this date. Terminology and proper names are generally accurate, but occasionally there is a lapse such as Huh Hoto (for Hohehot), Altay (for Altai) and Genghis (outmoded French form for Chingis). Mr. Petrov prematurely notes the end of Lamaism in Mongolia when, as matter fact, remnants persist; indeed, recent visitors have found some evidence of new growth with the establishment of a Lamaist academy and the recruitment of new candidates for the priesthood.

There is no indication of the major sources used in writing the book, but it is authoritative

and without any distinctive bias. The treatment is generally sympathetic to the Mongols and will serve very well both as a guide for tourists and as a source book for the public schools.

PAUL HYER

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Social Fragmentation and Political Hostility: An Austrian Case Study. By G. Bingham Powell, Jr. (Stanford, Calif.: Stanford University Press, 1970. Pp. 201. \$7.50.)

The notion that a fragmented society is incompatible with stable, effective, democratic government is a venerable one, but only recently has it been subjected to concerted, empirically based scrutiny. Professor Powell's admirable case study of social relations and political performance in an Austrian community carries this scholarly dialogue several important steps forward.

The hypothetical causal chain here is familiar. Where social cleavages—economic, religious, ethnic, and so on—are cumulative rather than cross-cutting, social and political organizations will tend to follow these primary cleavage lines, creating mutually hostile subcultures. The resultant intergroup enmity will exacerbate political conflicts, imperiling effective democratic government. Austria has long been considered a classic example of such a "centrifugal" polity: irreconcilable antagonism between the "Red" and "Black" *Lager* is said to have led inexorably to the collapse of the First Republic, and only the extraordinary "Grand Coalition" between the leaders of the two *Lager* is supposed to have averted similar difficulties after World War II.

Powell finds that at first glance politics in his community of Hallein conform to this pattern. Leaders of the two principal parties vilify one another, refusing to cooperate even in situations of high communality of interest. Innovative solutions to public problems are lost in a short-sighted struggle for narrow tactical advantage. Elaborate arrangements for reciprocal self-defense (the so-called *Proporz* system) constantly threaten to collapse, jeopardizing all stable government. Access by ordinary citizens to the political process is restricted by the rigidity of party ties. Even though the "load" on this political system is minimal, as Powell convincingly shows, the high levels of conflict severely impair the performance of the system. This is just the pattern of politics which the theory of social fragmentation is intended to explain, and using data on mass attitudes and elite behavior, Powell carefully examines each link in this lengthy theoretical chain.

Though partisan affiliation is closely associated with both social class and religious orientation, religion and class are themselves virtually uncorrelated, implying that in Hallein at least, these lines of cleavage are cross-cutting rather than overlapping. Still, as predicted, partisans in "pure" cleavage positions—upper-class Catholic supporters of the OeVP, and working-class Protestant or areligious supporters of the SPOe—display less tolerance for their opponents than do partisans in "cross-cutting" cleavage positions.

As in the rest of Austria, many secondary associations in Hallein have close ties to one or the other of the two main parties, and Powell finds that voters whose organizational memberships are limited to groups allied to their own party show considerably greater hostility toward their opponents than do members of associations cutting across party lines. Moreover, hostility is greater still among the leaders of "cumulative" organizations than among their less active colleagues. The impact on political enmity of organizational fragmentation along party lines is even more direct than is the impact of social fragmentation.

Thus, as predicted, individuals in "pure" social and organizational positions show more hostility toward political opponents than do cross-pressured individuals. But in Hallein both the absolute level of "purity" and the absolute level of partisan hostility are quite *low*. That is to say, although the data do tend to confirm the theoretical propositions linking social fragmentation and political hostility, they cast doubt on the view that Hallein is deeply fragmented, at least at the mass level.

At the elite level, on the other hand, Powell finds much higher levels of partisan enmity. This situation seems due in part to patterns of recruitment, in part to patterns of elite-mass interaction, and in part to the strategic imperatives of political competition. Mutual fear and hostility, even if found only at the elite level, tend to be self-perpetuating. Thus, like Lijphart, Stiefbold, Lorwin, and other recent students of "segmented pluralism," Powell finds the key to the system in the values and habits of elites and subelites.

The strength of Powell's work is the sophistication and breadth of his theoretical concerns and the care with which he explores the distinctions and propositions suggested by his data. He is not always equally careful in his marshaling of evidence nor equally sophisticated in his measurement techniques. His eagerness to illuminate theoretical propositions sometimes leads him to stress relationships which are statistically dubious. But Powell is here not "testing"

theory, but "exploring" it, and such methodological deficiencies are not critical.

Since his "exploration" leads him to highlight the importance of elite perspectives, it is unfortunate that his examination of this factor is much less systematic than his analysis of mass attitudes, which turn out to be less significant. His examination of the correlates of partisan hostility at the mass level, for example, leads to some unexpected insights into the origins of this hostility, and similarly serendipitous discoveries might have emerged from systematic analysis of elite attitudes. Or another example: There are fleeting references to "norms of civility and tolerance" at the elite level, but we are nowhere given an assessment of the breadth and depth of elite adherence to such norms.

But perhaps the most serious weakness of this study is its surprisingly ahistorical character. Powell refers repeatedly to the impact of the past on contemporary attitudes and behavior, but aside from a brief, conventional summary of local history, he makes no attempt to look longitudinally at changing patterns of social cleavage, social organization, political attitudes, political conflict, or systemic performance. As Lorwin and others have argued, Austria (like other fragmented societies in postwar Europe) seems to be undergoing "de-fragmentation" (the Dutch have a word for it—*ontzuiling*), and many of Powell's conclusions—about the relative importance of elite and mass attitudes, for example—may depend on the particular stage which Hallein has reached in this process.

In fairness, it must be added that Powell seems to recognize these weaknesses in his analysis, and we may perhaps look forward to more intensive exploration of these facets of the problem in his subsequent work. This initial installment is an outstanding contribution to our understanding of the bases of effective, democratic government, of interest not merely to students of Austrian and European politics, but to anyone concerned with empirical theories of governmental performance.

ROBERT D. PUTNAM

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The Government of London: The Struggle for Reform. By Gerald Rhodes. (Toronto: University of Toronto Press, 1970. Pp. 320. \$9.50.)

Gerald Rhodes, of the Greater London Group at the London School of Economics and Political Science, has produced a lucid and probing narrative study of "the struggle for re-

form" in London government, concentrating on the years from 1957 when the Government decided to appoint a Royal Commission on Local Government in Greater London to 1965 when the London Government Act of 1963 became fully effective. As the author reveals, the story of local government in modern London had earlier beginnings. The Local Government Act of 1888 created the County of London with jurisdiction over most of the built-up area of that day, and in 1899 the numerous lower-tier units in the county were consolidated into 28 Metropolitan Boroughs. Following these actions, the pattern of London local government remained almost undisturbed for six decades. In 1956 and 1957 the Conservative Government did hesitatingly address the question in two White Papers. Then, somewhat surprisingly, in July 1957 the Government announced its decision to appoint a Royal Commission.

The membership of this Commission (called the Herbert Commission for its chairman, Sir Edwin Herbert, now Lord Tangley) and its terms of reference provided clear signs of the Government's commitment to examine fully the issues of metropolitan boundaries and of local government centralization. None of the members had any inhibiting connection with local governments in the London area. The terms of reference gave the Commission power of review over the continuous built-up area of Greater London and the power to consider all the functions then being performed there by local governments. Thus armed, the Commission held extensive hearings, taking evidence from local government associations, national government ministries, political parties, professional bodies, and university groups. The greater part of this evidence supported the status quo or proposed quite limited changes; only the Greater London Group (Professor Robson, chairman) presented a comprehensive design for reform.

The unanimous report of the Herbert Commission (October 1960) proposed, in an effective presentation, that a Council for Greater London be established with jurisdiction over an area about 10 per cent smaller than the Commission's review area and that 52 Greater London Boroughs replace the approximately 100 existing local units. The metropolitan goal was thus substantially sustained; the centralization issue was resolved in two ways: the existing local units were reduced by half while the resulting larger boroughs were made the primary units of local government, the GLC being given few exclusive powers and made a weaker center than its predecessor, the LCC.

Reactions to the Herbert report were mixed,

the greater portion again being hostile to change or proposing solutions much milder than the Herbert recommendations. Nevertheless the Government moved forward with a White Paper (November, 1961) which endorsed the main proposals of the Herbert Commission, invited responses from the affected local governments, engaged in a two-day parliamentary debate on the White Paper, made concessions in boundary matters and in the arrangements for education, and finally (November 1962) introduced its London Government Bill. In this year-long passage from White Paper to London Government Bill, the metropolitan area was further reduced by about 15 per cent; the boroughs were made larger and reduced in number to 32, plus the City of London; the LCC education system was renamed but kept intact; and the whole London reorganization question became a major political party issue, with the Labour party in determined opposition. From introduction to enactment (July 1963) the Bill was in progress more than seven months, but its final provisions reflected no major change.

Considering the Labour spokesmen's charges that the London reorganization was a Tory plot to capture London, the first GLC and London Borough election results in 1964 were ironic: Labour won GLC by a ratio of two seats to one and got majorities in 20 of the 32 boroughs. (Subsequent London elections produced a different pattern: the Conservatives won GLC majorities in 1967 and 1970 and a majority of the boroughs in 1968, but in 1971 Labour came back to win a majority of the boroughs.)

The reform goal of metropolitanization was thus largely met; three-fourths of the London metropolitan region as defined by the Herbert Commission's review area was placed under the GLC umbrella. The centralization goal, never as clearly articulated, was somewhat ambiguously met: the local units were reduced to one-third their previous number, but this centralization was balanced by restricting GLC powers and by continuing the direct relations of the London Boroughs with national government ministries.

The consequences of these new and old arrangements for the effective governance of Greater London is to be the subject of a projected second volume by Mr. Rhodes; all students of metropolitan governments can therefore look forward in confidence to an equally thoroughgoing and illuminating appraisal of London government in action.

WALLACE S. SAYRE

Columbia University

Nation-Building in Africa: Problems and Prospects. By Arnold Rivkin. Edited by John H. Morrow. (New Brunswick, N.J.: Rutgers University Press, 1969. Pp. 312. \$10.00.)

For those who knew Arnold Rivkin and found his work thoughtful and useful, this posthumous work is a sad disappointment. It would be better to remember Mr. Rivkin by his earlier books and his work with the World Bank than by this plethoric collection of banalities. Anything in this book has been said better and earlier elsewhere, a point the author underscores by well-placed quotations that summarize what he has been trying to say in the preceding pages.

The mere mechanical problem of outdated is discouraging enough to begin with. Most of the detailed narrative, case studies, and initial papers or speeches on which this book is based date from 1964–1966; the Ghana coup had just occurred, the Nigerian military takeovers had just taken place. The author died in September, 1968; the book was edited by former Ambassador John Morrow and bears the publication date of 1969. This review was requested in March, 1971, and written in July, 1971, presumably over a year after the book appeared. The basic material is now half a decade old, presenting a strange game of mirrors for a study that aspires to look ahead into the near future. The Africa of 1966 or even of 1969 is not the Africa of 1971.

If the insights were new or brilliant or challenging, all such mechanical observations would be trivia. But the argument, a rebuttal to those Africans and Africanists who believed that the single party was the uniquely effective means of mobilization and development, reads like an evening talk to the Rotarians or an Army reserve seminar. For the most part, the developmental possibilities of African states are "uncertain," and the promised "continuum [of] . . . prospects" (p. 205) is no more explicitly presented than are the criteria or measures for establishing such a continuum. There is no doubt, however, that Nkrumah's Ghana is "bad" and federal Nigeria is "good," with the other states scattered in between, closer for the most part to Ghana than to Nigeria. Africa's chances at development are poor because Africa tends toward the "centralized, unitary, one-party, authoritarian-oriented, large public sector" end of the spectrum (pp. 205, 227, 257). But the relationship between centralization and development is only summarily asserted, never critically examined. Even the fact that "good," pluralist Nigeria broke down into a long and bloody civil war, while "bad," centralized Ghana merely underwent a comparatively eas-

ier military coup is never subjected to much analysis or reflection.

The central concept of the work is "nation-building," "the central process in the life of all the new states of Africa (as we have now defined it), which can be *consciously* directed or at least influenced and which will determine what they are to become" (p. 10). Whatever such a notion means, it seems to include the concept with which it is usually contrasted: state-building. Socialization and institutionalization are treated as inseparable, as is economic development. There is not enough precision in the central concept to argue with, analyze, or even simply restate.

A few ideas flit through, tantalizingly. There is the notion of devolution of power, which appears least fleetingly in the chapter on federalism in the discussion on local government. But how the strengthening of local power structures can contribute to the building of a nation is never probed. There is an unusual and intriguing notion about the relation between class and status divisions, with the suggestion that classless vertical divisions may be conducive to development (or nation-building) (pp. 244–245). But the potentially fascinating idea for speculation and analysis is not pursued. The author strongly asserts the virtues of free enterprise, pluralism, and agricultural investment for development (and nation-building), but the discussion is superficial and never makes any attempt to overcome counterarguments in a debate that has been around for a long time.

Although this book covers the continent, both functionally and by country, I am sorry to say that I can see no purpose it serves. It is not a textbook, a contribution to a great debate, an exposition of new material, or an analysis of timely events. It is a tired rehash of old saws, whose superficiality does not even do justice to the importance of many of the principles it propounds. Pluralism deserves a better defense; even the single party concept deserves a sounder attack.

I. WILLIAM ZARTMAN

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Haiti: The Politics of Squalor. By Robert I. Rotberg. With Christopher K. Clague. A Twentieth Century Fund Study. (Boston: Houghton Mifflin Company, 1971. Pp. 456. \$10.00.)

This book can be divided into three distinct parts. The first is comprised of Chapters 1, 5, 6, and 10, and is supposedly the essence of the work: an analysis of the Haitian political system under Duvalierism. An attempt is made to formulate a theoretical framework characteriz-

ing Haiti as a "predatory" state—although the meaning of the term remains confused throughout the book. A predatory state is simultaneously described as "one in which brigandage is the predominant form of power, where effective power is exercised by praetorian specialists in violence who insure the safety of and respect for the formal center of power" (p. 342) and "as a species of what Harold D. Laswell and Abraham Kaplan (*Power and Society: A Framework for Political Inquiry* [New Haven, 1950]) called virocracy—the form of rule in which the elite is 'recruited in terms of well-being (vigor, vitality, prowess)' (p. 211)" (p. 342n). The author, who has been in Haiti only a few days during past years, has arrived at various sweeping conclusions about the psychological character of the Haitians. He claims that most Haitians show signs of paranoia and that most Haitian political problems derive from certain putative child-rearing patterns.

America was not familiar to Alexis de Tocqueville before he visited it and wrote his last work, *Democracy in America*. But de Tocqueville travelled intensively throughout the United States to observe the country and its people. He kept himself from reading what others had published about it for fear of being unconsciously influenced by them. In writing about Haiti, however, Rotberg proceeded in just the opposite way. His attempt to conduct field research having been frustrated, he collected *everything* written about Haiti he could put his hands on, hired five research assistants and two translators, and held seminars on Haiti at which selective issues were debated. As a result, the book is not the work on national development it pretends to be but is, instead, an instant historiography similar to the many published yearly in the United States analyzing the policies of current American presidents. The wealth of trivial details, intrigues, and rumors reported throughout Rotberg's text help keep it on the level of the lengthy journalistic accounts already published about Haiti (i.e., Diederich and Burt, *Papa Doc*, etc.). For readers familiar with Graham Greene's novel, *The Comedians*, Rotberg's accounts will seem *déjà vu* and quite dull.

The second part of the book—Chapters 2, 3, and 4—is a survey of Haitian history. It is well developed chronologically, but the inaccuracies are too plentiful, and the space allotted to this review too short, to allow this reviewer to rectify them. Rotberg's skimming survey only sketches the events that had made the advent of a Duvalier almost inevitable. The Duvalier phenomenon has been entirely misunderstood by the American press, the United States State De-

partment, and Rotberg's other sources of information. This stage was one that Haiti had to go through in order to make socioeconomic development possible. Although the old elite was soon replaced by a new one, the new elite is clearly more open, more representative, more responsive to the rest of the population. Now that François Duvalier is dead, that his son Jean-Claude is the thirteenth President-for-Life, and that a very competent and reliable group of technocrats are working to achieve the goals of Duvalierism, "Papadocracy" seems, in retrospect, to most Haitians to have been a necessary transitional stage. (This view does not prevail, however, among the small circle of exiles who were displaced either by the Duvalierist social revolution or by the violence that accompanied it. These exiles are as far out of the mainstream of the system as are the Cubans who left Cuba under Castro.)

The third part of the book—Chapters 7, 8, and 9—deals with human resources, the structure of the economy, and policies for economic development. This part, presumably written by Professor Clague, is the only one that seems a worthy contribution to the goals of the work. These three chapters alone would have constituted a good guide for scholars or foreign government officials interested in assessing the need for external support for the development of Haiti—if they were not lost in the midst of trivia.

A good analysis of the Duvalier years is needed to fill a void because, like Argentina's Perón and Brazil's Vargas, Duvalier has been one of the most important landmarks in the development of the Haitian political system. From now on, the history of Haiti will be analyzed in terms of the periods before, during, and after François Duvalier. The violence of his regime will not be ignored (nor does one ignore that violence under Perón or Vargas), but the unique social changes brought about by the regime will determine forever the course of Haitian economic development. Rotberg's book leaves the void to be filled by a more serious study.

JEAN-CLAUDE GARCIA-ZAMOR

Howard University

Scandinavian Political Studies, Volume 5/1970.

A yearbook published by the Political Science Associations in Denmark, Finland, Norway, and Sweden, Chief Ed.: Olof Ruin. (Oslo: Universitetsforlaget; New York and London: Columbia University Press, 1970. Pp. 366. \$10.00.)

In 1622 at the University of Uppsala Chancellor Johan Skytte endowed from his personal

funds a chair in Wellspokenness and Knowledge of State (*vältalighet och statskunskap*). He had earlier been tutor to Gustaf II Adolf, the first king to make Sweden into a great power. In providing for the training of young diplomats in rhetoric and politics, Skytte created what was to become the oldest academic tradition in political science.

Rudolf Kjellén, the founder of geopolitics, in 1923 was succeeded as Professor Skytteanus by Axel Brusewitz whose students in a score of dissertations composed a unique compendium on parliament-cabinet relations throughout Europe, and in a dozen seminar papers compiled the first comparative study of the social composition of legislatures. Brusewitz's *docent*, Herbert Tingsten, in 1937 published a quantitative study of comparative electoral analysis whose title, *Political Behavior*, became the password for a generation. More recently Stein Rokkan, at an institute endowed by a Norwegian shipping magnate and statesman, Christian Michelsen, has become a leader in combining sociological, statistical, and historical perspectives on politics.

In the last twenty years new universities and larger cadres have provided long-needed opportunities for younger scholars. Still, political science in Scandinavia remains intimate enough to allow a scholar to move from political philosophy to quantitative political sociology, or from public administration to international relations.

Scandinavian Political Studies, appearing annually since 1966, provides a selection of original essays in English featuring the best of political analysis by or about Scandinavians. Among the current contributors are five Swedes, three Danes, three Norwegians, two Finns, and one American. Most articles include extensive bibliographies and run to about 15,000 words, which gives scope for sustained argument and well-marshalled data.

Two opening essays examine concepts current in the discipline. Erik Rasmussen (of the University of Aarhus) finds Easton's notion of "political system" more adequate than the critics have allowed. Lars Rudebeck (Uppsala) finds fault with the concept of political development as propounded by Almond and Powell (who get a C), by David Apter, and by his fellow Scandinavian Erik Allardt (both of whom get B's); and pleads for a "logically coherent . . . approach" that is explicit about its "historical and normative . . . premises." Rudebeck's own historical materialism tends to reduce political to economic problems. Dismissing "political modernization" as a concept "narrower" than, but virtually "interchangeable" with political

development, he overlooks the work of Black, Deutsch, Huntington, Moore, Rustow, and others who, under the heading of modernization, have treated just those historical, normative, and international aspects of political change that Rudebeck is rightly concerned with.

The third essay, by Stein Rokkan, is entitled "The Growth and Structuring of Mass Politics in Western Europe: Reflections of [i.e., on] Possible Models of Explanation." The models on which he reflects are those he has built separately in various articles over the years (most of them collected in his *Citizens, Elections, Parties*): (1) A model explaining "the sequence of steps in the institutionalization of formal mass democracy"; (2) one explaining "the timing of the growth . . . of organizations for mobilization of mass support"; and (3) a "unified model" that pulls together these two strands of government and politics, of democratic institutions and mass parties. To integrate these models, he insists, we need "time-specified variables"; and to arrive at a parsimonious list of such variables Rokkan himself proceeds by "paired comparisons." He offers one such comparison between Ireland and Finland, the two ex-colonies among European democracies—and it proves a brilliant sketch. For example, Rokkan suggests that the phased sequence of political crises in Ireland in the last century ("the cry for justice for the Catholic religion, the cry for land, and the cry for . . . independence") had much to do with preventing the kind of polarized class politics characteristic of Finland.

The upshot of Rokkan's article is a two-and-a-half page table that features 31 independent variables, concerning the territorial, cultural, economic, and organizational configurations of the political struggles in Europe since the sixteenth century, and 12 dependent variables comprising the patterns of democratization since the nineteenth century and of party alignment in the twentieth. As Rokkan recognizes, there is implicit here a major "challenge to comparative political analysis: the great question facing the student of comparative development is whether he can immerse himself so thoroughly in the details of national political history and still retain his primary interest in the parsimony of an overarching schema of concepts and propositions" (p. 66). If any one can rise to the challenge—and rise to it both in lucid prose and in computer printouts—Stein Rokkan can.

There follows a suggestive study of "Local Budgeting, Decision-Making or Ritual Act?" by Johan Polsen (Bergen), based on a Norwegian

locality and making fullest use of what we have learned from political scientists on budgets, sociologists on organization, and anthropologists on ritual. "Sex Roles in Politics" by Elina Haavio-Mannile (Helsinki) similarly compares the Finnish evidence on women's voting with what is known from other countries and with available hypotheses by Duverger, Lipset, and others. (Finland, it is worth recalling, gave the franchise to women in 1906, preceded by New Zealand in 1893.) The next three essays take up electoral and parliamentary behavior in Scandinavia. Risto Sänkiäho (Helsinki) applies the Mann-Shapley index to party combinations in Finland. Nils Stjernquist and Bo Bjurulf (Lund) offer a computerized roll-call analysis of party discipline within and voting coalitions among the five parties in the Riksdag. Bo Särilvik (Göteborg) analyzes the Swedish elections of 1964, 1966, and 1968—a period of steady gain for the Social Democrats. Finally, Egil Fivelsdal (Copenhagen) and John Higley (Texas), in a long essay, find that the leaders of the Norwegian Federation of Labor comprise "a relatively closed corporate structure" whose "social backgrounds effectively distinguish them from other key elites" and who "reach power . . . through long and complicated career patterns and mechanisms which ensure experienced, politically seasoned 'insiders' " (p. 204).

A separate section entitled "Reviews" includes concise summaries of recent Scandinavian events of interest to political scientists: Norway's "first regular 'ins-outs' election in September 1969"; the Danish referendum of 1969, which rejected a Folketing measure lowering the voting age to 18; and "The Zig-Zag Course of the Nordek Negotiations." ("Nordek" is the organization for Scandinavian economic cooperation proposed by Denmark after de Gaulle's second veto of British entry into the EEC, and Claes Wiklund details the course of negotiations as well as the relevant data on public opinion and attitudes of organized interests.)

Like its predecessors, the volume concludes with a bibliography (406 items this time) of books, pamphlets, and articles by Scandinavian political scientists or about Scandinavian politics, with titles given both in the original and in English. Among the items I added to my own reading list are a study by E. Wittenberg (in German) on Bismarck's image in Weimar Germany; by J. Galtung, *et al.* (in Norwegian) on Gandhi's political ethics; by S. A. Söderpalm (in Swedish with English summary) on big business and the Swedish transition to dem-

ocracy; by Vilho Lukkarinen on *The Role of Air Power in a Nation's Total Capability and in International Society* (Helsinki, 1969); and by C. G. Widstrand *et al.* on *African Boundary Problems* (Stockholm, 1969). If I knew Finnish, I would turn at once to A. Talikka's dissertation on "Frustration and Democracy: A Sociological Study of Frustration Situations as Preconditions of the Strategies in Political Conflicts" (Tampere, 1969). It is a theme that could not be more relevant in Finland than it is right here in the United States.

A perusal of *Scandinavian Political Studies* provides a welcome reminder that the systematic study of politics can be a truly international endeavor. European scholars in the last two decades have fully mastered American standards of empirical accuracy, explicitness of concept, and quantitative precision. Americans for their part would do well to emulate the philosophic breadth and historical depth that enriches the work of their colleagues in Europe.

DANKWART A. RUSTOW

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The Ministry of Housing and Local Government. By Evelyn Sharp. (London: George Allen and Unwin Ltd.; New York: Oxford University Press, 1969. Pp. 253. \$5.50.)

This is the fourteenth of the New Whitehall Series of descriptive monographs of the government departments of the United Kingdom. Each is ascribed to the Permanent Secretary, though whether some are the outcome of the collective drafting, in which the British Civil Service excels, is a matter for the reader to determine. This one is ascribed to Evelyn Sharp, one of the outstanding women civil servants in whose work the British service also seems to excel, and its personal style indicates that Lady Sharp, Permanent Secretary until 1966, is indeed the author. Unfortunately, the format of the series severely limits the scope of the study, so that the author is inhibited from making a full survey of the relationship between central and local government. This is indeed unfortunate because given her long experience and intimate knowledge of British administration, a wider study and evaluation by Lady Sharp of this significant aspect of government would have been valuable and fascinating.

Within these limitations the book discusses in detail the problems and the main strengths of English local government, which are especially interesting to American readers because of the contemporary strains and stresses in the relationship among federal, state, and local govern-

ment. The English use of central government as a means of ensuring uniform and reasonable treatment for the individual citizen, implicit throughout the whole range of the Ministry's activities and reinforced by the regular process of parliamentary criticism, contrasts with the contemporary reaction in the United States against Washington and all it stands for. The relative ease in achieving compromise and consensus in English government is frequently implied, but the author deals frankly with areas of difficulty and disagreement, which as she indicates can sometimes be both violent and protracted.

The value of the study to the scholar is the balanced presentation of each area of the Ministry's functions, and the specialist will find each section largely self-contained, with cross-references where necessary. There is an introductory historical chapter and a concluding description of the staffing and organization of the Ministry, which has recently been merged into a new Department of the Environment. The chapter dealing with the organization of local government is necessarily incomplete because local government in England is passing through the most important phase of reorganization since the County Councils Act of 1888, and neither the necessary legislation nor the process of reorganization will be completed for another three years.

There is a particularly interesting chapter on housing, which describes the way in which, over several decades, the municipalities have become more and more deeply involved and suggests that within a relatively short time all rented housing, city apartment buildings excepted, will be in municipal hands. In housing the limitations on the Ministry seem greater than in any other area of its activities, yet it has important responsibilities to the citizen for controlling standards, costs, and priorities.

The chapter on public health should encourage defenders of ecology and is an object lesson in the English flair for achievement by the inevitability of gradualness rather than by confrontation. The author suggests that "Britain was the first country to make a determined effort to reduce industrial pollution and still leads the world. . . . The air over the cities of Britain is infinitely cleaner than it was—and will become cleaner still." These advances, begun as long ago as the Alkali Act of 1863, were extended and consolidated in 1906 by further legislation which gave the Minister powers to administer by inspection, now strongly reinforced by a Clean Air Act of 1956. The first Rivers Pollution Prevention Act dates back to 1876, al-

though the real drive in this area began with a White Paper, issued in 1944, which proposed *A National Water Policy*. This led to further legislation: the Water Act of 1945, the Rivers (Prevention of Pollution) Act of 1951, and the Water Resources Act of 1963. River Boards were first established in 1948. The 1963 Act went much further by setting up a Water Resources Board and by replacing the local Boards by twenty-seven River Authorities.

The section on planning gives a useful and comprehensive explanation of what is involved in town and country planning. Beginning with the legislation of 1909, planning has been advanced by a series of Acts, the most recent passed in 1962, and has gradually evolved into a full-scale regional effort. The author does not in any way underestimate the difficulties and intricacies of the problem. She also discusses the relatively slow progress of national parks in Britain and gives a good account of the New Towns and all that has been involved in their development.

FRED TICKNER

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Religion and Change in Contemporary Asia.

Edited by Robert F. Spencer. (Minneapolis: University of Minnesota Press, 1971. Pp. 172. \$6.50.)

The seven essays which comprise this collection were first presented as a series of lectures at the University of Minnesota in 1968. Thus it is hardly surprising that the essays treat very different problems, and collectively provide little in the way of answers to the comparative questions which their juxtaposition naturally suggests. Apart from the very general theme stated in the title, there is only a nine-page introduction by the editor to tie them together. Most of the essays provide useful syntheses of what has previously been written on their respective subjects, and several contain stimulating insights.

In one of the best chapters, Arthur F. Wright analyzes the decline of Buddhism in China during the past fifty years. In addition to the hostility of successive Republican governments and the stigma of wartime collaboration with the Japanese, the Buddhists failed to articulate a meaningful social ethic and a world-view congruent with modern science. The rapid disintegration of Buddhism under Communist rule must be seen as a culmination of trends perceivable in the preceding decades. Marxism-Leninism, like Neo-Confucianism before it, constitutes a total coherent system, and therein lies its appeal to modern Chinese man. Bud-

dhism, on the other hand, "offered no dream of an ideal society since, under its tenets, there could be no such society in a world of illusion and suffering" (p. 25).

Joseph M. Kitagawa's survey of the background of the new religions in Japan is heavily weighted on the side of the historical, tracing the development of Shinto, Confucianism, and Buddhism in varying relationships to each other. The new religions which have sprung up since 1945 are characterized as doctrinally simple and eclectic, mainly led by charismatic founders. Their holistic world views imply a basic unwillingness to divorce religion from politics, and hence the remarkable political phenomenon of Soka Gakkai.

In "Some Religious Components in Vietnamese Politics," I. Milton Sacks analyzes the four major religious groups in South Vietnam—the Cao Dai, the Hoa Hao, the Roman Catholics, and the Buddhists—in terms of their involvement in the political process. The first two—syncretist sects founded in 1919 and 1939 respectively—functioned as powerful political and military forces until their private armies were virtually destroyed by Diem in 1955–56. Sacks's treatment of the Catholics and the Buddhists is very brief, presumably because the political monks in the anti-Diem movement and subsequent events are much better known.

Agehananda Bharati's essay, "Hinduism and Modernization," develops the theme that in contemporary India the process of Sanskritization—the emulation of high-caste customs—is the phase of culture change prior to modernization. Those thoroughly Sanskritized, i.e., Brahmins and the other high castes, go on to the phase of secularization and modernization. Bharati then turns to a discussion of the *modernization of Hinduism*, by which he means the whole movement of modern Hindu apologetics and religious reform, epitomized in the work of Vivekananda. Bharati's numerous and not always illuminating anecdotes make his essay twice as long as the average in this volume. The ludicrous extremes to which some Hindu apologists have gone in attempting to establish the validity of their tradition in the modern world is a point worth making, but it is surely belabored in this essay, chiefly, one suspects, for its entertainment value.

Manning Nash discusses the Burmese experiment in the use of Buddhism for national revitalization. Nash emphasizes the divergent consequences of this involvement—political effectiveness and developmental stagnation:

The political successes of U Nu were in large part due to his ability to mobilize Buddhist sentiment,

to display traditional piety, to embody the religious life, and to exemplify the virtues of the middle way. However, his kind of revitalization of Buddhism did nothing to make it more rational, more modern, more conducive to the urgent problems of national consolidation and economic recovery and growth (p. 120).

The relationship between Islam and democracy in the Indo-Pakistan subcontinent is the subject of Aziz Ahmad's interesting contribution. The nineteenth-century Muslim modernist reaction to the idea of democracy was instinctively negative, for any form of majority rule meant the political dominance of Hindus over Muslims. In the twentieth century, however, Muslim spokesmen increasingly reconciled themselves to the growth of democratic institutions, while pressing for safeguards for the minority community, and ultimately, of course, for a separate Muslim state. Ahmad analyzes the claims made by modernists and apologists that early Islam contained the core of political democracy in the principles of *shūra* (mutual consultation) and *ijmā* (consensus). While he does not emphasize the point, it is clear that he regards these interpretations as the efforts of men forced to defend their tradition on foreign terrain. That is, the West provided the basic assumptions regarding the value of democracy, and Islam had to come to terms with it.

Ahmad then goes on to inquire into the relationship between Islam and democracy in Pakistan since 1947. He discusses the role of the *ulama* in the process of constitution making, and points out that some traditionalist and fundamentalist political ideas were "absorbed or adopted with mental reservations in the constitutional documents" (p. 139). In practice, however, all administrations have sought to mold Pakistan into a modern national state—but not into a modern *democratic* state. The failure of democracy is clearly not attributable to the influence of Islamic ideas. The military government which came to power in 1958, and the military regime which succeeded it, must be explained in secular terms, and these phenomena are common to many countries of the Third World, non-Muslim as well as Muslim.

One can only agree with Professor Ahmad as far as he goes. There is, however, a more subtle question to be asked: To what extent has the political culture of Pakistan been molded by a religious tradition with strongly authoritarian elements? While no conclusive answers are possible, it is worth considering the extent to which authoritarian value systems encased in religious tradition lend support to authoritarian political regimes. It is necessary to consider

not only explicitly political ideas, but the general attitudes which underlie political behavior.

In the concluding essay Robert R. Jay refers to precisely this attitudinal element in his discussion of religious and political conflict in Java. Jay starts with the fact of the 1965 massacre of half a million Communists and their supporters, and then probes Indonesian history for clues to the ideological fanaticism which produced violence on such a scale. He finds a significant pattern of this phenomenon in the several centuries of conflict between orthodox Islam and Javanese syncretism. The pattern of ideological fanaticism did not "cause" the massacres of 1965, but it certainly helps to explain them.

DONALD E. SMITH

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Documents of Korean Communism 1918-1948. By Dae-sook Suh. (Princeton: Princeton University Press, 1970. Pp. 503. \$15.00.)

Although the Korean Communists organized a party as early as in August, 1919, the study of Korean Communism had been a scholarly virgin land for all practical purposes until Professor Suh's work, *The Korean Communist Movement 1918-1948*, was published in 1967. The current volume, *Documents of Korean Communism 1918-1948*, contains sixty-six documents which constituted the main basis of the former volume. For his study of Korean Communism, Professor Suh collected some 124 documents from various archival sources, and selected only about a half of them for this latter volume on the ground of what he calls "contextual importance to the (Communist) movement as a whole." These documents are arranged in six parts or stages, largely in chronological order: the early stage, 1919-25; the Communist parties in Korea, 1925-35; the Communist International, 1928-35; the activities abroad, 1930-44; and after the liberation, 1945-46.

In making the selection of documents to be presented, the author is keenly aware of the problems and possible distortions arising from the political division of Korea today. "The fiercely anti-Communist South condemns Communism in Korea, past and present alike, and does little to study Communism." On the other hand, the Communists in the North, without many ties with the old Korean Communist movement, dismiss their predecessors' efforts as "paltry, having little or no significance for the present." The author notes that "in past studies by the Koreans and the Japanese, only two or

three documents, usually of Comintern origin, were appended either to condemn or to glorify Communism in Korea" (ix).

In a masterful effort to place the selected documents in proper perspective and explain their historical significance, the author begins each of the six parts with a succinct commentary which provides an indispensable historical background—though unavoidably fragmentary. Some of the documents would be nearly unintelligible without these informative commentaries that only someone with author's extensive knowledge of Korean Communism could write.

Also useful are the biographical sketches of many key figures in the Korean Communist movement, e.g., Pak Chin-sun (pp. 12-13) and Pak Hŏn-yŏng (pp. 480-82). Some of these materials provide fascinating insights into little known aspects of the lives of some Korean political leaders who are not known primarily as Communist activists, such as Kim Kyu-sik (pp. 17-18) and Kim Tu-bong (pp. 482-85) whose revolutionary activities began under the Korean nationalists—not Communists.

It is remarkable that this volume, essentially a compilation of documents, biographical and reference data interwoven with straightforward commentaries, succeeds in presenting a comprehensive picture of the tortuous evolution and convulsions of the Korean Communist movement during three decades. The book manages to convey the agonies of Korean Communism—agonies that partly reflected the hapless fate of the Korean peninsula under Japanese occupation and the rootless vagabond existence of many Korean Communists who were barely tolerated in China and Siberia. It is not surprising that a recurring theme of many documents from the early stage of the Korean Communist movement reads:

In order to secure the normal spiritual and physical development of the Korean toiling masses it is necessary to free Korea from the yoke of Japanese imperialism and capitalistic exploitation (p. 48).

Thorny paths of the Korean Communist movement are also recounted in many painful incidents, such as the Alexeyvsk Incident of June 1921 in which the factional struggles of two Korean Communist cliques, the Shanghai and the Irkutsk groups, burst into an armed confrontation in the Russian Maritime Province. The problem of factionalism is shown to have been a persistent *malaise* that sapped the vitality of the movement. A document written by the late Otto Kuusinen in 1931 lamented in part:

In many parties factional skirmishes were taking place for years. There are parties that have become (universally) notorious in this respect such as the Polish and American parties, but the record belongs to the Korean factions (p. 271).

In fact, internecine struggles among the Korean Communists—undoubtedly intensified due to their lean and parasitic existence in inhospitable alien lands—caused much difficulty with the Comintern. Although the Korean Communists were organized as the Korean Communist Party in 1919, that party did not become a member or a section of the Communist International until 1928.

Documents presented by Professor Suh also reveal some new findings and new insights into the politics of Korean Communism. One notable example, introduced in the author's commentaries:

Despite the commonly accepted notion that the Russians masterminded the coalition between the Communists in the north (North Korea) and the Yen'an groups, the coalition was actually initiated by the Yen'an groups, the North China Korean Independence League which changed its name to *Shinmin-tang*. This is made clear by the next document (p. 478).

One criticism of the work is that the author should have double-checked and edited the translations of some documents for the sake of readability.

Professor Suh states his hope that the importance of this study will lie not merely in the presentation in English of hitherto unavailable documents of Korean Communists, but rather in the formulation of a broader framework of inquiry. Using this framework, scholars may be able to interpret the existing documents more intelligently, uncover as yet unavailable key documents, analyze in detail various segments and phases of the Korean Communist movement, and thus enhance the understanding of Communism in Korea. The volume succeeds admirably in whetting serious interests in the study of Korean Communism by providing scores of key documents. Professor Suh's efforts have produced an indispensable reference work for the study of the Korean Communist movement.

JOHN KIE-CHIANG OH

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Social Stratification in Africa. Edited by Arthur Tuden and Leonard Plotnicov. (New York: The Free Press, 1970. Pp. 392. \$10.00.)

The editors of this symposium cite the lengthy list of comparative studies produced by

social anthropologists, to which list their own volume is intended as a positive contribution. But as they rightly state, "scholars have been relatively little interested in stratification as compared with kinship and political organization, as a review of past research in Africa indicates" (p. 2). The editors argue correctly that stratification is related to the "dynamics of social change" (p. 2). In this volume they have collected an impressive set of studies by very well known social anthropologists, all, with one exception, now teaching in American universities. Thus Aidan Southall writes on the A!ur, Arthur Tuden on the Ila, James Vaughan on the Marghi and neighbouring West Sudanic peoples, Jacques Maquet on Rwanda, Allan Hoben on the Amhara and Ronald Cohen on Bornu. Each of these has presented an account of the ethnographic present—the traditional patterns of stratification observed during field research in the past two decades. Three authors—Leonard Plotnicov, Clyde Mitchell, and Pierre van den Berghe—write respectively of modern urban situations in Jos (Nigeria), Central Southern Africa, and South Africa.

Yet the book is a great disappointment. The ethnographic data are certainly valuable, but the theoretical approach adopted by most authors, and in particular by the editors themselves, seems to derive from the most sterile approaches of western sociologists to social stratification. If one had hoped that social anthropologists would bring a breath of fresh air into this field of study, he will not find it in this volume.

The editors seem to be on the right track when they state that "present theories, definitions and conceptual frameworks of social stratification have proved inadequate for dealing with African conditions" (p. 6); but then they continue with the amazing statement that this is because "within [African] societies great differences in wealth or control over the means of production are absent." Notwithstanding, the editors state that they are not concerned with social ranking or with the universal inequality of the distribution of rewards in society. Their concern is solely with stratified societies, that is to say "societies composed of ranked social groups that are organized on bases other than kinship statuses or biological factors" (p. 8). By this definition not all African societies are stratified. Several contributors in the volume discuss societies which we would all agree are stratified—the Rwanda or Amhara, for instance. But to include the Ila in this volume seems to be stretching a point, for the Ila are clearly an egalitarian people among whom slaves are easily absorbed; the division

between free born and slave justifies inclusion, yet could not almost every traditional African society be included on such grounds?

The result of this approach is a static study of the character of these social groups and the comparison between some of the almost infinite variety of patterns which occur in Africa. Problems of definition dominate the editors' Introduction. That hoary question—Should "caste" be used solely for Indian societies?—is raised. How, the editors ask, does caste differ from feudalism, for Rwanda is described as a caste society and Amhara as feudal? Similarly, definitions of slavery and class are briefly raised and also the distinction between social stratification and social pluralism.

What one misses, both in the editors' Introduction and in most of the subsequent contributions is any discussion of the manner in which the dominant group maintains its position in the society, that is, of the power relationships in the society. Missing, too, is a clear distinction between the pattern of stratification as seen by the ethnographer and that perceived by the people themselves in the various societies—and in particular the differences between the perceptions of those in superior and in inferior groups. For a much more penetrating analysis of traditional African society the reader might turn to Ladislav Holy, ed., *Social Stratification in Tribal Africa* (Prague: Czechoslovak Academy of Sciences, 1968), which, significantly, is not mentioned in the volume reviewed here.

P. C. LLOYD

University of Sussex

Kultura Essays. Ed. by Leopold Tyrmand. New York: The Free Press, 1970. Pp. xi, 344. \$8.95.)

As befits a nation distinguished neither by wealth nor power but which regards itself as more cultured than its domineering neighbor, there are two Polish periodicals called *Kultura*: one in Warsaw and another in Paris. The latter is unquestionably the best periodical in Polish: beyond the reach of censorship and at the same time free from the clericalist and nationalist narrow-mindedness and morbid introversion which mar so many exile publications.

The selected essays under review provide a fair sample of the kind of material which appears in *Kultura*; and while they show that it is one of the most highbrow general periodicals in the world, they hardly add up to anything that might be called a book in any but the material sense. The articles on art and literary criticism call for no comment here, since only the one

on the purges of the Ukrainian writers is of direct political interest. Though moving, the eyewitness accounts of the Hungarian and the Czech tragedies, and of the death of an old Bolshevik contain no new factual information. The author of the highly intelligent and sensible (though not very original) general article on ideologies—Alexander Herz—can hardly be blamed for having accepted the prevalent view about the end of ideology, although it is to his credit that he was less sure about it than most people at that time. As all its heroes have already faded into unimportance, Z. A. Jordan's massively erudite article on revisionism is by now of interest only to specialized historians who can read the book in English of which it forms a part. As Stawar's book is only available in French and Polish, the extracts included here (though not very well chosen) will be useful to the English reader, especially on account of their history. When Stawar was given a state funeral in Warsaw, the bosses did not know that he had asked *Kultura* to publish this manuscript. Although it contains a number of intelligent remarks for the benefit of elderly Polish exiles prone to hallucinogenic nostalgia, the last and longest article in the collection suffers itself a bit from this very tendency, as its author does not seem to realize that "a political line" of a tiny group of exiles will not make the slightest difference to the march of events, and therefore is of little interest to anyone else. *Kultura's* more important task is to keep up the Polish cultural and intellectual standards in the fields which are stifled in Poland.

There is one article in the collection, however, which makes it a necessary item in every library where recent political history is studied: namely, Miedzinski's account of how he negotiated with Stalin through Radek on Pilsudski's behalf, and how Pilsudski tried to organize a preventive war against Hitler.

S. L. ANDRESKI

University of Reading, England

Power and Conflict in a Mexican Community:

A Study of Political Integration. By Antonio Ugalde. (Albuquerque: University of New Mexico Press, 1970. Pp. 193. \$10.00.)

This study of political dynamics in the medium-sized Mexican city of Ensenada represents a refreshing addition to the growing body of literature on Mexican politics, an area of research in which interpretation has often outrun data. One of the few available empirical studies of politics on the community level, the book contains a wealth of detailed information on

the organizational life of the community's residents, along with welcome attention to informal aspects of the political system such as corruption and the daily routines of government bureaucrats. The liberal use of illustrative quotes and cases greatly enhances the study's readability.

Unfortunately, this rich descriptive material is only loosely connected to a central thesis, making the study's argument hard to follow. As the author indicates, Ensenada cannot be regarded as a typical Mexican town because of its privileged tax status as a duty-free zone, its image as a frontier town filled with opportunity, its rapid growth by migration, and its proximity to the United States. Features of Ensenada's politics such as the existence of a strong opposition party (PAN) and a lack of polarization along interest lines are at times attributed to the community's unique location and social characteristics and at other times presented as the leading edge of a trend that will spread as other Mexican communities reach Ensenada's relatively high level of economic prosperity. Similarly, Ensenada's power structure is described initially as a microcosm of national politics containing all major groups (except student organizations) and revealing "the essential components of conflict and the essential processes for resolving these conflicts in the Mexican system." As the book develops, however, Ensenada emerges as a rather distinctive local situation in which labor and political organizations bear little ideological resemblance to their national counterparts.

Perhaps the most serious shortcoming of the study is its assumption that conflict is inherently dysfunctional and ought to be avoided at all costs. A conservative value judgment is smuggled in along with this assumption, since conflict resolution is one side of a coin whose other side is preservation of the status quo. Any given social system will serve the interests of some of its members better than others and thus will not be equally functional for all. If the system involves great inequalities in the distribution of wealth, political influence, and access to basic social services, then conflict may be highly functional from the point of view of the disadvantaged, even though it costs the social system some stability and integration.

The author's commitment to the concept of functional integration seems to prevent him from drawing some obvious conclusions from his data. The political system he describes is dominated by a relatively small group of prominent economic and government figures plus the "co-opted" leaders of various organizations.

Some of these leaders represent segments of the population whose interests conflict, but the leaders themselves are in agreement on many issues and capable of a high degree of cooperation. The elite (our term, not his) is linked together in a well-described system of political brokerage that has "reduced social tensions and enhanced the integration of the society" (p. 180). This integration, however, is achieved at the expense of the poor, who remain both politically marginal and economically deprived despite their nominal membership in organizations that formally represent them and engage in sporadic "welfare" activities and "good will gestures" to maintain their support.

The author maintains that "the lower economic groups . . . are not very concerned about the quality of [public] services; they are pleased if their children can go to school, if they receive some medical attention, if they have water piped into their homes for four or five hours at night" (p. 129-30). They do not, he reports, regard violence as a legitimate response to neglect or unfavorable treatment. Neither do they show signs of nonviolent political militancy at the polls; while PAN's support is growing, its program is virtually indistinguishable from that of PRI, and its voting strength is not drawn disproportionately from the lower class. Indeed, the political sophistication of the lower class is shown to be so low that a recent attempt to hold primary elections for PRI candidates ended in mass confusion. Rather than focusing exclusively on the trade-offs and compromises among members of the oligarchy, the study might have shed more light on the future of Mexican politics by analyzing the sources and supports of this docility, resignation, and political naiveté among the poor, and the mechanisms which reinforce their lack of political mobilization and thus reduce their ability to challenge the system as it presently operates. The author concludes that "in the final analysis, the viability of the present Mexican political system and its increasing democratization seem to be contingent on the possibility of the regime's reducing the system of political patronage without destroying itself" (p. 183). We suggest that in Ensenada, the system's viability is far more dependent on the continued passivity and low aspirations of the lower class, qualities that may prove fragile indeed in a community so exposed to the demonstration effect of rising militancy among the poor in the United States.

CAROLYN NEEDLEMAN
MARTIN L. NEEDLEMAN

University of Rhode Island

Student Politics in Argentina: The University Reform and Its Effects, 1918-1964. By

Richard J. Walter. (Basic Books, Inc., Publishers, 1968. Pp. xviii, 236. \$7.50.)

This is the first volume of the Basic Books series on student politics, edited by Seymour Martin Lipset (subsequent volumes have been devoted to French, Indian, and Chilean students). It is quite fitting that the first book in the series be a history of student politics in the nation which gave birth to university reform.

Although essentially a history, written by a historian, primarily for historians, a number of questions of interest to political scientists are posed in the book's Introduction: (1) "What have been the ages and socio-economic backgrounds of student leaders?" (2) "What factors have made students politically influential?" (3) "Have there been connections between national political parties and university organizations?" (4) "How influential have been professors, national political leaders, and intellectuals on student thought and action?" (5) "What percentage of the university youth have participated in political activities?" (6) "Has the student movement been of one defined political tendency?" (7) "What has been the relationship between the quality of education and student political activity in national affairs?" (8) "What have been the political attitudes and activities of student leaders after graduation from the university?" (pp. x-xiv) These questions are given brief, partial, and often tentative, answers in the book's Conclusion; however, the seven chapters in between ignore these questions, while concentrating almost exclusively upon historical narrative.

The best parts of the book are probably those sections which describe student activities in 1918 and 1958, two periods of well coordinated political action directed toward the attainment of clearly articulated goals. In 1918, when both the national government and public opinion were basically sympathetic, the students gained acceptance of virtually all their demands for university reform: optional classroom attendance, no tuition, periodic review of professorships, university autonomy [extraterritoriality], and most importantly, student participation in university government. On the other hand, forty years later, faced with the determined opposition of the national government, and having alienated the general public to a great extent, the students' attempt to prevent the legalization of private universities was a total failure.

Also well documented is the political naïveté of Argentine students, especially with regard to

their views of organized labor and the armed forces. For more than half a century now, the students have tried to effect an alliance with the workers, but with almost complete lack of success in the period covered by the book; the closest thing Argentina has had to a labor government—the administration of Juan Perón between 1946 and 1955—was vehemently opposed by the students who considered Perón a right-wing dictator. At the same time, the students have long felt that the armed forces should be "the arm of the people" which can be called upon to depose unpopular regimes, but which should never itself govern—a more than slightly unrealistic position in post-1930 Argentina. Walters refers to this as a "propensity to favor rapid action and change and the corollary failure to anticipate or consider long-term effects and consequences" (pp. 188-189).

Perhaps the most disappointing feature of the book is that it is so badly out of date. Although published in late 1968, the book ends its coverage in 1964; and in fact very little is reported of the 1959-1964 period. This is apparently because the field research for the dissertation from which the book is taken, was completed in 1964. This terminal date, however, is very unfortunate, for in July of 1966 (twenty-three months before the book's Preface was written), President Onganía intervened in all of Argentina's national universities, abrogating most of the principles of university reform. Complete control over higher education was transferred to the Education Secretariat; student representation in university government was eliminated; and all forms of political activity within the university were banned. Thus the Argentine university system of the last five years bears little resemblance to the system during the 1918-1964 period as described here.

Although this is certainly not a "how-to" book, it might profitably be read by both students and administrators in United States universities, especially if they would consult the works of Brignardello, Ciria, Inglese, and Silvert to bring the story up to date.¹ University administrators might learn that student participation in university government, at least in the case of the first nation to try it, does not automatically lead to chaos, but rather that students are capable of contributing substantially to the modernization of higher education. At the same time, students in this country might well reflect upon the fate of their counterparts in Argentina—and in Brazil, Mexico, and Peru as well—for when the Argentine students resorted increasingly to violence, they lost almost all their

earlier public support, and thus proved to be virtually impotent in the face of an openly hostile military government. The predictable result was the immediate loss of all their hard-won gains of the previous half century.

PETER G. SNOW

University of Iowa

The Changing Structure of Europe: Economic, Social, and Political Trends. By Robert H. Beck, Harold C. Deutsch, Philip M. Raup, Arnold M. Rose, and John G. Turnbull. (Minneapolis: University of Minnesota Press, 1970. Pp. 286. \$9.50.)

The study of regional integration, in Europe and elsewhere, is in need of genuine interdisciplinary work. To date, the field has been largely dominated by political scientists and economists working separately with different paradigms. Political scientists have been primarily concerned with identifying, measuring, and theorizing about the relationships among different dimensions of integration—focusing on political institutions, elite and mass attitudes and interactions, as well as a variety of social and economic transactions. Economists have been less concerned with models of integration than with theorizing about its consequences for international trade and total global welfare, economic growth within the integrating unit, and even with its impact on business practice and life style. Somewhat to my surprise, economists have done relatively less empirical work than political scientists—one notable exception being Lawrence Krause's *European Economic Integration and the United States*. Only Amitai Etzioni, among sociologists, has shown a significant concern with integration.

This lack of interdisciplinary work is always something of a tragedy for the social sciences. Whether we are interested in public policy or in social processes, disciplinary boundaries virtually always limit our vision. At best, these boundaries are an analytic convenience which justifies a partial paradigm. At worst, they are historical curiosities which encourage and excuse our limited vision. In the field of integration studies, the problems are compounded since it is scarcely possible to study integration within the confines of a single discipline nor, in my judgment, can the component parts be satisfactorily extricated from one another. Accordingly, scholars must either learn the very difficult skill of transcending their disciplines, or we must find ways to pool our concerns so that we can begin to pull together.

For these reasons, it was with considerable anticipation that I picked up this volume on

The Changing Structure of Europe by a "research team" from the University of Minnesota, composed of an historian, Harold C. Deutsch; a sociologist, the late Arnold M. Rose; an economist, John G. Turnbull (assisted by Jean Belden Taber); an agricultural economist, Philip M. Raup; and an educational philosopher, Robert H. Beck. So much about the project seemed promising. It had apparently been carefully nurtured in faculty seminars between 1963 and 1966. The field work included ample time in Western Europe and extended to a summer in Eastern Europe for three members of the team. The authors chose to range broadly across trends rather than to zero in on a narrow and precise dependent variable. I began to read with the hope that some new ground might be broken and that we would, at the very least, begin to see the rewards beckoning to those who were willing to press ahead with interdisciplinary work.

Perhaps my expectations were too high, particularly in light of the rather modest claims made by the authors themselves:

This is intended to be a summary volume on the research of the five contributing scholars, and it is addressed primarily to the interested reader, not the specialist (pp. 8-9).

Moreover, since *individual* detailed monographs by each of the scholars are supposed to follow this collection, perhaps interdisciplinary integration was not the overriding concern of those involved in the project. In any case, the book, as it turns out, is a rather uneven group of essays. If each were as compelling as the work of agricultural economist, Philip M. Raup and sociologist, Arnold M. Rose, then the book could be highly recommended—with or without a real interdisciplinary payoff. But the other essays tend to be rather cursory, although economist John G. Turnbull does bring together a good deal of information in an altogether satisfactory manner. Moreover, I must question the theme that holds the volume together: the premise that integration is largely a non-zero sum game from which the entire West—whether considered by region, by nation, or by social class—will benefit in the short as well as the long run. The authors all, in other words, lean rather heavily on "end of ideology" pluralist assumptions. I consider this unifying theme unfortunate because it is unquestioned and largely implicit. The result is that almost of all of the participants seem to think of integration as all benefits and no burdens. Certainly, there is little in the way of a sustained effort to make a balanced appraisal of burdens and benefits.

Philip M. Raup's study of the "constraints and potential in agriculture" is, on the other hand, a first-rate work of political economy—sensitive to social, political, and economic variables. Raup traces the way in which Common Market policy makers have been constrained by cultural traditions and the resultant structural rigidities to choose an agricultural policy which will be increasingly costly within the member states. He then shows that this process is likely to lead directly to a crisis of authority at the regional level, and that it is pregnant with potential conflicts with the rest of the world in general and the United States in particular. Professor Raup's brief essay is clearly the best analysis I have seen of the subject.

Arnold M. Rose's inquiry into cross-national migration is less far-ranging in scope but more ambitious in its effort to test hypotheses and uncover findings. Its essential conclusion is that, despite national differences, the

cultural and political background of the migrants—by country, not as individuals—has little or nothing to do with the kind of adjustment they make in the immigrant society . . . what is important for integration and adjustment is the openness of the *immigrant* country—certainly in its overt policies, programs, and practices and probably in its informal attitudes as well (p. 217).

This finding must be taken as tentative given the rather simple conceptualization of certain variables and the questionable methods used to quantify them, but despite this reservation the essay remains a useful, systematic, and challenging beginning. Professor Rose soberly helps us to keep his cautiously optimistic findings in perspective by suggesting that, measured by the American experience of the pre-1921 period, "The Integration of Peoples" in Europe has been limited. "But—even at the height of the agitation of racist hatred—there was almost no one who insisted that the migrants return to their homelands after a period of labor in the United States" (p. 220). Of course, the American experience suggests that immigration may either be more or less than integration and, therefore, leads us to some more fundamental questions about the premises of Professor Rose's essay.

Unhappily, this volume tends to fall between stools. As an introduction to European integration it is not as systematic as it might be in covering the terrain and presenting findings. Professor Rose's essay, for example, does not—nor was it intended to—assess the growth of a transnational society, generally; it deals only with migration. Moreover, while some of the essays are professional in tone and make origi-

nal contributions, others are cursory. Most important of all, the interdisciplinary promise of the book is not really fulfilled.

STUART A. SCHEINGOLD

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The Soviet Union and the Law of the Sea. By William E. Butler. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 245. \$12.50.)

Professor Butler of the University of London has given us a book that is topical in two respects. In the first place, he describes an evolution of Soviet theoretical views and diplomatic practice in an important area of international law and international relations, following the change in the status of the Soviet Union as a great power. In the second place, he analyzes the Soviet position in the light of the development of the rules of the law of the sea, which is an area of vigorous change and reform, resulting from technical progress and from the hunger for minerals and raw materials.

In spite of its revolutionary posture, the Soviet government has generally clung to the position of the imperial regime and has adhered to the accepted standards of international conduct. Some of the Soviet government's behavior was voluntary, dictated by the best interests of the country; some of it was enforced by the other powers (namely, Great Britain). This period of conformity and continuation, which according to Professor Butler lasted until 1960 (though some change in Soviet attitude came to the surface in 1946), occurred because the Soviet Union had no navy or merchant marine to speak of, and therefore had to meet other competitors on their own terms.

Since 1946, Professor Butler points out, the Soviet government insisted on increased strictness in enforcing its internal legislation regarding fishery rights. At the same time, theoretical writings in the Soviet Union acquired a new tone and formulated new aggressive demands. The extension of the concept of territorial waters, as well as the revival of such notions as "historical" and "closed" seas, and the reinterpretation of the freedom of the seas—all these were vigorously promoted and defended, while the institution of "innocent passage" was attacked.

The new tone of the Soviet legal literature was the result of two forces: Increased security concerns, and expansion of the Soviet navy spurred Soviet writers to criticize the traditional rules of international law as a basis of the claims of other maritime powers. And yet, Soviet literature must not be read as an interpre-

tation of Soviet diplomatic policy. As Professor Butler puts it: "... Soviet international legal doctrine is often more important for insights into contemporary policy trends or preferences, than as a reliable guide to modern state practice" (p. 143). I have found this observation to be true in other areas of Soviet international relations as well.

A subtle, but demonstrable, change in Soviet sea policy occurred when the Soviet Union became a major sea power. As its fishing fleet came to be perhaps the most sophisticated in the world, the Soviet government turned its attention to another direction. Insistence on security interests became muted. Freedom of the seas and navigation took precedence over the exclusive control of coastal waters, the doctrine of innocent passage was returned to favor, and closed sea doctrine was de-emphasized.

From a general point of view, Professor Butler argues very convincingly that there is no connection between Marxism-Leninism and Soviet approach to substantive issues of the law of the sea—an observation which is true of all other areas of international law. Thus, Professor Butler concludes that Soviet collaboration with other powers on the law of the sea not only has occurred but can reasonably be expected to continue.

The book is organized by subject rather than by periods. In seven major chapters the author deals with such topics as: (a) legal regime of territorial waters; (b) the regime of internal sea waters; (c) closed sea doctrine; (d) the continental shelf; and (e) the deep sea bed. The chapters on the continental shelf and the deep sea bed are of particular interest in view of Soviet innovations (in the Finnish case) in attempting to solve the delimitation problem and the still unresolved deep sea bed problem.

The book ends with a chapter containing a broad analysis of Soviet doctrine and practice; maps, tables of statutes and treaties, bibliography and an excellent index further commend the book to the attention of the reader.

KAZIMIERZ GRZYBOWSKI

Duke University

The Growth of World Law. By Percy E. Corbett. (Princeton: Princeton University Press, 1971. Pp. xii, 216. \$7.50.)

The Structure of Impartiality: Examining the Riddle of One Law in a Fragmented World. By Thomas M. Franck. (New York: The Macmillan Company, 1968. pp. 344. \$8.95.)

With the end of World War II the nations resumed their customary roles as makers of in-

ternational law and simultaneous guardians of the peace and of sovereignty. As Wolfgang Friedmann clearly elucidated in *The Changing Structure of International Law* (1964), there was an acceleration of lawmaking that was a reflection of and response to the increasing number and variety of contacts between states that were fostered by public and private international organizations, science, technology, business methods, and a concern for underprivileged peoples.

Once again, lawmaking does not seem to have made the nations effective guardians of the peace, unless one chooses to treat as a major accomplishment the containment of violent conflict within the bounds of limited war and conventional weapons. Many are uncomfortable that twenty-six years of violence thus contained, slightly longer than the interval between the outbreaks of the two World Wars, is no indicator that guardians of sovereignty can also be guardians of peace.

Among the many who doubt the portent of accelerated lawmaking in a world still structurally decentralized are the authors of the two books under review. This is not to say that either author underrates the significance of international lawmaking. As Percy E. Corbett says at the outset of his book, law is essential to the organization, operation, and progressive development of community. But he regards it as no substitute for the common interest without which no community can come into being. No one who employs the state as his model of a community—and most Western scholars do so despite crime in the streets and other manifestations of disorder—is likely to disagree with Corbett's judgment that the level of common interest among nations hardly corresponds to the aspirations expressed in the ancient concept of a world community governed by natural law.

In concise prose, Corbett reviews progress toward "world order" in the sense of a supranational community that would not necessarily be a community of states. Nor, presumably, need world order be a philosopher's millennium that, as Corbett says, sustains man's faith in his potentialities. Insofar as there has been progress toward world order, it has been in the form of half measures taken in the effort to control forces endangering mankind. These successive half measures provide the substance of Corbett's review of the development of public international law, progress in world and regional organization, the relationship between law and war in the light of Vietnam, and the problem of controlling international propaganda.

Corbett occasionally suggests things that might be done to improve international law. For example, to avoid the farce of an invitation to another state to intervene on behalf of a political faction as in Czechoslovakia and Vietnam, Corbett suggests a convention requiring collective procedures that would be both a first step toward badly needed enforcement by defining just what is to be enforced and an educational device. Indeed, a sustained campaign of popular education is urged as essential in order to counteract the myth that the state, governed by men who reject the evidence that it is an anachronism, is the best provider of peace, security, and welfare.

It is rather commonplace to assert that evolution toward a world order significantly different from the present international system requires, among other things, that international law as *ius inter gentes* make room for world law as a law for individuals, corporations, and other private organizations. Students of international politics concentrate so heavily on governments that they produce little to parallel the lawyers' study of the "new law merchant." That law is evolving from twentieth-century commercial arbitration, particularly by the International Chamber of Commerce Court of Arbitration at Paris; it is evolving even though its body of precedent is limited because records are made public in only those of the more than 2000 disputes since 1924 that have gone to formal adjudication. Corbett's section on commercial arbitration is nicely complemented by Franck's plea for the denationalization of disputes between private persons that are needlessly magnified when transformed into disputes between states merely to render them justiciable. Worse, keeping private persons out of international courts impedes the growth of a large body of gap-filling precedent.

Third-party procedures are the specific focus of Thomas M. Franck's *The Structure of Impartiality*, a work marked by its literary elegance. Franck also gives attention to the "power syndrome" (one-party imposition of its will by force) and the "compromise syndrome" (two-party lawmaking). But it is the "impartiality syndrome" (third-party lawmaking) that Franck examines with the thought that "the failure of the international community to develop a system of third-party lawmaking comparable to that of the national community may well prove to be the fatal error of our civilization" (p. 46). Aware that Western, but not Eastern, religions entail a constant judgment of man by God and of God by man and have helped to produce a society dedicated to judg-

ment, Franck seeks to utilize the habit of judging by applying it more broadly to the disputes of nations under circumstances that ensure that impartiality is both present and visible to the litigants.

Franck's exposition rests on the hypothesis that the impartiality of third-party decision-makers is directly related to the extent to which the community utilizes third-party procedures. Franck treats the impartiality of a judge as the ability to perceive and work with hypotheses that have no provable validity, but are a shorthand for group communication of group values. Such treatment helps to illuminate the problem of the international judge who seeks commitments from societies that operate within different systems of legal hypothesis. Franck then examines such matters as the need for equality of impartiality, the conventional wisdom of such barriers to greater use of third-party procedures as reliance on nuclear deterrence, gaps in international law, and the psychological aspects of impartiality.

However, Franck's attention to the psychology of third-party decision making rests chiefly on older psychological studies and concepts. One's appreciation for his attention to the psychological dimension is thereby tempered with the wish that the wealth of more recent studies and experiments had been taken into account. One could also wish that both he and Corbett had considered whether third-party procedures at the international level are at about the same stage of development and utilization as those within nations at a time when the power to raise money and armies lay with local lords rather than with central governments. Doing so would seem to be a proper route to understanding the international legal system if one's model or reference point is to be the national legal order.

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The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam. By Alexander L. George, David K. Hall, and William R. Simons. (Boston: Little, Brown and Company, 1971. Pp. 268. \$3.50.)

In order "to formulate a theory regarding the uses and limitations of coercive diplomacy," George, Hall, and Simons have undertaken a systematic analysis of three recent crises in United States foreign policy: the Laos crisis of 1960-61, the Cuban missile crisis of 1962, and the Vietnam intervention of 1964-65. George supplied the introduction, the theoretical framework, and the analytical conclusion,

as well as the chapter on Cuba; Hall contributed a discussion of the Laotian crisis, and Simons, an analysis of the decisions to commit ground forces in Vietnam. The three chosen instances of the use of coercive diplomacy and the events surrounding them are described as they appeared at the time to American decision makers, and the narrative is documented from open, mostly press, sources. The accounts are substantial, informed, and reliable; and even though, as accounts of policy making, two of the three—the Laotian and the Vietnamese—have been superseded by the Pentagon Papers which appeared in June-July 1971, the main line of the argument does not appear to have been significantly affected.

The authors view coercive diplomacy as one of several alternative strategies for employing force in international relations, and one that is distinguishable from three other types of controlled use of force: the quick decisive military strategy (the "blitzkrieg," Hungary 1956); the strategy of attrition (as in guerrilla warfare); and the test of capabilities within very restrictive ground rules (the Berlin blockade). By contrast with these, in coercive diplomacy force is used in an exemplary, demonstrative manner, in discrete and controlled increments, to affect the opponent's will rather than his capabilities. Much of it consists of the use of threats (that is also blackmail), but "if threats alone do not suffice and force is actually used, it is employed in a more limited, selective manner" (p. 18). The two basic variants of coercive diplomacy are the "try and see approach, the weak variant, and the tacit ultimatum approach, the strong variant" (p. 27).

The principal finding of this series of studies is that this strategy, in spite of its attractions (above all, its economy) is viable only when a special set of eight conditions is present (including, for example, the strength of American motivation and the urgency and clarity of American objectives, the adequacy of domestic support, or the availability of usable military options). All eight conditions were present in the Laotian and Cuban cases but most were missing in Vietnam, and it is the absence of these contextual conditions that, in the authors' analysis, accounts for the failure of the intervention.

Even if the conditions are favorable, however, large problems of operationalizing the strategy remain. These include the risks inherent in each resort to an ultimatum, the problem of timing strong coercive threats, and so on. "For all these reasons," concludes George, the principal analyst,

there will be few crises in which coercive diplomacy—and particularly the strong variant of it . . . will constitute a feasible and useful strategy . . . Military power is of greater value to diplomacy when it is not used too often. When it is used, it should not be relied upon to accomplish difficult and complex objectives. . . . The wise statesman of a powerful country . . . would be wise to threaten only on behalf of the most important of his country's interests and to make modest, not far-reaching demands on others. It is prudent, as well as moral that the strong should respect the vital interests of the weak" (pp. 250-252).

On this cautionary note the book ends, and it is difficult to dissent from its prudent and circumspect judgments. These judgments carry additional weight because the three case studies, in addition to furnishing theoretical distinctions, also facilitate an assessment of the policies of "controlled, flexible response" inaugurated in 1961 as a substitute for the strategy of "massive retaliation" of the Eisenhower era. The Kennedy administration's search for a "more diversified set of military options along the entire spectrum of violence . . ." was expected to open the way for "many more discriminating and effective uses of limited force on behalf of a variety of foreign policy objectives" (p. 7). The analyst aptly comments that "this expectation has not been fully realized" and proceeds to analyze the reasons, including those for the "Vietnam tragedy" in terms close to the wisdom of classical diplomacy (see especially chapter 5).

There is, however, a flaw in this approach. Not unlike earlier strategic theorists (from whom he differs in other important respects), George views force primarily in an instrumental fashion, to be manipulated more or less skillfully, in greater or lesser accord with available contextual advantages. His theoretical analysis does not clarify the purposes for which American force should be used, and it understates the degree to which the employment of force alters and contaminates these purposes. It regards the national interest (as laid down by the executive branch, for the Congress is curiously absent) as an adequate definition of these purposes and is content to assert, in realist fashion, that coercion is justified in defense of vital interests.

But the matter cannot be laid to rest so easily. Theoretical analysis must take note that the worldwide responsibilities and the global military forces of the Great and Super-powers call for criteria of purpose that go beyond the national. Nor is it irrelevant or "utopian" to observe in a discussion of coercive diplomacy that in signing the Charter, all members of the

United Nations solemnly took on the obligation of refraining in their international relations, from the "threat or use of force" (Article 2, paragraph 4). What Great Power can afford to risk forfeiting its legitimacy and the moral basis of its position by habitually engaging in what could be, in fact, illegal practices, on the grounds of defending its interests?

The answer must be, of course, that (barring exceptional circumstances) force can safely be employed at the global level, if it is employed at all, for societal purposes alone. If the Great Powers control most of the world's military force, the threats to use such force, always dangerous, must be governed by purposes wider than national. This would mean that the means of coercion need to be tied to political structures of broader responsibility.

The three cases—Laos, Cuba, Vietnam—show little evidence of concern for such responsibility on the part of policy makers. This concern would have had to be shown not only in intergovernmental consultations or discussion in international organizations, but also in the form of a conscious design for alignment with major sectors of world opinion. The global system of Great Power authority, however, makes only rudimentary provision for world responsibility, and for this reason each crisis and each threat or use of force is a source of new danger.

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The Fulcrum of Asia: Relations Among China, India, Pakistan, and the U. S. S. R. By Bhabani Sen Gupta. (New York: Pegasus, 1970. Pp. xvi, 383. \$8.95.)

China's World: The Foreign Policy of a Developing State. By J. D. Simmonds. (New York and London: Columbia University Press, 1970. Pp. ix, 260. \$10.00.)

These two books and their authors share a number of similarities. Both writers are men who have close personal and professional connections with the areas about which they write. Both men worked on their books at universities with good research facilities. Both subscribe to the generally recognized thesis that foreign relations are strongly affected by domestic considerations. Both men are conscientious researchers and uninspired writers. Consequently, their books are generally useful but frequently soporific.

Beyond these similarities, the books differ in scope and in the nature of their virtues and deficiencies. Bhabani Sen Gupta's book contains 46 pages of notes with a total of 664 footnotes

(some of them a full page long), no bibliography, no maps and a fair number of typographical errors. Sen Gupta, an experienced Indian journalist, makes extensive use of contemporary newspapers and magazines as well as secondary sources. Among his primary sources on China is *Peking Review*, an English-language weekly, which frequently carries long quotes from *Peking People's Daily* and the official Party periodical, *Red Flag*. The author is careful to cite dates and page numbers for *Peking Review* references, but at least three times in nine pages (footnotes 20, 23, and 42, on pages 152, 155, and 159) he cites *People's Daily* by date, but gives no indication whether the articles cited appear in the Hong Kong American Consulate's serial translations (*Selections from China Mainland Press; Extracts from China Mainland Magazines; and Current Background*) or not. Such information would be helpful to those who do not read Chinese.

Dealing as he does with four countries—India, Pakistan, China and the U.S.S.R.—Sen Gupta seems to have fallen into the error, common among specialists, of assuming that most readers will be as familiar with the geography of the areas under discussion as he himself is. I assume that this is why there are no end paper or other maps—an inconvenient omission for those of us interested in but not very familiar with the areas along China's southern and southwestern perimeter.

Sen Gupta (or his proofreaders) have handled the romanization of Chinese and other names rather well but not without some error. On pages 188, 196, and 303, for example; respectively: Chou Yank appears for Chou Yang; Kashmir for Kashmir; and Chin Chia-king for Chen Chia-kang. Such errors in themselves are minor but they are perhaps symptomatic of a carelessness that makes the reader uneasy.

In the preface of his book the author is refreshingly frank about the human frailties to be found among all individuals and governments involved. His first chapter sketches the background of the various national interests in this area and suggests the direction in which events seem to be moving. His second chapter details the arrival and growing presence of Soviet influence in South Asia. The third gives a fair account of the challenges from and to China rising out of the border disputes, Tibet, and the Soviet Union's support of India. The fourth and fifth chapters describe and analyze the events and consequences of confrontation, while the sixth and final chapter spells out some of the perceptual problems of the area's protagonists and would-be analysts.

Sen Gupta's *Fulcrum of Asia*, plodding

though it is, plows some new ground and turns up some ideas which—while perhaps not all brand new—deserve attention. The way in which nationalism in former colonial or semi-colonial nations evolves into something mighty close to imperialism contributes to an understanding of the complexity of Chinese, Indian, and Pakistani claims in the border areas of the subcontinent. Sen Gupta examines the Soviet Union's growing involvement in the subcontinent and the ways in which pragmatism and national interest (especially where China is concerned) tend to outweigh or alter Russian ideological considerations. This is a useful tool in interpreting recent and current developments in this constantly troubled part of the world.

J. D. Simmonds, born in China and long a professional observer of that country, has kept both his book and his footnotes leaner than Sen Gupta's. In citing his sources, Simmonds usually refers his readers directly to the translations. He has limited his scope to one country—China, has avoided secondary sources, does include some maps, and has had better luck with proofreaders than Sen Gupta. However, *China's World* is no more readable than the *Fulcrum of Asia*, and may even be less of a contribution to the literature.

Part of Simmond's problem is that he is working over historical ground already pretty well covered by a number of other specialists. He has produced a chronological account, with some analysis, of China's foreign relations up to 1969. His expressed aim is to contribute to the methodology of foreign policy study by setting "a number of the essential elements" of China's foreign policy line in "a framework of a developing temporal and geographical world view, and all in the context of the domestic strategic dynamism" (p. 1). Unfortunately the work falls short of this ambitious goal.

Simmonds spends considerable time recording the Chinese Ministry of Foreign Affairs' vice-ministerial appointments and its changing allocation of responsibility for its various departments. All of this he tries to relate, with varying degrees of success, to the Chinese domestic scene.

This book might have been simpler and more useful if Simmonds had merely stated at the outset that he was interested in doing a chronology of China's foreign relations with some speculation on possibly significant causes for and results of change over the years. But such an approach, especially if contained in a proposal aimed at getting a year's study leave or funds for research, is probably less likely to be successful than one larded with terms like "domestic strategic dynamism" and intentions of

developing a "new approach" to methodology.

This criticism is not meant to imply that *China's World* is without merit. Simmonds has given a reasonably concise review of China's foreign policy and some useful insight and grounds for speculation on the factors involved in that policy. He reminds the reader of the importance, especially in an agrarian and still relatively unindustrialized and unc cosmopolitan nation like China, of domestic forces and frailties. He sketches clearly the influence on China's leadership of long years of guerrilla tactics and the relation of those tactics to foreign policy. His treatment of the United Front concept and its role in both domestic and international affairs is neatly handled. But his chapter on the "Intermediate Zone" left this reviewer with a feeling of puzzled boredom.

This unevenness of treatment, plus Simmond's occasional speculative reference, as on p. 76, to "untested Cultural Revolution sources" (which are also not footnoted or identified in any other way) detracts from an otherwise adequate sourcebook on Chinese foreign policy.

To sum up: For students of Chinese foreign policy J. D. Simmons had produced a useful chronology with some ideas worth considering; Bhabani Sen Gupta has produced a rather more thought-provoking book on an area and problems about which, to date, there has been too much provocation and too little intelligent thought.

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Old World, New Horizons: Britain, Europe, and the Atlantic Alliance. By Edward Heath. (Cambridge: Harvard University Press, 1970. Pp. 89. \$4.95.)

The Atlantic Fantasy: The U.S., Nato, and Europe. By David Calleo. (Baltimore and London: The Johns Hopkins Press, 1970. Pp. 182. \$7.00, cloth; \$2.95, paper.)

Starting from different perspectives, with markedly different styles and emphases, these two books show substantial agreement concerning Atlantic international relations. Stylistically, Edward Heath is easily distinguishable from David Calleo. Political actor and political analyst have different conceptions of the proper use of language as a tool for persuasion and thought, argumentation and reasoning. On the one hand, with Heath, there is a careful tone of prudence and moderation, an emphasis on the incremental nature of proposed change. On the other, with Calleo, we see a higher emphasis on elegance in metaphor and analogy, on flamboy-

ance and originality. Substantively, Heath is more concerned with the relations of Britain, the new Europe, and the wider world. Calleo, preoccupied with American over-extension in the world at large, deals incisively and at some length with the nature of the Cold War, with Atlanticism as an ideology, and with the more limited problems of NATO, monetary relations, Gaullist foreign policy, and the implications of European independence from the United States.

Beneath these and other differences, however, lies a relatively common understanding of the meaning of history, the nature of present international problems, and the profile of an appropriate strategy for the future. Both authors would move to restructure Atlantic relations by encouraging American withdrawal from Europe and simultaneously going forward with Western European integration. While increasing the military scope of Western European cooperation, they would leave the drawing together of all of Europe for a later day.

The political collapse of Europe after two world wars and the success of American nation building gradually led to massive American involvement in international affairs. Both authors believe that this involvement is excessive, and both would cut it back to an earlier and more modest scale. Heath recalls George Canning's "memorable words" to the House of Commons in 1826 and "reverses the old proposition." "I called the New World into existence to redress the balance of the Old," said Canning. For Heath, "the Old World must now be brought together to redress the balance of the New" (p. 11). Calleo believes that overambitious American "aspirations and worldwide commitments" have led to an "American crisis." He says that "future historians may be puzzled by the present-day rebirth of American isolationism—not because it came at all, but because it took so long in coming" (p. 3).

European cohesion may help to alleviate American burdens. Heath believes that it is now possible that some of the American "responsibility for . . . world security," some of America's "loneliness" can be shared. In this perspective, British "commitments" outside Europe should be maintained. Indeed Western Europe might "in time come to carry out these obligations or some of them jointly as a European mission" (p. 69). For Calleo a more cohesive Europe might shift "the primary burden for Europe's defense to the major Western European states," and "reduce America's external burdens without sacrificing her true interests" (p. 131).

Defense, for each, is one of the main dimen-

sions of renewed effort for European integration. American withdrawal will be facilitated by the creation at last of a European defense system, based largely on existing French and British nuclear capabilities, as well as German conventional forces. Heath believes that "the British and French nuclear forces should be pooled to form a joint deterrent which would be held in trust for Europe" (p. 4), and Calleo agrees in his discussion of "the problems of organizing the European deterrent" (p. 127).

While Germany will continue to supply conventional forces, it will not have its own nuclear weapons. "The United States," Calleo says, "has no interest in a demilitarized West Germany," but "no one wants the Germans to have nuclear weapons of their own" (p. 127). Heath notes "a new determination to stand up for German interests in defense," but accepts the validity of German renunciation of "national control and national ownership of nuclear weapons" (pp. 71, 83).

Such a system will hopefully help to forestall nascent German concern about full rearmament, in the absence of a substantial American presence, while continuing to maintain a strong front against the Soviet Union. "Without reasonable satisfaction from the allies," Calleo believes, "the Germans would inevitably be compelled toward either a separate nuclear force or a close accommodation with the Russians" (p. 128). Heath recognizes a "legitimate interest" of the Eastern bloc in "ensuring that Germany does not emerge as an independent military power dominating Central Europe" (p. 73).

Heath has a somewhat darker view of the Soviet Union, but both agree that wider European unification is a task for a more distant future. Only after the construction of the system described may one hope "for that even wider unity on which rest the hopes of those still enslaved" (p. 84), says Heath. For Calleo his strategy is the prerequisite to "thrust apart the superpowers from their prolonged and increasingly inane confrontation along the Iron Curtain" (p. 145).

This shared outlook leaves something to be desired. In terms of a window on the past, the experience of the last twenty years does not support the assumption that the best way to promote international cooperation is through common military tasks. NATO's problems, with which Calleo deals at length, illustrate this point; and it may well be that the European Communities made what progress they did because, rather than in spite, of the early failure of EDC.

As for the future, an alternative strategy

might take the possibility of American withdrawal from Europe as a first step in reducing military capabilities from one end of Europe to the other, freeing scarce resources for domestic and international activities in other social sectors. Calleo looks in this direction to some extent when he states that "there is much to be said for an inspected nuclear free zone in Central Europe, including both Germanies, Poland, Czechoslovakia, and perhaps the rest of the Balkans" (pp. 129-130). This initiative, however, seems to bear more heavily on some Europeans than on others. Elsewhere, he implies the importance of nonmilitary activities for the super-powers." Perhaps they might even come to believe," he concludes, "that their greatest contribution to the world community lay in the civilization which they achieved at home, rather than the power they exerted abroad" (p. 145). This could just as easily be true for all participants.

Calleo's study is an incisive analysis of the pattern of Atlantic international politics. His emphasis on fantasy, myth, illusion, and ideology, and his hardheaded discussion of the disproportional costs and benefits of Atlantic relations provide a welcome addition to the existing literature. Heath's lectures are most interesting because of his position as British Prime Minister. While the material is drawn largely from standard Atlantic legends, the form which it assumes under the hand of a man at one center of Atlantic power gives it a particular importance.

In spite of their differences, the most significant aspect of the two works may be the substantial degree of policy convergence which their arguments convey. One wonders if such agreement is a relatively chance happening, or whether it reflects some deeper emerging Atlantic foreign policy consensus that, as the American military presence in Europe is finally drawn down, Western Europe, moving as if to fill a vacuum, will expand and coordinate its military capabilities, maintain the division and subordination of Germany, and come to accept the permanent partition of all of Europe. If this is the case, then one may wonder also about the marginal benefits of further exploring the proposition that good neighbors make good fences.

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The Logic of Images in International Relations.

By Robert Jervis. (Princeton: Princeton University Press, 1970. Pp. 281. \$8.30.)

How can a power induce a rival to comply with its wishes while avoiding the actuality, or

even a serious risk, of armed conflict? The problem has many dimensions. Some of these have to do with the balance of power and attempts to improve it, for example, by increasing one's arms strength, or recruiting allies. The present book, however, is concerned with a different dimension of the problem, namely, exercising influence over the other's conduct by suitable communication strategies, usually treated under the heading of diplomacy or propaganda. Mr. Jervis, however, does not discuss either in breadth but focuses attention upon one fundamental component of diplomatic and propagandistic activities, the production of "images."

An "image," as Mr. Jervis uses the term, is that picture of the entire behavioral profile of the communicator which his communications cumulatively implant in the recipient's mind. These "communications" include not only verbal messages but also significant nonverbal acts that provide cues about the communicator's behavioral profile. Mr. Jervis' fundamental contention is that images can have a significant effect upon the recipient's conduct in and by themselves, and, moreover, that such effects are eminently worth pursuing, because the production of images avoids the substantial costs and risks inherent in "balance of power" strategies. Images, he says, are produced "on the cheap."

Verbal and nonverbal communications, the author says, contribute to the generation of images in two fundamentally different ways: they may act (or be interpreted) either as "signals" or as "indices." "Signals," he explains, "are statements or actions the meanings of which are established by tacit or explicit understandings among the actors" (p. 18). To put it more sharply: Communication by signal presupposes a code common to the sender and the recipient; the information encoded by the sender will be the one the recipient will obtain by decoding the signal. Mr. Jervis notes, as a fundamental characteristic of signals, that they can be true or false, and what is more, sincerely meant or deliberately misleading, their text as it stands leaving it entirely open whether they are one or the other. Hence the decoded meaning of a signal can function as part of the sender's image only when it is weighted for truthfulness, an operation necessarily independent of decoding as such.

Communications as "indices" are a different matter. The information they convey in *this* capacity is not that encoded by the sender using a conventional verbal or nonverbal language, but that kind of information content that natural events carry, the decoding of which involves breaking the code of "nature." The index function of a signal, for example, may reside in behavioral symptoms such as uncontrolled voice

inflections (or changes in the rate of heartbeat), that cast doubt upon the truthfulness of the signal. Information conveyed in the index mode will enter into the image as it is conveyed. It would seem, then, that senders have only very incomplete control over the images produced by their communications, since encoded signal information will be weighted by the recipient, whereas index information is supplied involuntarily.

The most fascinating part of Mr. Jervis's analysis of image production, however, is that devoted to casting doubt upon this conclusion. According to him, image producers have many ways of manipulating both the signal function and the index function of their communications. He discusses these techniques in considerable detail, drawing upon a vast body of literature covering many fields (history, psychology, strategic analysis, and so on).

As to the desirable images themselves, Mr. Jervis mainly stresses two behavioral profile elements as essential to impart: "resolution," the readiness to take high risks, on the one hand, and "peacefulness," lack of aggressive intent, on the other. This definition of the main objectives of communication strategy essentially goes back to the deterrence doctrine of the recent past and thus gives the whole analysis a somewhat dated air. This applies particularly to the discussion of some concrete case material, for example, the Cuban missile crisis. The treatment of the problem of disengagement in Vietnam, contained in the last chapter, however, also contains some remarkably original and apt points. Generally speaking, the study has the merit of providing a number of fresh insights into the way in which the self-projected image of the actors enters into the international interaction process.

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The Theory and Practice of Neutrality in the Twentieth Century. By Roderick Ogley. (New York: Barnes & Noble, Inc., 1970. Pp. 217. \$8.00.)

It is a most useful endeavor to present the student of international affairs and of modern history with an up-to-date picture of the law and practice of neutrality, since this specific pattern of political behavior of governments and nations does generally not enjoy great sympathy with the medium and great powers, and certainly not in the Anglo Saxon world. It is, therefore, incompletely known. Mr. Ogley, a Lecturer at the University of Sussex, gives such a picture in a new volume of the well known

World Studies Series edited by James Henderson. He proceeds by reproducing selected documents referring to his subject, with a substantial introduction of his own and short introductory remarks to most of the documents.

Documents? Along with such truly documentary materials as extracts from the Hague Convention of 1907, from the very interesting exchange of notes and declarations surrounding the neutralization of Belgium and its violation by Germany in 1914, from speeches and resolutions relative to the policy of neutrality of the USA in the opening phases of the two World Wars, and many similar texts, the author/editor reproduces a great many passages from historical writing and from articles in political journals. This creates a serious imbalance. Whereas documents in the true sense reflect the spirit of the moment and attitudes of the actors involved at the time of their writing and speaking, the historian's and the politician's writings represent a retrospective description of what has happened or what they believe happened or what they would like to happen. The value for the reader, then, depends entirely on the quality of each presentation, and on his ability to situate the writer correctly.

Among Ogley's somewhat haphazard selections from books and magazine articles, some provide a well balanced picture of particular situations of neutrality, such as Ambassador Hägglöf's brilliant notes on Sweden in World War II, or Emile Cammaerts' biography of King Albert I of the Belgians. Others are of altogether different quality: some partisan such as a book by E. Borchard and W. P. Lage on American neutrality (1940), others pleading a specific cause, such as the piece by Max Petit-pierre, a former Foreign Minister of Switzerland, urging the world to believe that his thoroughly passive and timid foreign policy had gained a new dimension by simply adding to the traditional concept of neutrality the word "solidarity." To this category of special pleas belongs also the final section of the author's introduction to the book, where he concludes his otherwise objective assessment of legal, economic, strategic, and political factors of neutrality with a plea for a neutral Britain, or where he deals with a chimera such as "civilian defense" in contrast to active resistance to an aggressor.

Ogley groups his material historically, in five parts, beginning with the neutrality of Belgium, passing then to World War I, from there to the days of the League of Nations, to World War II, and finally to the atomic age. This classificatory method is not very helpful when it comes

to show different types of neutrality—permanent neutrality, *ad hoc* neutrality, neutralism, nonbelligerence, nonalignment. The author/editor is most successful when he examines the nature of nonalignment as clearly distinct from neutrality; he describes nonalignment as an offshoot of the Cold War and a policy which does not prevent its authors from being violently partisan in other respects. On the other hand, he is vague when it comes to drawing the boundary between permanent neutrality and a neutral attitude in some particular conflict, or between neutrality and the mere fact of not being actively involved in war. It seems strange, for example, to describe the Soviet Union, bound to Germany by the treaty of 1939, waging war against Finland, as a neutral.

In his own parts of the text, the author gives an excellent account of the strategic conditions of neutrality, explaining how the skillful use of deterrence—using military and economic arguments—is a condition of survival of neutral nations, and what the preferred instruments of deterrence of small nations may be. He seems, however, to contradict himself when he asserts that a guarantee by a great power is the strongest military asset of a neutral, having said himself, in another context, that a guarantee by *one* power would be in contradiction with any policy of neutrality.

Such logical contradictions, along with a few material errors pointing to some lack of familiarity with the subject, and the too narrow choice of titles in the Selected Bibliography limit somewhat the usefulness of the book as a true introduction to an interesting and little known subject. Having pointed to this inherent weakness, the reviewer is glad to endorse fully the author's conclusion, when he writes:

. . . neutrality, far from being an anachronism, is a condition that states are likely to find themselves in, by accident or design, with increasing frequency in the last third of this century. . . . The neutrality that we are likely to see will, then, be a somewhat messy neutrality; its rules may be improvised, and the powerful may be able to disregard them. But there will persist . . . a reluctance of third parties to involve themselves in other's conflicts, which may well lead to a dampening down of these conflicts . . . (p. 205).

URS SCHWARZ

*Graduate Institute of International
Studies, Geneva*

Small Nation Survival: Political Defense in Unequal Conflicts. By V. V. Šveics. (New York: Exposition Press, 1970. Pp. 271. \$8.50.)

In recent years the literature of international

relations, which traditionally has been about great-power politics, has been supplemented by a number of useful books about the behavior and role of small states in international politics. While David Vital has spelled out some of the consequences of the material inequality of states for unaligned small nations, Robert L. Rothstein has analyzed the security problems of small powers in alliances. Professor Šveics's concern is with unaligned small nations involved in conflicts with militarily superior powers. Drawing on some older writings about strategy, especially early nineteenth-century German works, and on more recent books about resistance movements (particularly those dealing with reactions to German occupation in the Second World War), he analyzes a variety of unequal conflicts, but notably that between Czechoslovakia and the Soviet Union in August 1968. He concludes that it is perfectly possible for a small nation successfully to defend itself against even a vastly superior military power, provided it knows how to do it and has will-power enough to go through with it.

The way to resist overwhelming force effectively, Professor Šveics argues, is to transfer the struggle from the military to the political level. If the small nation has been defeated militarily and has been occupied by the enemy, the defender should realize that the defeat is no more than an unimportant tactical setback and should continue the struggle politically by resorting to all possible means of noncooperation and resistance. Summoning up its full national strength, the small nation should engage the enemy where *he* is weakest, prevent him from gaining control of the *polis*, erode his grip on the state, and generally intensify the conflict until final victory is secured. Šveics does not claim that the idea of subordinating the military side of the conflict, or stage of the struggle, to the political is new. Indeed, he shows well how the relationship between war and politics was clarified more than one hundred and fifty years ago by A. H. D. von Bülow and Carl von Clausewitz; how the lessons they had drawn from the Revolutionary and Napoleonic Wars were almost forgotten in the nineteenth century; how they were taught again by T. E. Lawrence after his experiences with the Arabs; and how they have been put to use in the resistance movements and people's wars of recent decades. Šveics's forcible restatement of ideas which seem as relevant now as ever before is a worthwhile contribution to a subject in which old insights are sometimes forgotten in the search for new theories.

Nevertheless, it is clearly with action rather

than ideas that Professor Šveics is ultimately concerned. Admitting his deep commitment to the cause of small states struggling to retain or regain national freedom, he sets out to explain the correct strategy for these states. With passion and determination he strives to convince his readers and to persuade them to act on his instructions. The book is essentially prescriptive—offering directions to intellectuals, administrators and others about their roles in resistance to the occupation power, making suggestions for Sweden, Switzerland, and other small unaligned states how best to prepare their defense, and so on. As a guide to resistance movement leaders, and as a warning to statesmen who contemplate subjugating a small nation by superior force, it is a persuasive piece of work, which for its effect relies to a considerable extent on the technique of repetition.

In the minds of the more skeptical readers this book may leave a few queries. Does the author, in his effort to put military power in its proper political perspective, perhaps tend to underestimate the importance of force in international politics? Is it not true, for example, that the survival of the smaller members of the old European states system was due to the balance of power operating among the great military powers at least as much as to the national political strength mustered by threatened small powers? Is he perhaps, in his endeavor to establish old truths about the efficacy of noncooperation and resistance, inclined to ignore improvements in some of the means available to modern governments bent on keeping down hostile nations? Finally, is his adherence to the creed of nationalism, in which he rightly recognizes the main source of resistance, too uncritical? Is it, for example, plausible to argue (as he does on page 134) that nationalism cannot be a source of aggression or an obstacle to international cooperation? Lurking in his distinction between the mature nationalisms of certain small states and the aggressive imperialisms of some great powers may be the notion of the intrinsic moral superiority of small nations, a notion which may be detected on other pages too, and which may be suspected of permeating the whole book. But it is essentially because of their relative powerlessness rather than because of any intrinsically superior virtue that small states tend to behave differently in international relations.

Professor Šveics's book shows again how different the international political process looks when viewed from the angle of the smaller members of international society; and it encourages one to hope that new insights may be

provided by those scholars who examine the states system from levels lower than that of the great powers.

CARSTEN HOLBRAAD

The Australian National University

The Reality of Foreign Aid. By Willard L. Thorp. (New York: Praeger Publishers, for the Council on Foreign Relations, 1971. Pp. 370. \$12.50.)

The Reality of Foreign Aid is another in a steady succession of nuts-and-bolts books on foreign aid. These books carefully take apart the foreign aid process and examine it under the microscope, detail by tedious detail. This is not meant as a pejorative remark. The details of the planning, giving and receiving of foreign aid are important and need repeated examination to permit us to chart changed conditions and profit from past experience. But these details are not the stuff of glamorous, exciting books. Rather, books like *The Reality of Foreign Aid* mute the glib sensationalism of many of the derogatory observations about foreign aid which enliven political rhetoric. They make the reader see that foreign aid is a tough, risky enterprise, bound to require compromises which are displeasing to all sides. For every accusation which seems to highlight glaring misconduct on the part of donors or recipients, there are commonplace explanations which indicate that every mishap is not necessarily somebody's fault, let alone the outcome of evil scheming.

Thorp's book proceeds, more methodically than most, from question to nagging question. Why are donor and recipient nations alike disenchanted with the foreign aid process? Why is it so difficult to bring about satisfactory progress in the underdeveloped two-thirds of the world? How does one go about transferring capital and expertise effectively? To which countries should aid go and for what purposes? How much should each receive and for how long? Should economic transfusions take the form of trade, aid, loans, or other means of transfer? There are also questions about the administrative and institutional aspects of aid giving: Should aid be more heavily private or public? Should it be given through bilateral arrangements or multilateral institutions? What coordinating mechanisms are needed within the recipient and donor countries and internationally?

For each of these questions, Thorp presents a careful analysis of the problem in historical perspective, giving full details of past experiences and of present social, economic, and political factors. He typically follows this analysis

by outlining the advantages and disadvantages of various types of solutions and often by adding recommendations. He has included dozens of helpful tables to illustrate various aspects of the aid flow from the United States and other major donors.

Most of Thorp's recommendations are cautious and relate to minor reforms in aid giving rather than sweeping restyling of the aid process. Mr. Thorp, for five years the Chairman of the Development Assistance Committee of the Organization for Economic Cooperation and Development, is experienced enough in the difficult art of foreign aid administration to shy away from advocating dramatic reforms which excite academic planners but chill the souls of practitioners. Yet it may be just such bold changes which are required to transform the picture of foreign aid successes from one of modest upward trends to one of steeply rising trendlines.

A few randomly selected examples will suffice to indicate the tenor of Thorp's reform proposals: (1) Citizens of developing nations should ordinarily be trained in institutions established in their own countries or areas rather than in developed nations. (2) The burdens of loan repayments, which now strangle economic growth, must be eased through cooperative efforts of the lending countries. (3) Multilateral aid giving should be improved through better coordination of existing bodies, rather than through the creation of new agencies. (4) The United States should assume a larger share of the world's growing aid burden by making long-term commitments for economic development in the Third World.

The volume ends with an appendix on "The Foreign Aid Numbers Game" which debunks all types of foreign aid statistics. The reader is shown, as he has been throughout the book, that appearances are deceptive and that one needs a host of additional information to put every "fact" and statistic into proper perspective.

Though the book is scrupulously objective in its discussions of specific problems, it does not question the basic assumptions behind foreign aid. Thorp briefly examines some of the arguments in favor of foreign aid and, in many scattered comments, indicates that aid, measured by various economic yardsticks, is beneficial for donors and recipients alike. Yet the other side of the coin is hardly examined. We are not told in what respects foreign aid may be detrimental and whether the criteria by which benefits are gauged are adequate and fully defensible.

This reviewer shares Mr. Thorp's beliefs in the value of foreign aid to foster technologically advanced societies throughout the world. Nonetheless, it would be refreshing in a scholarly study to find reappraisal of basic assumptions along with analysis of goals and procedures. In the process of re-examination, an experienced analyst may discover truly new political approaches that may be the key to a more rapid and humanly satisfying development process.

DORIS A. GRABER

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Designs in Diplomacy: Pages from European Diplomatic History in the Twentieth Century. By Mario Toscano. Translated and Edited by George A. Carbone. (Baltimore and London: The Johns Hopkins Press, 1970. Pp. 433. \$16.50.)

This volume brings together eight separate articles and essays originally published by Professor Mario Toscano over a twenty-year period. They were revised and updated several times, the last in the process of being brought together for translation and publication in English. The final work of revision was done by two of Toscano's assistants in the spring and summer of 1968 when the author was already very ill, but he was able to review their work and give it his approval before his death.

Toscano's position as a distinguished diplomatic historian, as chairman of the commission supervising the publication of Italian diplomatic documents (for the period 1861-1943), as special advisor to the Italian Foreign Office in the postwar period with the rank of Ambassador, and as a sometime *éminence grise* behind certain foreign ministers of the 1950's, gave him an unparalleled opportunity to see the foreign policy process at work from perspectives not ordinarily available to scholars.

Despite the disparate issues and long time period covered in this collection, two themes stand out. At the forefront is the theme of failure. From the first essay on the "Failure of the Hungarian-Rumanian Rapprochement of 1920" to the last on the inadequacies of intelligence sources which led to failures of judgment, Toscano is preoccupied with agreements which were never consummated, approaches which fell flat, results which were disappointingly inadequate. Even when partial successes were achieved, as in the reestablishment of diplomatic relations with Russia and France in 1944 and 1945, the political benefits expected remained largely unrealized.

The second theme is Italian diplomacy and

the Second World War. With the exception of the first essay mentioned above, all the others concentrate on different aspects of staying out of, getting into, and getting out of the war. These are not well-rounded syntheses of the whole story. They are a specialist's effort to bring to bear the evidence of newly acquired primary sources on problems which are already well known to his readers. Toscano is writing for a small, knowledgeable, audience of experts, and his essays assume a highly competent readership. In reading them one must remember that they are only part of the story, which is all Toscano intended them to be.

As I read the essays I drew a number of conclusions, some explicit, some implicit. One is that Benito Mussolini and Galeazzo Ciano were lightweights in diplomacy. I already believed that, but Toscano's work drives this conclusion home. The tragedy of Italy, as Toscano indicates, was to have such incompetents in charge. They had a first-rate, skilled professional as their Ambassador in Moscow from 1939 to 1941 but all his talent and expertise couldn't help Italy, given the deficiencies of his political superiors.

Another conclusion involves the character of the Italian-Russian *rapprochement* which the Germans prevented from coming to fruition in the spring of 1941. It was obviously based on the mutual recognition of spheres of influence between Russia and Italy, with Russia granted preeminence in the countries bordering the Black Sea and at the Straits, and Italy receiving similar recognition for the Mediterranean. Toscano remarks that the proposed understanding was strikingly similar to the Racconigi Agreements of 1909 between liberal Italy and Czarist Russia. This led me to reflect on the relevance or lack of relevance of ideology, Communist or Fascist, to foreign policy.

I was struck again by the importance of the "old boy" network. Emissaries are chosen, assignments are made, because someone is related to a *Marchesa* close to Salazar who can influence the Prime Minister to serve as an intermediary with the English. The efforts of the Italians to get out of the war on favorable terms in the spring of 1943 are directed to the British rather than the Americans because the Italian upper classes have contacts with the British aristocracy. It is not exclusively an aristocratic "old boy" network, however, but also a diplomatic one, from which the Soviet diplomats are not excluded. The negotiations for a *rapprochement* between Italy and Russia in 1941 were made easier because Ciano and the Soviet *Chargé d'affaires* in Rome, Helfand, had been

junior foreign-service officers stationed in China together for a number of years. The record also indicates that in the last analysis these personal contacts and influences are not conclusive. None of these links was able to help Italy, with England or with Russia.

This is classic diplomatic history, drawn from the primary documents and the memoirs, written and oral. There is no indication that Toscano is aware of modern theorizing in the area of foreign-policy decision making. None of the models which have been developed—interaction models, organization theory, bureaucratic politics—are evident in this work. I can be accused of criticizing him unfairly, but in a conversation in the late 1950's he remarked that me that his work in the foreign office taught him the limitations of diplomatic documents. As presented here decisions are made by one or two people—Mussolini and Ciano, Hitler and Von Ribbentrop, Stalin and Molotov. Civil and military experts at best provide information, and occasionally an opinion. Mussolini, as a dictator, operating in a world of secrecy and conspiracy, overvalues intelligence information because his mentality gives more credence to data if it is obtained by secret means than by more straightforward routes. But this conspiracy view of history is not limited to dictators, nor is the "spontaneous and completely subjective" way in which Mussolini often made decisions. Perhaps Toscano knew what he was doing when, in spite of personal bureaucratic experience, he decided to follow his "old-fashioned" conceptual framework.

NORMAN KOGAN

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China and the Foreign Powers: The Impact of and Reaction to Unequal Treaties. By William L. Tung. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1970. Pp. xxii, 526. \$15.00.)

As Arnold Toynbee observed in his introduction to the *Major Peace Treaties of Modern History, 1648-1966*, "The modern West has had many good things to give to the world in the field of science and technology, but in the field of international relations the effect of the Westernization of the world has been disastrous." To attest to the truthfulness of this statement, China is a case *par excellence*.

For thousands of years China had lived in its own world, the so-called "T'ien-hsia" (All-under-Heaven), and its relations with the peoples around its periphery—Koreans, Japanese, Vietnamese, Siamese, Burmese and others—were usually conducted in a Sinocentric system

based on the assumption of Chinese superiority. This Chinese world order, generally speaking, was hierarchical in form, benevolent in spirit, and peaceful in operation. In Chinese terminology, it was called "wang-tao" (peaceful way), which was not only different from, but directly opposed to, the "pa-tao" (forcible way) or power politics of the West.

When Westerners came to China in the eighteenth and nineteenth centuries, they first demanded equality with China, and after dealing the Chinese several blows in war they imposed upon China a series of unequal treaties, which almost reduced China to a colony. According to Dr. Sun Yat-sen, the international status of China at his time was even worse than that of a colony. He called China a "hypo-colony" in order to indicate that she had many masters without anyone of them accepting the responsibility for her.

China's response to the challenge of the West was more than one century of turbulent changes, including the Self-strengthening Movement, the "Hundred-day" Reform, the "Double-ten" Revolution, and the May Fourth Movement. Because none of these changes seemed to be enough, mainland China finally went Communist in 1949—a total Westernization in the sense that Communism originated in the West.

This tragedy of modern China is the subject of William L. Tung's book, which contains twelve chapters, four appendices, and numerous notes. The first three chapters cover the historical background. Chapters 4 and 5 analyze the impact of the unequal treaties upon the sovereignty and territorial integrity of China. From Chapter 6 to Chapter 9, the author describes China's reaction to the unequal treaties and their final termination during World War II. In the last three chapters he discusses China's problems as a resurgent great power in Asia and beyond. He concludes the book with these words: "For the fulfillment of her national aspirations, all segments of China are prepared to seek international support wherever attainable, but will strongly react to any confrontations if unavoidable" (p. 410).

Dr. Tung should be congratulated for his excellent work in this book, particularly for its lucidity, brevity, and systematic arrangement. He has made good use of primary as well as secondary sources. It is especially gratifying to find a separate index of treaties at the end of the book. It is even more admirable for a Chinese writer to maintain such fairness and objectivity as are displayed in his treatment of so many controversial problems, including the Yalta

Agreement (p. 271), the responsibility for the "loss" of mainland China to Communism (p. 299), and the international legal status of Taiwan (pp. 310–311).

The main thesis of this book is to prove that "although China's troubles were partly due to her own impotence, the Chinese attitude toward foreign powers in recent decades has had much to do with the national humiliation endured during the past century" (p. vii). To the extent that "to every action there is a reaction" (p. vii), the author may be regarded as successful. He could have done even better, however, if he had gone a little deeper and explained why America was tagged by the Chinese Communists as their number one enemy even though the United States was the most friendly to China among all the Western Powers, and why Nationalist China waived its war claims against Japan even though China suffered the most from Japan during the last hundred years. Here we need an insight into the ideology and political philosophy of the two rival regimes in China.

Another desirable improvement would be the addition of a few maps to those parts of the text where the problem of national boundaries is discussed. The simple map on the front page is not sufficient.

Some terms used by the author are neologisms with dubious value. For instance, to call the Nationalists' purge of Communists a "purification movement" (p. 236) is a translation from Chinese too literal to be comprehended by Western readers. The description of imperialists' penetration of China by means of railways as "railway imperialism" (p. 194) is also a peculiar coinage.

Finally, there are quite a few misspellings and printing errors. Of course, these and other flaws, which can readily be removed when the book comes up for revision, are minor impediments in a very useful book.

SAMUEL SHIH-TSAI CHEN

Central Connecticut State College

Bridge Across the Bosphorus: The Foreign Policy of Turkey. By Ferenc A. Váli. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 399. \$12.50.)

Bridge Across the Bosphorus may be called a survey of recent Turkish foreign relations. It is the only book of that description to have appeared in any language in at least two decades. In fact, although there are some valuable specific studies, mostly by Turkish scholars, we would say there is not much literature of any kind on Turkey's foreign relations since World

War II. Prof. Váli has consulted what there is and drawn much from Turkish newspaper reports and from personal interviews, and has finally given us the benefit of his long interest in Turkish affairs. (His several previous books and many articles in English are all on other subjects.) As an amply informative survey of a set of issues of which we have scanty knowledge, this book is likely to be very highly appreciated. There are apt to be differing views, of course, about its approach to foreign policy analysis and about some of its interpretations.

The introductory chapters present a general historical background, then observations on what the author calls the geopolitical, ethnic and ideological foundations of Turkish foreign policy, and then some description of public opinion. The four central chapters analyze relations with the parties at issue regarding the NATO alliance, the Straits, Cyprus, and the Arab-Israeli and other regional issues. Concluding the book is an evaluation of economic development and a final chapter bringing together the themes of the whole work.

The title, preface, and epigraph ("This bridge signifies Turkey's wish to join Europe."—Willy Brandt) might at first lead readers to expect the popularized, simplistic interpretation of Turkey as a Westernized or unambivalently Westernizing nation. The author does favor Westernizing tendencies, and he may be somewhat optimistic about them, but his treatment is certainly not simplistic. It seems to this reviewer almost always judicious and acute. He points out difficulties and costs: the development plan (including investment in the bridge) requires postponing some needed social benefits, NATO membership probably hinders Cyprus policy, ethnic solidarity might possibly come to endanger peace and security. There are tensions among ideals and many intricate choices involved in the "wish to join Europe," and the main body of the text is a sensitive guide to them.

Related to the major theme of Westernization and its problems are several minor themes that are traditional among students of international relations. They include the importance of geographic conditions, the quality of Turkish diplomacy, the interplay of indigenous and outside cultures, the sometimes drastic effects of Great-Power interactions and ambitions, the subtle relations between national sentiment and calculations of national interest, and also "the commotion created by the small but noisy exponents of red and black fanaticism" (p. 377). Except about Turkey's radical left and right and in a few unimportant summary comments

(compare pp. xi with 68–70 and 35 with 210–218, for instance), a careful critical judgment is sustained throughout.

Turkey, of course, is no longer the close ally of the United States that it was from the late 1940's to the early 1960's. Chapter IV quickly reviews the origins and development of their "Cold War" relationship and then goes on to relate the incident which more than any other dissolved it. Trying to stop an imminent Turkish military intervention in Cyprus in 1964, President Johnson wrote Prime Minister İnönü that the U.S. and other NATO allies might not consider themselves obliged to protect Turkey if a Soviet intervention ensued. That part of Johnson's letter "gives me the impression," İnönü replied, "that there exists between us a wide divergence of views as to the nature and basic principles of the North Atlantic Alliance" (quoted from p. 131). The author remarks:

Up to that time, the Turkish leaders had never seriously analyzed the context of American interests relating to Turkey or the Turkish interest as it related to the United States' role in the affairs of Turkey. That Turkey ceased to follow the American initiatives automatically, but acted after due consideration with regard to national interest, amounted to a change. It was, for instance, realized that all major countries of the Atlantic Alliance have, to some extent, mended their fences with the Soviet Union, whereas Turkey had stayed behind as the last inflexible "Cold Warrior." Similarly, Turkey had completely neglected her relations with the Third World. Consequently, she had found herself voted down on the Cyprus dispute in the United Nations by her official opponents (the Soviet bloc countries), by the neutralists, and also by some of her official allies. Thus the search for new orientations began.

Within a few years Turkish diplomacy was able to bring the country from an uncomfortable isolation to a new complex of relationships . . . (p. 133).

This interpretation of Turkey's "Cold War" phase is not easily confirmed. Perhaps it is true that officials did not adequately analyze either American interests or the risks of a close alliance. Or possibly the Menderes government (1950–1960) did analyze them and decided that a sort of junior partnership best served its domestic interests, as it understood them, while at the same time complying with the requirements of the "free world community" as interpreted at that time by the senior partner and others. In other words the old policy might have been deliberately based on transnational and partisan domestic interests as well as national ones. Perhaps the author would agree, and add that maybe neglect of the national

ones exacerbated the troubles of the later 1950s and helped produce the 1960 Revolution. Thus the interpretation might be more explicitly developed. In any case this book makes its best contribution in treating the new complex of relationships that grew up after 1964, and in setting them in long-range historical perspective.

Historical perspective informs just about everything in this book, and approximately half of it consists of the kind of history-writing exemplified in the *Surveys* of the Royal Institute of International Affairs. Such writing is short on explicit generalization. While manifestly a detailed narrative, it develops some coherent general themes, but leaves them and indeed leaves a whole complex analysis quite unexplicit. Consider the author's description of how Turkey and Greece have been "interlocked nations" (chapter VI). It ends with comments which do make the narrative themes plain enough but which do not form systematized generalizations. For instance: "They display hatred and contempt for each other, yet they are drawn together by self-interest, rivalry, or the admiration of what is different between them" (p. 269). How are they different from the *frères ennemis* that Raymond Aron described? The author seldom goes into what are now called theoretical questions. His comparisons of Turkish with Japanese, Iranian and Ottoman foreign policy are cogent but very brief, leaving the book's value for comparative analysis of a formal kind mostly to the reader's induction. But the volume contributes to political science in part because its kind of history-writing does. The analysis is not explicit, but it is there to be explicated.

Approximately another half of the book treats the most recent period directly. The several issues and the interests and views of different parties are thoughtfully described, in the manner of an experienced observer able to give both precise details and broad reflections. We simplify much by saying that he makes the new and still changing relationships intelligible most of all by showing us their meaning for Turks. For instance, Turks increasingly see foreign policy as tied in with issues of cultural development and national identity, not just with security and economic development. Prof. Váli is sensitive to these interconnections. Perhaps the chief merit of his book for most American readers is its sympathetic yet judicious representation of the predominant (as well as some dissenting) views of Turks.

This is a thorough and perceptive monograph on Turkey's situation in the world. By

lacing a survey with analytic insight and by elucidating Turkish views, it contributes to scholarly and enlightened public discussion.

CARL M. DIBBLE

Ithaca, New York

Revolution and Chinese Foreign Policy: Peking's Support for Wars of National Liberation. By Peter Van Ness. (Berkeley: University of California Press, 1970. Pp. 266. \$6.50.)

There are pitfalls for Western observers who discuss Communist China's support for wars of national liberation abroad. One temptation would be to treat it mainly in terms of the Communist conspiracy and the Yellow Peril myth. The opposite tendency would be to approach the issue with a guilty conscience and an unqualified admiration for the new China. Fortunately, the book under review is a sober and objective study, free of such clichés and predispositions.

The stated purpose of the book is to analyze systematically both the theory and practice of Chinese support for revolutions in all countries of the third world (Asia, Africa, and Latin America). To begin with, Part I addresses itself to the Maoist ideology of the national liberation struggle. Included in the discussion are Peking's maximum and minimum programs for revolution as expounded by Lin Biao. The former, the ideal type, requires Communist leadership and is based on Mao's theory of new democracy, while the latter is construed broadly enough to include almost all vigorously pursued, anti-imperialist revolutions, regardless of leadership. Although the first three chapters of the book contain nothing new in either sources or interpretations, they admirably provide the necessary theoretical foundation of Chinese revolutionary policy for a careful inquiry into what the Chinese actually do in their dealings with foreign countries.

Undoubtedly the most significant portion of the book is Part II, which examines the actual practice of China's support for wars of national liberation immediately preceding the Cultural Revolution. Here the analysis is limited to a single year, 1965, and attention is focused on Chinese public endorsement of revolutions, relations with foreign revolutionary movements, and criteria for selecting countries as targets for revolution. From the findings of his empirical data, the author is able to conclude that national interest rather than ideological consideration has essentially determined China's international behavior. In 1965, for example, the primary factor influencing Peking's selection of

targets for revolution was found to be the relative friendliness or hostility of foreign governments toward mainland China. Citing Burma and four other countries as illustrations, Professor Van Ness points out that countries governed by regimes friendly to Peking were not selected as targets for revolution—even when revolutionary movements were extremely active in those countries, and even though some of the movements were led by pro-Peking Communists.

The impact of the Cultural Revolution upon Chinese foreign policy is treated in the final section (Part III). A sharp shift is shown to have taken place in the pattern of Chinese support for wars of national liberation during the period of the Cultural Revolution and especially in 1967. Great emphasis was placed on replication of the Chinese revolutionary model and the propagation of Mao's thought, often to the detriment of local revolutionary efforts as well as Chinese foreign-policy interests. Nevertheless, the author correctly views this shift as only transitory and maintains that it reflected not so much the advent of a new foreign policy

as the precedence of domestic concerns over international interests.

On the whole, *Revolution and Chinese Foreign Policy* is a well researched book. It presents some interesting information and perceptive comments. It contains ten useful tables and one helpful bibliographical note.

The main difficulty with the book lies in the narrowness of its scope. Although justified from the standpoint of research manageability, the confining of the study to a short span of three years (1965–1967) clearly restricts the value of its findings and generalizations. By the same token, the book suffers from another limitation in that its analysis is focused on Peking's *public* endorsement of foreign wars of national liberation without due attention to other forms of support.

Despite these shortcomings, the volume proves to be an important work and will serve as a valuable stimulus to further studies in this highly complex and fertile field.

SHAO-CHUAN LENG

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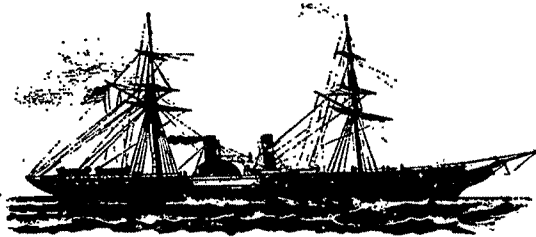
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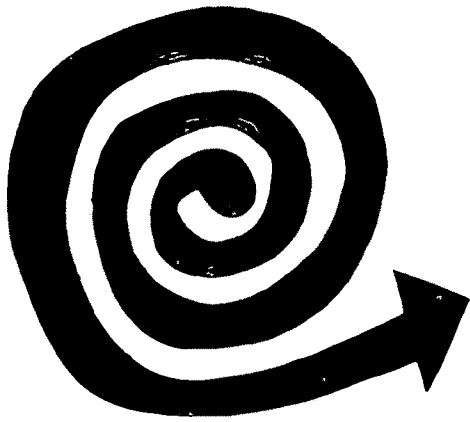
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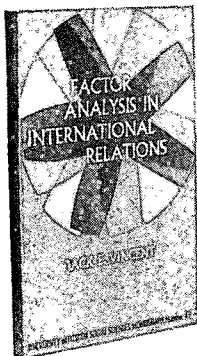
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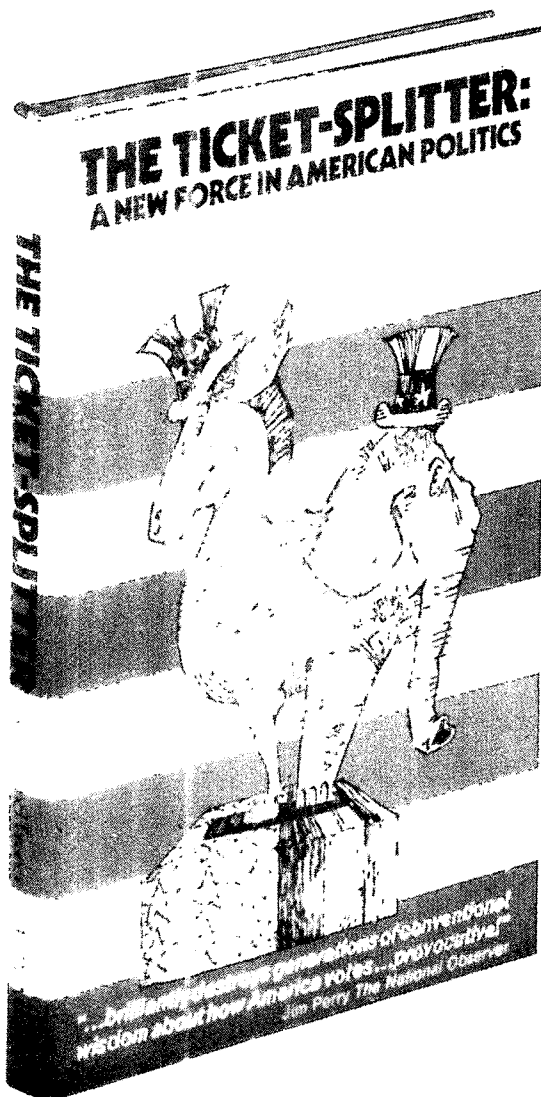
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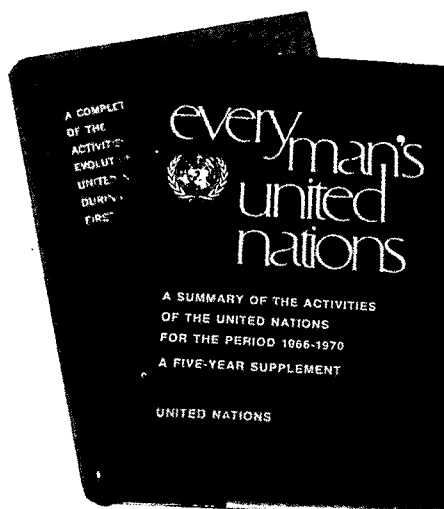
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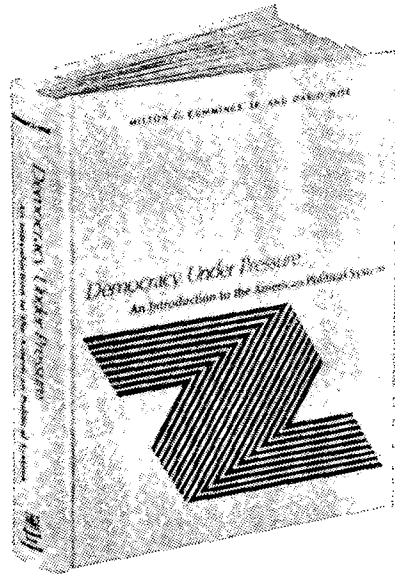
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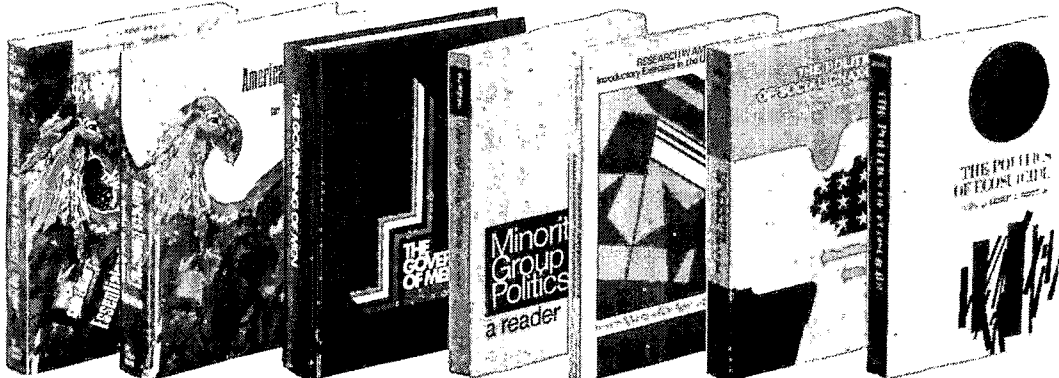
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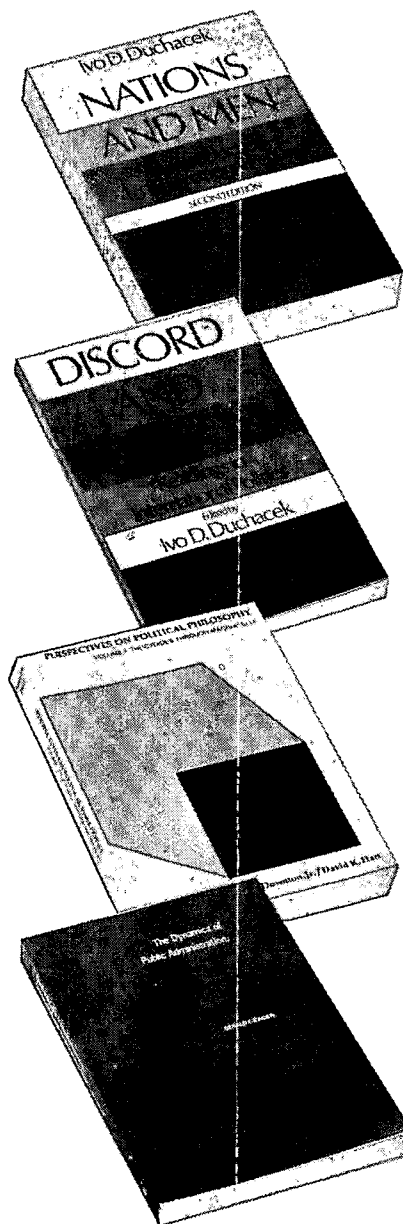
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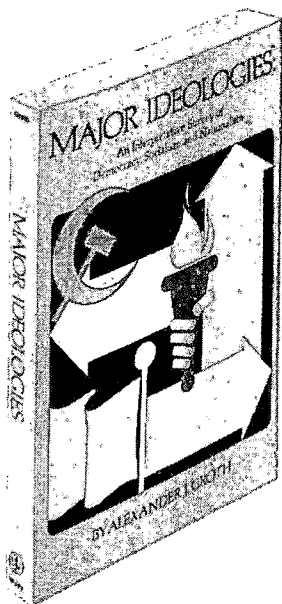
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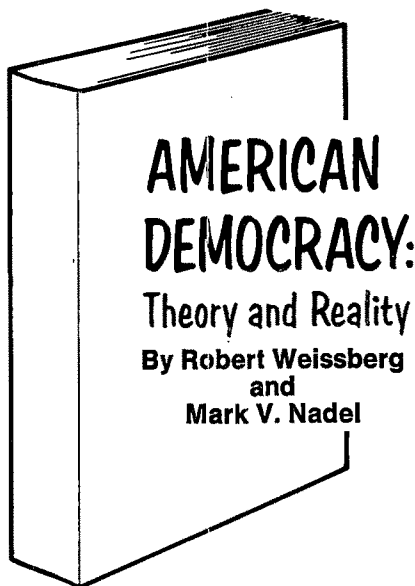
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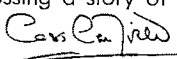
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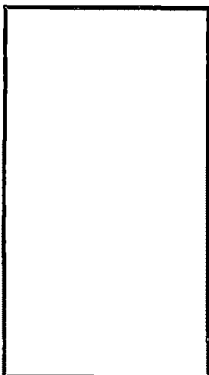
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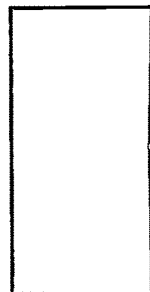
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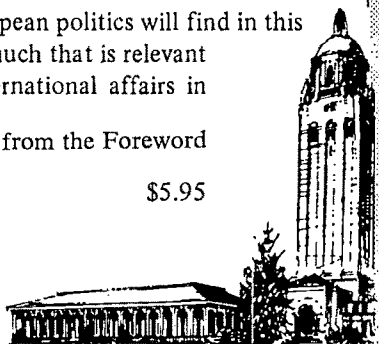
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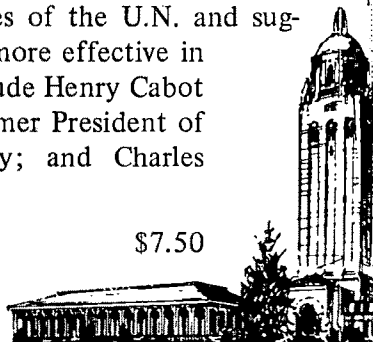
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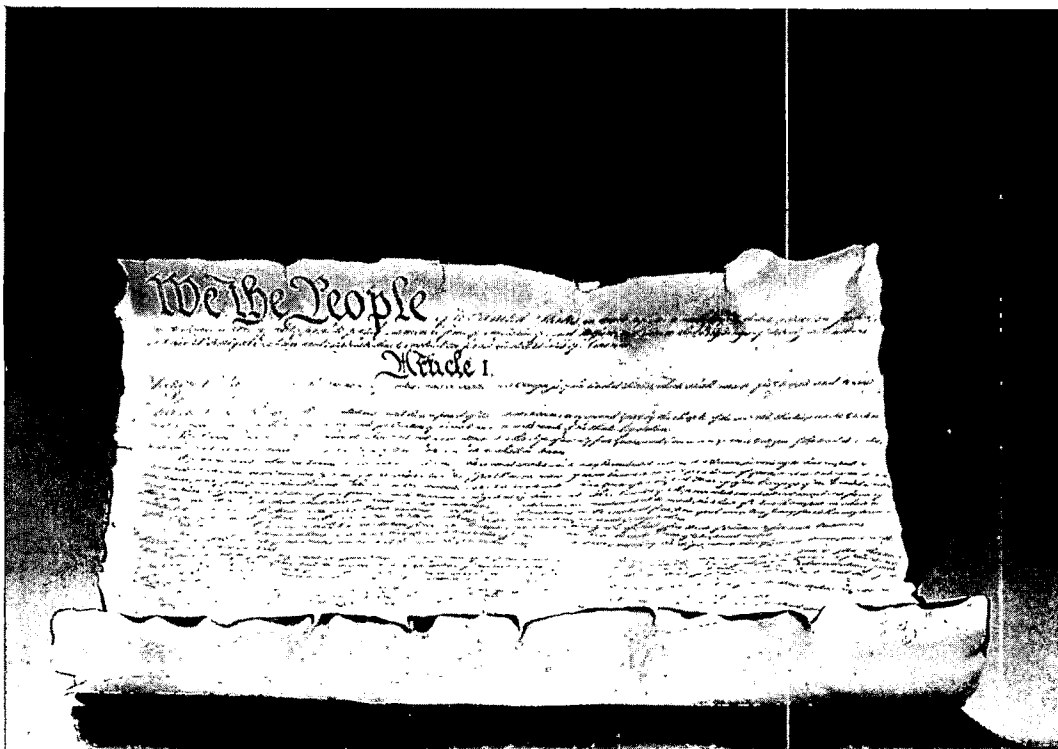
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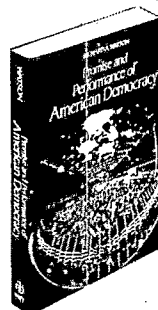
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INDEX TO ADVERTISERS

ABC Clio	357
Addison-Wesley Publishing	395
Aldine-Atherton Publishing	400, 401
Allyn & Bacon Inc.	350, 351
American Elsevier Publishing	375
Arno Press	348
Basic Books	342, 343
Brookings Institution	304, 305
Univ. of California Press	362, 363
Cambridge University Press	388
Columbia University Press	313, 379
Consensus Publishing	312
Cornell University Press	392, 393
Dodd, Mead & Co.	338
Dorsey Press	327
William B. Eerdmans Publishing	367
European Community	10
University of Florida Press	366
Free Press	330, 331, 332, 333, 334, 335
James E. Freel Associates	397
General Learning Corporation	384
Goodyear Publishing	344
Greenwood Publishing Co.	383
Harcourt, Brace, Jovanovich	372, 373, 374
Harper & Row	Cover 2, 322, 386, 387
Harper & Row Paperbacks	396
Harvard University Press	302, 339, 356
D. C. Heath & Co.	307, 337
Holbrook Press	345
Holt, Rinehart & Winston	376, 377, 378
Hoover Institution Press	390, 391
Houghton Mifflin	315
University of Illinois Press	370
Johns Hopkins Press	394
J. B. Lippincott	358
Little, Brown & Co.	318, 319, 320, 321
Markham Publishing	Cover 4, 303
McCutchan Publishing Corp.	389
McGraw-Hill Book Co.	360, 361
David McKay	314
Macmillan Co.	323, 324, 325
Charles E. Merrill Publishing	339
MIT Press	371
Northwestern University Press	402
Ohio State University Press	326
Oxford University Press	353, 354, 355
F. E. Peacock Publishers	352
Penguin Books	336
Politics	349
Praeger Publishers	306, 329
Princeton Information Technology	340
Princeton University Press	380, 381
Prentice-Hall	328, 341, 346
Rand McNally	368, 369
Random House	310, 311
Rutgers University Press	346
St. Martin's Press	308, 309
Scott, Foresman & Co.	385
Stanford University Press	399
State University of New York Press	365
Teachers College Press	364
TIAA	404
U. N. Publications	371
Van Nostrand Reinhold	359
University Press of Virginia	396
John Wiley & Sons	316, 317, 347, 382, 398
World Law Fund	Cover 3
Yale University Press	328

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CONTENTS

- 415 From Confusion to Clarity: Issues and American Voters, 1956-1968
Gerald M. Pomper
- 429 Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election
Richard W. Boyd
- 450 Comment Richard A. Brody and Benjamin I. Page
- 459 Comment John H. Kessel
- 466 Rejoinder Gerald M. Pomper
- 468 Rejoinder Richard W. Boyd
- 471 A Theory of Neighborhood Problem Solving: Political Action vs. Residential
Mobility John M. Orbell and Toru Uno
- 490 Some Conceptual Problems in Nuclear Proliferation George H. Quester
- 498 On the Concept of Ideology in Political Science Willard A. Mullins
- 511 Preconditions of Mayoral Leadership Jeffrey L. Pressman
- 525 Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the
Imbalance Thesis Lee Sigelman
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William D. Coplin and J. Martin Rochester
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ARTICLES

- 415 **From Confusion to Clarity: Issues and American Voters, 1956-1968.** Analysis of national election surveys from 1956 to 1968 reveals significant changes in the voters' perceptions of issues and the major parties. There has been a considerable increase in the correlation of party identification and opinion on six major issues, relating to social welfare, racial integration, and foreign aid. Voters are more prone to see a difference between the parties on these issues and are increasingly likely to identify the Democratic party as favorable to federal governmental action. These findings contrast with those of *The American Voter* and similar studies. The reasons for the changes cannot be found in demographic factors, as tested by controls for age cohorts, education, region, and race. More probably the explanation lies in strictly political factors. A particularly important event was the presidential campaign of 1964, in which ideological differences between the parties were deliberately emphasized. The electorate responded to this campaign by becoming more ideologically aware, and its learning appears to have persisted through the 1968 election. This finding suggests that past conclusions about the low ideological awareness of the electorate were specific to the Eisenhower era, and that the issue content of the vote will vary with the stimuli provided by the general political environment.

By GERALD M. POMPER, Professor of Political Science, Livingston College, Rutgers University.

- 429 **Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election.** The importance of issues in deciding elections changes from one election to the next. As Key has shown, the issues of the role of the federal government in social life helped create the New Deal Democratic majority. In contrast, issues had only a marginal impact on the apolitical elections of the 1950s. Converse's technique of normal vote analysis reveals that issues were again highly related to the vote in 1968. This was particularly true of attitudes toward Vietnam, urban unrest and race, social welfare, and Johnson's performance as president.

Yet, even in an election in which issues appear important, some can have very different consequences for popular control of policy than others. On some issues, the electorate exercises no effective constraints on leaders' policy choices. On others (e.g., the escalation in Vietnam), the electorate permits leaders a wide array of options when a policy is adopted and passes a retrospective judgment on such choices in subsequent elections. Finally, on still other issues, the public may limit the options of leaders at the time a policy is adopted. The paper suggests the stringent conditions necessary for this type of popular control to exist.

By RICHARD W. BOYD, Assistant Professor of Government, Wesleyan University.

Comment. By RICHARD A. BRODY, Professor of Political Science, Stanford University and BENJAMIN I. PAGE, Assistant Professor of Government, Dartmouth College.

Comment. By JOHN H. KESSEL, Professor of Political Science, Ohio State University.

Rejoinder. By GERALD M. POMPER.

Rejoinder. By RICHARD W. BOYD.

- 471 **A Theory of Neighborhood Problem Solving: Political Action vs. Residential Mobility.** People have three ways of responding to neighborhood problems: leaving (exit), political action (voice), and doing nothing (passivity). The model assumes: 1. Voice is more likely to ameliorate neighborhood problems than exit or passivity; exit, in fact, can make things worse, 2. Rational behavior on the part of residents, coupled with constraints that limit options: status, race, the responsiveness of government and the nature of the problems. Survey data on one city are combined with census data differentiating neighborhood types. Voice is characteristic of suburban areas among high and low status whites; exit is characteristic of white urban areas. Among ghetto blacks—whose exit options are severely constrained—voice is most characteristic. Problems faced by blacks and whites living in the city are similar, while their adaptations are different.

By JOHN M. ORBELL, Associate Professor of Political Science, University of Oregon and TORU UNO, Assistant Professor of Political Science, San Francisco State College.

- 490 **Some Conceptual Problems in Nuclear Proliferation.** While significant progress has been made in establishing a legal barrier to the further spread of nuclear weapons, some important nations

are withholding approval of this Non-Proliferation Treaty (NPT). Political factors are very significant, but agreement is made even more difficult by persistent ambiguities in technological forecasts of "how far we or they are from the bomb." The time-lag for any crash weapon programs will not remain as extended as one might hope, because civilian technology itself is drawing states ever closer to *de facto* military capabilities. Yet the length of this time-lag may be crucial to the maintenance of peace if political crises emerge. Since scientists offer widely varying estimates on such time-lags, a graphical formulation is offered to reduce ambiguity. In the end, the effectiveness of International Atomic Energy Agency (IAEA) safeguards under the NPT may depend more on their symbolic and political impact than on their technology.

By GEORGE H. QUESTER, Associate Professor of Government, Cornell University.

- 498 **On the Concept of Ideology in Political Science.** Although the term "ideology" is ubiquitous in modern political discourse, it is used in diverse and usually ambiguous ways which limit its value as an analytical concept. The main ambiguity arises from the fact that, as most writers use it, the concept of ideology does not provide criteria for distinguishing ideological thought from nonideological thought. Lacking this power to make concrete discriminations, the concept fails to achieve empirical relevance. This paper attempts to remedy that deficiency and save the concept of ideology for the explanation of politics.

The problem of conceptualization is approached by viewing ideology primarily as a cultural phenomenon. As such, it is argued, ideology has characteristics that distinguish it from other symbol systems. Of special importance in this regard is the identification of basic differentia between ideology on the one hand, and myth and utopia (with which ideology is often confused) on the other. The features of ideology identified in this comparative analysis are then discussed in fuller detail with a view to understanding (1) the significance of ideology in politics, and (2) the way in which the concept of ideology can help us to understand politics, insofar as politics involves ideology.

By WILLARD A. MULLINS, Associate Professor of Political Science, Carleton University.

- 511 **Preconditions of Mayoral Leadership.** Observers of city politics have often stated that political leadership—usually mayoral leadership—is a crucial ingredient in a city's ability to deal with its problems. And studies of successful leaders in urban systems have led to the formulation of a model of the "political entrepreneur" who is adept at accumulating political resources and pyramiding them to gain increased influence. Based on four years of observation of politics in Oakland, California, this study suggests that successful mayoral leadership and resource-pyramiding may be limited by governmental structure, the personality of the mayor, and the nature of the political system.

In Oakland, fragmented institutional authority and the council-manager form of government have created obstacles for elected officials. A privately oriented, "nonpolitical" mayor has avoided publicity and has tended to underutilize, rather than pyramid, his resources. Finally, in an amorphous political system characterized by a lack of group and party activity, a mayor is denied both information and support. By examining the ways in which the performance of Oakland's mayor diverges from a model of political leadership, we can identify some of the factors that support or limit such leadership.

By JEFFREY L. PRESSMAN, Instructor in Government, Dartmouth College.

- 525 **Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the Imbalance Thesis.** This research note analyzes a key premise of the thesis of "institutional imbalance" in less developed nations, the idea that administrative modernity promotes overparticipation in the performance of political and governmental functions. Judgmental data on fifty-seven Latin American, Asian, and African nations suggest that level of administrative development is highly correlated with overparticipation, but in precisely the opposite direction from that predicted in the literature—that is, the relationship is negative. This finding indicates the necessity of reconsidering some of the conventional wisdom of comparative administration.

By LEE SIGELMAN, Instructor in Political Science, Vanderbilt University.

- 529 **The Permanent Court of International Justice, the International Court of Justice, the League of Nations and the United Nations: A Comparative Empirical Survey.** After a brief discussion of the existing literature and a description of the data set, the paper compares the role of four international institutions in two-party disputes in terms of (1) participant behavior and characteristics,

(2) relationships between participants, and (3) characteristics of the disputes and institutional responses. The major purpose of the comparison is to assess various middle-range theoretical ideas that have been suggested by scholars and to determine whether the differences on the variables are a function of structural (PCIJ and ICJ versus League and UN) or historical (PCIJ and League versus ICJ and UN) variations. A final section of the paper presents a scheme for further research on the role of international institutions in the international bargaining process.

By WILLIAM D. COPLIN, Director, International Relations Program and Associate Professor of Political Science and J. MARTIN ROCHESTER, Research Associate, International Relations Program, Syracuse University.

- 551 **The Swing Ratio and Game Theory.** We propose a simple game-theory model of single-member plurality electoral systems, two parties with unequal resources being the players. Strategies consist of allocations of resources among the n contests, and a party's payoff is the number of contests to which it has assigned more resources than the other party. Mixed strategies exist which are asymptotically optimal as n increases. Identifying a party's proportion of total resources with its total vote proportion, we predict that the swing ratio, or marginal seat proportion per vote proportion, is 2. This compares to empirical findings which range between 2 and 4, and to the hitherto unexplained cube law, which predicts 3. We suggest that the strategic problem modeled by this game accounts for the major part of the swing ratio effect. Factors which vary from system to system, such as proportion of hard-core support attached to parties, may amplify this effect.

By DAVID SANKOFF, Visiting Member at the Centre de recherches mathématiques, Université de Montréal and KOULA MELLOS, Lecturer, Département de science politique, Université d'Ottawa.

- 555 **The Strategy of Ambiguity: Uncertainty and Electoral Competition.** In this paper problems of social choice in general, and political choice in particular, are considered in light of uncertainty. The space of social alternatives in this formulation includes not only pure social states, but lotteries or probability distributions over those states as well. In the context of candidate strategy selection in a spatial model of political choice, candidate strategy sets are represented by pure strategies—points in the space of alternatives—and ambiguous strategies—lotteries over those points. Questions about optimal strategy choice and the equilibrium properties of these choices are then entertained. Duncan Black's theorem about the dominance of the median preference is generalized, and further contingencies in which the theorem is false are specified. The substantive foci of these results are: (1) the conditions in which seekers of political office will rationally choose to appear equivocal in their policy intentions; and (2) the role of institutional structure in defining equilibrium.

By KENNETH A. SHEPSLE, Assistant Professor of Political Science, Washington University, St. Louis.

- 569 **Cluster-bloc Analysis and Statistical Inference.** Cluster-bloc analysis is a useful method of examining the voting records of a legislature, in order to find what subgroups of members regularly vote together. Agreement scores are calculated for every legislator with every other legislator. Then when a group is found to have all its members in high agreement with each other they are referred to as a cluster bloc. These groups, which are discovered empirically, are not necessarily the same as the formal caucus groups. So far each researcher has had to use his own judgment as to what constitutes "high agreement," but it can be shown that the cutoff points can be established statistically, against the null hypothesis of random voting. Since each score can be tested for significance, it is possible to use statistically based indices of cohesion for the legislature or any specified subgroup and indices of adhesion between the various subgroups. Examples are given for the African group in the UN General Assembly.

By PETER WILLETTTS, Research Associate, Department of Political Science and Public Administration, Makerere University, Kampala.

583 COMMUNICATIONS

From Robert S. Erikson, William Crotty, Hermann Garbers and Bruno S. Frey.

585 EDITORIAL COMMENT

588 BOOK REVIEWS

From Gold to Garbage: A Bibliographical Essay on Politics and the Environment Walt Anderson, ed., *Politics and Environment: A Reader in Ecological Crisis*; Lynton K. Caldwell, *Environment: A Challenge for*

Modern Society; Congressional Quarterly, Inc., *Man's Control of the Environment*; Council of Environmental Quality, *Environmental Quality: The Second Annual Report*; J. H. Dales, *Pollution, Property & Prices*; J. Clarence Davies III, *The Politics of Pollution*; Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal, *Environmental Control: Priorities, Policies, and the Law*; Harold W. Helfrich, Jr., ed., *Agenda for Survival*; Huey D. Johnson, ed., *No Deposit-No Return: Man and His Environment*; Editors of Ramparts, *Eco-Catastrophe*; Roger Revelle and Hans H. Landsberg, eds., *America's Changing Environment*; Leslie L. Roos, Jr., ed. *The Politics of Ecosuicide*; Joseph L. Sax, *Defending the Environment: A Strategy for Citizen Action*; Richard Saltonstall, Jr., *Your Environment and What You Can Do About It: A Citizen's Guide*.

By CHARLES O. JONES, Maurice Falk Professor of Politics, University of Pittsburgh.

596 Political Theory, History of Political Thought and Methodology

- Adler, Mortimer J., *The Common Sense of Politics*. George Kateb, p. 596
 Banks, J. A. *Marxist Sociology in Action: A Sociological Critique of the Marxist Approach to Industrial Relations*; and Zwerman, William A., *New Perspectives on Organization Theory: An Empirical Reconsideration of the Marxian and Classical Analyses*. Maria Hirszowicz, p. 612
 Boulding, Kenneth E., *A Primer on Social Dynamics: History as Dialectics and Development*. Robert Nisbet, p. 597
 Held, Virginia, *The Public Interest and Individual Interests*. Glendon Schubert, p. 599
 Howe, Irving, ed., *Essential Works of Socialism*. Kurt L. Shell, p. 602
 Janowitz, Morris, *Political Conflict: Essays in Political Sociology*. Lewis A. Coser, p. 603
 Kelleher, Grace J., ed., *The Challenge to Systems Analysis: Public Policy and Social Change*. Walter Buckley, p. 604
 Lachmann, L. M., *The Legacy of Max Weber*. Don Martindale, p. 605
 Marx, Karl and Engels, Friedrich, *Writings on the Paris Commune* (Hal Draper, ed.); and Marx, Karl and Engels, Friedrich, *Birth of the Communist Manifesto* (Dirk J. Struik, ed.). Alfred G. Meyer, p. 606
 Powell, Elwin H., *The Design of Discord*. Paul A. Dawson, p. 608
 Sjöblom, Gunnar, *Party Strategies in a Multiparty System*. Deane E. Neubauer, p. 609
 Telford, Shirley, *Economic and Political Peace*. Jan Pen, p. 610
 Vachet, André, *L'Idéologie Libérale*. Frederic S. Burin, p. 610
 Zwerman, William A., *New Perspectives on Organization Theory: An Empirical Reconsideration of the Marxian and Classical Analyses*; and Banks, J. A., *Marxist Sociology in Action: A Sociological Critique of the Marxist Approach to Industrial Relations*. Maria Hirszowicz, p. 612

613 American Government and Politics

- Anderson, Stanley V., *Ombudsman Papers: American Experience and Proposals*. Henry J. Abraham, p. 613
 Green, Philip and Levinson, Sanford, eds., *Power and Community: Dissenting Essays in Political Science*. Norton E. Long, p. 618
 Hertzberg, Hazel W., *The Search for an American Indian Identity: Modern Pan-Indian Movements*; and Sorkin, Alan L., *American Indians and Federal Aid*. Wilcomb E. Washburn, p. 614
 Huit, Ralph K. and Peabody, Robert L., *Congress: Two Decades of Analysis*. Alan Rosenthal, p. 616
 Kendall, Willmoore and Carey, George W., *The Basic Symbols of the American Political Tradition*. John A. Wettergreen, p. 617
 Levinson, Sanford and Green, Philip, eds., *Power and Community: Dissenting Essays in Political Science*. Norton E. Long, p. 618
 Marini, Frank, ed., *Toward a New Public Administration: The Minnowbrook Perspective*. Victor A. Thompson, p. 620
 Nimmo, Dan, *The Political Persuaders: The Techniques of Modern Election Campaigns*. Bernard Hennessy, p. 622
 O'Leary, Michael Kent, *The Politics of American Foreign Aid*. Clarence E. Thurber, p. 623
 Pierce, Lawrence C., *The Politics of Fiscal Policy Formation*. Aaron Wildavsky, p. 624
 Sorkin, Alan L., *American Indians and Federal Aid*; and Hertzberg, Hazel W., *The Search for an American Indian Identity: Modern Pan-Indian Movements*. Wilcomb E. Washburn, p. 614

626 Comparative Government and Politics

- Baring, Arnulf, *Aussenpolitik in Adenauer's Kanzlerdemokratie*. Paul J. Cassidy, p. 626
 Bon, Frédéric and Michelat, Guy, *Attitudes et comportements politiques à Boulogne-Billancourt: enquête par panel 1965-1967*. Wesley D. Camp, p. 627
 Burlatskii, F. M., *Lenin, Gosudarstvo, Politika*. David E. Powell, p. 628
 Brüning, Heinrich, *Memoiren 1918-1934*. John Brown Mason, p. 629
 Butler, David and Pinto-Duschinsky, Michael, *The British General Election of 1970*. John E. Turner, p. 630
 Caplan, Gerald L., *The Elites of Barotseland 1878-1969: A Political History of Zambia's Western Province*. William Tordoff, p. 632

- DeGregori, Thomas R., *Technology and the Economic Development of the Tropical African Frontier*; and Goody, Jack, *Technology, Tradition, and the State in Africa*. Ali A. Mazrui, p. 633
- de Kadt, Emanuel, *Catholic Radicals in Brazil*. Gilbert B. Siegel, p. 635
- Devlin, Bernadette, *The Price of My Soul*; O'Neill, Terence, *Ulster at the Crossroads*; and Jackson, T. A., *Ireland Her Own*. Warner Moss, p. 636
- Di Palma, Giuseppe, *Apathy and Participation: Mass Politics in Western Societies*. Joseph LaPalombara, p. 637
- Doob, Leonard W., ed., *Resolving Conflict in Africa: The Fermeda Workshop*. Margaret G. Hermann, p. 638
- Fox, Edward W., *History in Geographic Perspective: The Other France*. Arthur L. Stinchcombe, p. 640
- Gann, Lewis, *Guerrillas in History*. J. Bowyer Bell, p. 642
- Gombin, Richard, *Les socialistes et la guerre: La S.F.I.O. et la politique étrangère française entre les deux guerres mondiales*. Orville D. Menard, p. 642
- Goody, Jack, *Technology, Tradition, and the State in Africa*; and DeGregori, Thomas R., *Technology and the Economic Development of the Tropical African Frontier*. Ali A. Mazrui, p. 633
- Gott, Richard, *Guerrilla Movements in Latin America*. Susan Kaufman Purcell, p. 644
- Gregorian, Vartan, *The Emergence of Modern Afghanistan: Politics of Reform and Modernization*. Louis Dupree, p. 645
- Gulliver, P. H., *Neighbors and Networks: The Idiom of Kinship in Social Action Among the Ndendeuli of Tanzania*. Peter Rigby, p. 646
- Hermet, Guy, *Les Communistes en Espagne*. Gabriel Jackson, p. 647
- Jackson, T. A., *Ireland Her Own*; O'Neill, Terence, *Ulster at the Crossroads*; and Devlin, Bernadette, *The Price of My Soul*. Warner Moss, p. 636
- Jacobs, Jane, *The Economy of Cities*; and Osborn, Frederic J., *Green-Belt Cities*. Charles R. Adrian, p. 648
- Kanowsky, Eliyahu, *The Economic Impact of the Six-Day War: Israel, the Occupied Territories, Egypt and Jordan*. Efraim Torgovnik, p. 650
- Kumao, Baron Harada, *Fragile Victory: Prince Saionji and the 1930 London Treaty Issue from the Memoirs of Baron Harada Kumao* (trans. by Thomas Francis Mayer-Oakes). Nobutaka Ike, p. 651
- Lévesque, Jacques, *Le Conflit sino-soviétique et l'Europe de l'Est: ses incidences sur les conflits soviéto-polonais et soviéto-roumain*. Ladis K. D. Kristof, p. 652
- Liebenow, Gus J., *Colonial Rule and Political Development in Tanzania*. P. H. Gulliver, p. 653
- Mendel, Douglas, *The Politics of Formosan Nationalism*. Richard Hsiuh Yang, p. 654
- Merritt, Anna J. and Merritt, Richard L., eds., *Public Opinion in Occupied Germany*. Kendall L. Baker, p. 656
- Moreno, José, *Barrios in Arms: Revolution in Santo Domingo*. Howard J. Wiarda, p. 657
- Morin, Edgar, *The Red and the White: Report from a French Village*. Sidney Tarrow, p. 658
- Nkrumah, Kwame, *Africa Must Unite*. Roberta E. Mapp, p. 660
- Nunn, Frederick M., *Chilean Politics, 1920-1931: The Honorable Mission of the Armed Forces*. Robert L. Ayres, p. 661
- O'Neill, Terence, *Ulster at the Crossroads*; Devlin, Bernadette, *The Price of My Soul*; and Jackson, T. A., *Ireland Her Own*. Warner Moss, p. 636
- Osborn, Frederic J., *Green-Belt Cities*; and Jacobs, Jane, *The Economy of Cities*. Charles R. Adrian, p. 648
- Pearson, Lester B., *Words and Occasions*. Zachariah Kay, p. 662
- Pelt, Adrian, *Libyan Independence and the United Nations: A Case of Planned Decolonization*. Yassin El-Ayouty, p. 663
- Pennar, Jaan, Bakalo, Ivan I., and Bereday, George A. F., *Modernization and Diversity in Soviet Education*. Joel J. Schwartz, p. 665
- Scott, Roger, ed., *The Politics of New States: A General Analysis with Case Studies from Eastern Asia*. Michael Leifer, p. 665
- Turner, Victor, ed., *Colonialism in Africa 1870-1960. Volume 3: Profiles of Change: African Society and Colonial Rule*. William J. Foltz, p. 666
- von Albertini, Rudolf, *Decolonization, The Administration and Future of the Colonies, 1919-1960*. Rupert Emerson, p. 667
- Wheelwright, E. L. and McFarlane, Bruce, *The Chinese Road to Socialism*. Chu-yuan Cheng, p. 669

670 International Politics, Law, and Organization

- Ball, M. Margaret, *The OAS in Transition*. Ann Van Wynen Thomas, p. 670
- Campbell, John Franklin, *The Foreign Affairs Fudge Factory*. I. M. Destler, p. 671
- Deener, David R. ed., *De Lege Pactorum. Essays in Honor of Robert Renbert Wilson*. Peter H. Rohn, p. 672
- Freedman, Robert Owen, *Economic Warfare in the Communist Bloc: A Study of Soviet Economic Pressure Against Yugoslavia, Albania, and Communist China*. Dennison Rusinow, p. 673
- Friedman, Wolfgang, *The Future of the Oceans*. Stanley S. Jados, p. 675
- Gordenker, Leon, ed., *The United Nations in International Politics*. William J. Bruce, p. 676
- Hanrieder, Wolfram F., *The Stable Crises: Two Decades of German Foreign Policy*. Ernst Nolte, p. 677
- Johnson, E. A. J., *American Imperialism in the Image of Peer Gynt, Memoirs of a Professor-Bureaucrat*. John D. Montgomery, p. 679

- Lindberg, Leon N. and Scheingold, Stuart A., *Europe's Would-Be Polity: Patterns of Change in the European Community*. Robert McGeehan, p. 681
- Maxwell, Neville, *India's China War*. Charles P. Schleicher, p. 682
- Reisman, Michael, *The Art of the Possible: Diplomatic Alternatives in the Middle East*. Saadia Touval, p. 684
- Reisman, W. Michael, *Nullity and Revision: The Review and Enforcement of International Judgments and Awards*. Charles H. McLaughlin, p. 685
- Rosenne, Shabtai, *The Law of Treaties, A Guide to the Legislative History of the Vienna Convention*. P. E. Corbett, p. 687
- Tuchman, Barbara W., *Stilwell and the American Experience in China, 1911-45*. Tang Tsou, p. 687
- Watt, D. C., *Survey of International Affairs 1962*. Adda B. Bozeman, p. 690
- Weston, Burns H., *International Claims: Postwar French Practice*. Victor G. Rosenblum, p. 691
- Zartman, I. William, *The Politics of Trade Negotiations between Africa and the European Economic Community: The Weak Confront the Strong*. Vernon McKay, p. 692

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Students of politics, from Plato to Marcuse, have frequently sneered at the inability of the "masses" to discern political reality. Whether citizens are misled by the shadows on the wall of the cave or by the shadows of the television tube, they are deemed to be fundamentally unequipped consciously to control political elites and rationally to direct public policy.

In contemporary times, such conclusions have been unintentionally strengthened by selected and often distorted findings of empirical voting research. In broad terms, political scientists have found voters to have limited interest in politics, to be strongly attached to their traditional parties and social groups, and to lack ideological coherence in their views of political issues.¹ Of particular concern here is the electorate's perception of issues, of coherent ideologies, and of the links between issue preferences and partisan preferences. Much voting research has indicated that these perceptions are cloudy. Large proportions of voters have "no opinion" or "don't know" their opinion on specific policy issues.² Only 12 per cent of the citizenry has been found to hold an ideological

view of the parties.³ The links between issue preferences and party choice are weak. Party identification was found in *The American Voter* to have little relation to general ideology,⁴ and McClosky, dealing with the same period, found "that substantial differences of opinion exist among the electorate on only five of the 24 issues" he examined.⁵ The Michigan volume indicated that there was little belief among the electorate that the parties differed on particular issues, and little agreement on the direction of whatever differences were perceived.⁶ Although the findings were carefully qualified, their general thrust justified the conclusion: "The electoral decision gives great freedom to those who must frame the policies of government. If the election returns offer little guidance on specific policies, neither do they generate pressures that restrict the scope of President and Congress in developing public policy."⁷

Reaction to these conclusions has varied. Some publicists or would-be "hidden persuaders" have selectively drawn from the voting studies to compose a picture of an irrational voter, easily manipulated by possessors of this arcane knowledge. Thus, a widely quoted book on "the New Politics" is taken seriously when it warns, "These new managers . . . can play upon the voters like virtuosos. They can push a pedal here, strike a chord there, and presumably, they can get precisely the response they seek."⁸ To Bernard Berelson, the voting findings offered the occasion to develop a new theory of democracy, in which stability depended on the limited involvement of the population.⁹

V. O. Key saw the danger that the findings of voting research could be misused to convert popular democracy into a system of elitist manipulation, in which, "fed a steady diet of bur-combe, the people may come to expect and to

* Mr. Thomas O'Donnell deserves many thanks for his work in arranging and computing the data on which this paper is based. I also appreciate the many suggestions of my colleagues at Rutgers University particularly Benjamin Barber, W. Carey McWilliams, Stephen Salmore, and Gordon Schochet, and the comments of John Kessel and Rick Piltz.

¹ The most important past works are, chronologically: Paul Lazarsfeld, Bernard Berelson, and Helen Gaudet, *The People's Choice*, 2nd ed. (New York: Columbia University Press, 1948); Bernard Berelson, Paul Lazarsfeld and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Angus Campbell, Gerald Gurin and Warren Miller, *The Voter Decides* (Evanston: Row, Peterson, 1954); Eugene Burdick and Arthur Brodbeck, eds., *American Voting Behavior* (New York: The Free Press, 1959); Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: Wiley, 1960); Ithiel de Sola Pool, Robert Abelson and Samuel Popkin, *Candidates, Issues and Strategies* (Cambridge: MIT Press, 1964); Philip Converse, "The Nature of Belief Systems in Mass Publics," in David Apter, ed. *Ideology and Discontent* (New York: The Free Press, 1964); and Campbell, Converse, Miller and Stokes, *Elections and the Political Order* (New York: Wiley, 1966). Important recent works will be cited below.

² V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1961), Chap. 4; Campbell et al., *The American Voter*, p. 174.

³ Campbell et al., *The American Voter*, p. 249.

⁴ Campbell et al., *The American Voter*, chap. 9.

⁵ Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), p. 419. Among party leaders, by contrast, differences existed on 23 of 24 issues.

⁶ Campbell et al., *The American Voter*, pp. 182–184.

⁷ Campbell et al., *The American Voter*, p. 544.

⁸ James M. Perry, *The New Politics* (New York: Clarkson Potter, 1968), p. 213.

⁹ Berelson, Chap. 14.

respond with highest predictability to buncombe."¹⁰ In his last work, he sought to revive the notion that issue and voting preferences are closely related. While he did demonstrate a certain consistency in the electorate's choices, he could not prove a causal connection. Key's work, along with Arthur Goldberg's,¹¹ however, did bring a renewed emphasis on the analysis of policy preferences to electoral studies.

In the past few years, this renewed emphasis has been evident in a number of independent reappraisals of *The American Voter's* finding of ideological unawareness among the electorate. One set of authors argues that mass ideological awareness has always been present to a greater degree than found by Campbell, Converse, Miller and Stokes, but that appropriate methods have not been employed to observe this awareness. Thus, focusing on local concerns, Luttbeg found considerable "constraint" or coherence, in mass attitudes.¹² Similarly, using a series of prepared statements, Brown found no difference in ideological awareness between political articulates and inarticulates.¹³ Most notable is the work of Lane, who was able to discern a developed ideology among New Haven workers through lengthy interviews.¹⁴ These strands suggest that previous studies demonstrated not the absence of ideology, but the absence of the ability to articulate hidden ideology.¹⁵

Another criticism is that the findings of *The American Voter* are time-bound. They may show a low degree of ideology among voters in 1956, but only because the 1956 election did

not stimulate ideological feelings. Replicating the Michigan study for the contrasting 1964 election, in which ideology was emphasized, Field and Anderson found a substantial increase in ideological awareness. In the Goldwater-Johnson context, a third of the respondents are classified as ideological, more than double the proportion of 1956 voters, and nearly three times the proportion of the total sample in the earlier study.¹⁶

The recent work of Pierce combines these two revisions. Using three measures of ideological awareness derived from Survey Research Center materials, instead of only one, Pierce also tested the change in awareness over time. A greater proportion of the sample was classified as ideological under the three-pronged analysis, and the proportion was found to have increased considerably from 1956 through 1960 to 1964.¹⁷ These recent findings suggest an increased awareness of politics among American voters. The analysis below provides further evidence to support this conclusion.

Changing Perceptions of Parties and Policies

The research presented here does not deal with general ideology, but with voter opinions and perceptions on the six precise policy questions which have been asked consistently of the national Survey Research Center sample in each quadrennial study since 1956. I will analyze the relationship between issue preferences and three partisan variables: party identification, the awareness of differences between the parties on these six issues, and the consensus among the electorate on the positions of the parties on these issues. In each of the three instances, we will find an increase in voter consciousness during the 1956-1968 period.

The six policy issues are: federal aid to education, government provision of medical care,

¹⁰ V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), p. 7.

¹¹ Arthur Goldberg, "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review*, 60 (December, 1966), 913-922; "Social Determinism and Rationality as Bases of Party Identification," *American Political Science Review*, 63 (March, 1969), 5-25.

¹² Norman Luttbeg, "The Structure of Beliefs among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 398-409.

¹³ Steven R. Brown, "Consistency and the Persistence of Ideology," *Public Opinion Quarterly*, 34 (Spring, 1970), 60-68.

¹⁴ Robert Lane, *Political Ideology* (New York: The Free Press, 1960).

¹⁵ See John Plamenatz, "Electoral Studies and Democratic Theory: I. A British View," *Political Studies*, 6 (February, 1958), 9.

A choice is reasonable, not because the chooser, when challenged, can give a satisfactory explanation of why he made it but because, if he could give an explanation, it would be satisfactory. The reasoning that lies behind the choice is often made in private language which the chooser never learns to translate into words intelligible to others because there is ordinarily no need for him to do so.

¹⁶ J. O. Field and R. E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly*, 33 (Fall, 1969), 380-398. While the coding in this research differs somewhat from that of *The American Voter*, the changes were essentially those made necessary by new procedures of the Survey Research Center.

¹⁷ John G. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970), 25-42. Confirming evidence is found in the recent work of David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389-400. Using responses to open-ended questions, RePass finds considerable mass concern for issues, an increase in issue awareness from 1960 to 1964, a close relationship between issue position and partisanship, and a significant partial correlation of .23 in 1964 between issue partisanship and vote, controlling for candidate image and party identification.

government guarantee of full employment, federal enforcement of fair employment and fair housing, federal enforcement of school integration, and foreign aid. Although the questions are not worded identically in the four surveys, they are sufficiently close to be highly comparable. Moreover, we are not concerned with the trend of opinion itself, but rather with comparisons of the structure of opinion in each of the election years, so that identical wording is not critical.¹⁸

In Table 1, positions on these issues are pre-

¹⁸ The differences in wording consisted largely of changes in form. In 1956 and 1960, the questions were asked as statements with which the respondent could agree or disagree (and also indicate the intensity of his opinion), e.g., "If cities and towns around the country need help to build more schools, the government in Washington ought to give them the money they need." In 1964 and in 1968, the respondent was offered a choice between two policies, each of which was advocated by "some people," such as—"the government in Washington should help towns and cities provide education," or "this should be handled by the states and local communities." There are two differences in wording of possible substance. In 1956 and 1960, the question dealing with full employment asks whether or not "the government in Washington ought to see to it that everybody who wants to work has a job and a good standard of living," while the later alternative does not include "who wants to work." In the earlier surveys, the question on racial equality asks whether, "If Negroes are not getting fair treatment in jobs and housing, the government should see to it that they do," while in later years the question

is more narrowly presented as dealing with jobs alone. I believe the basic thrust of these questions is not affected by these changes. The questions used are, by deck and column numbers, in 1956: 3/12, 3/18, 3/21, 3/24, 3/33, 3/54; in 1960: 4/59, 4/67, 4/55, 4/63, 4/72, 4/61; in 1964: 4/45, 4/56, 4/61, 5/11, 5/14, 4/67; in 1968: 4/54, 4/58, 4/60, 4/74, 4/76, 5/29. The surveys of 1948 and 1952 could not be used because questions were insufficiently comparable.

The same pattern is evident in the increasing spread between the extremes. Strong Demo-

Table 1. Party Identification and Policy Position, 1956, 1960, 1964 and 1968
(In Percentages Supporting "Liberal" Position)

Party Identification	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
Strong Democrat	80.0	66.8	51.0	53.6	74.2	74.5	78.2	81.3	75.6	71.2	52.6	53.1
Weak Democrat	78.1	59.0	44.1	38.3	67.3	60.2	65.2	72.1	64.0	62.4	38.4	39.7
Independent	71.0	53.2	39.3	32.9	55.8	56.7	57.2	55.3	55.0	56.6	31.0	27.0
Weak Republican	68.7	39.1	21.5	22.5	51.4	47.5	43.5	39.3	59.5	43.9	25.9	24.9
Strong Republican	67.7	44.5	15.5	12.0	45.9	54.2	23.6	42.7	51.5	52.7	16.1	25.4
Gamma	.15	.20	.34	.36	.24	.18	.45	.41	.19	.16	.31	.25

Party Identification	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
Strong Democrat	73.3	63.0	56.3	61.9	38.7	39.8	53.7	58.9	49.5	51.4	64.7	51.3
Weak Democrat	71.3	63.1	42.9	43.5	44.4	37.5	43.2	44.6	55.4	48.8	59.2	45.8
Independent	66.6	65.4	50.3	37.7	48.8	47.1	49.0	37.3	49.9	53.2	57.5	42.7
Weak Republican	70.8	62.7	36.3	37.8	49.3	43.0	50.5	37.4	48.2	54.0	56.6	47.0
Strong Republican	66.8	65.9	20.6	31.3	38.8	41.5	34.8	31.5	51.4	61.5	49.7	41.8
Gamma	.04	-.02	.22	.24	.04	-.01	.08	.43	.01	-.03	.08	.00

Table 2. Perceptions of Parties on Policy Issues, by Party Identification

Group	Existence of Party Differences											
	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those with Opinions Perceiving Party Differences)</i>												
Strong Democrat	58.1	62.8	73.8	75.7	63.2	68.6	86.4	85.8	73.0	74.5	80.3	78.1
Weak Democrat	44.2	53.2	60.9	53.0	48.9	59.0	77.2	71.2	54.2	66.3	69.3	65.4
Independent	43.4	52.3	53.5	48.0	41.2	45.3	77.4	62.3	36.4	57.6	55.2	54.2
Weak Republican	48.0	41.8	58.9	53.3	50.0	68.6	75.3	65.8	50.3	50.5	61.3	64.1
Strong Republican	59.0	63.0	78.4	73.3	64.9	66.7	87.7	75.9	63.7	42.5	76.7	79.1
Total Sample	52	55	66	59	54	58	82	71	55	64	70	63

Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	<i>(Percentages of Those with Opinions Perceiving Party Differences)</i>											
Strong Democrat	50.0	52.5	77.5	78.5	55.6	50.6	70.0	75.9	46.5	44.7	61.5	64.7
Weak Democrat	46.9	38.7	70.1	64.9	55.6	34.9	63.7	62.5	40.4	37.5	52.7	43.8
Independent	38.5	38.7	58.8	55.0	42.3	30.8	59.0	51.1	33.6	32.7	50.8	39.3
Weak Republican	46.0	44.3	53.2	47.7	50.0	36.4	52.1	53.1	50.0	32.0	50.3	42.8
Strong Republican	52.3	50.0	75.0	60.5	57.5	39.2	66.2	60.5	63.9	47.4	61.8	48.0
Total Sample	47	44	69	62	51	36	64	60	49	39	56	46

crats and Strong Republicans were already distinct on the issue of medical care in 1956, but the difference almost doubled in 1964 and remained considerably widened in 1968. The same pattern is evident on the other issues as well. Party differences became quite high by 1964 and then decreased slightly (on job guarantees, fair employment, and foreign aid) or increased slightly (on aid to education and school integration) four years later.

The increased policy distinctiveness of partisans is summarized by the increases in the ordinal correlation (*gamma*) included in the last row of Table 1. Policy and party preferences became far more congruent in 1964, and the correlation was very similar in 1968. The only major exception is the issue of school integration. For the first three elections in this period, no meaningful correlation existed between party identification and support of federal action on the issue (*gammas* = .04, -.01, .08). But by 1968, there was a considerable relationship, and the *gamma* statistic (.43) is higher than in any other instance. On five of the six issues—all but foreign aid—party identification meant something by 1968 other than a traditional reaffirmation: it was now related to the policy preferences of the voter.

A second change is evident in the awareness of party differences on these six policy questions. In 1956, the Survey Research Center found little awareness of differences. It subjected its respondents to a series of questions on each policy question. It eliminated those who had no interest in, or opinion on, an issue, as well as those who, holding an opinion on the issue, could not decide if "the government is going too far, doing less than it should, or what?" This latter question typically eliminated more respondents than the query on the issue itself. Those who were left after the multiple screening were then asked whether there was a difference between the parties. This remaining sample was presumably a relatively knowledgeable group, but in 1956 even *they* found rather little distinction between the parties on most issues.¹⁹ In later surveys, the question on the government's program was eliminated. Therefore, the

¹⁹ The answers of 1956 respondents can be found in Table 8-3 of Campbell et al., *The American Voter*, p. 182. It might be argued that 1956 voters were far more issue-conscious than the data reveal, but that they were thinking about different issues than those raised by the parties or the survey. This argument seems hardly plausible, since it would require a degree of ideological originality for which there is no evidence among any mass public.

remaining sample asked to differentiate the parties was larger, and presumably less informed. Nevertheless, they perceived more of a difference between the parties.

This changing awareness is seen in Table 2. Over time, the parties have come to be seen as more different on questions of federal government power. Again, the issue of foreign aid is exceptional. The critical effect of the 1964 election is evident once more, as the proportions seeing party differences changed most dramatically in the Goldwater-Johnson contest. In 1968, there was a regression downward toward lessened perceptions of party splits, although it was not a full regression. Only on the distinct issue of foreign aid was the perception of party difference in 1968 at or below the level of the Eisenhower period. For the other issues, during the decade of the 1960s, a significant and apparently enduring political lesson was learned about the existence of party differences. The lesson was particularly well learned by the stronger partisans. On virtually all issues and in all elections, the strong partisans were more likely to see party differences than were weak partisans, who in turn were more likely to see differences than independents. These results

clearly accord with the concept and significance of party identification.

A more important finding is involved in the third change, relating to the ideological identity of the parties. In 1956, even among the sample remaining after various filterings, there was relatively little consensus on the position of the parties. The greatest consensus existed on another question dealing with ownership of electric power and housing. On this item, three-fourths of those who saw a party difference also agreed that the Republicans were more favorable to private ownership of these industries.²⁰ The identification of the parties' ideological positions was lower on the six issues we are considering. (The item on power and housing was not used in later surveys, so it cannot be analyzed here.)

By 1968, as seen in Table 3, each of the two parties seemed to have a much clearer identity. The proportion seeing the Democrats as the more liberal or activist party had risen on every question, even on foreign aid. It is particularly significant that in almost all cases the perceptions of the parties showed a consensus greater

²⁰ Campbell et al., *The American Voter*, p. 182.

Table 3. Consensus on Positions of Parties on Policy Issues, by Party Identification

Group	Consensus on Party Positions											
	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)</i>												
Strong Democrat	90.5	95.2	96.5	94.1	93.9	95.8	98.2	98.8	85.5	98.8	98.8	97.0
Weak Democrat	86.5	90.5	93.6	82.6	84.6	90.4	95.8	88.7	71.8	93.3	93.9	88.5
Independent	66.3	74.6	83.8	64.3	76.4	87.7	93.5	82.7	61.9	82.3	89.3	71.4
Weak Republican	37.2	60.9	55.8	58.7	64.3	52.2	77.6	85.0	60.0	67.3	72.5	69.4
Strong Republican	31.6	47.9	55.0	41.2	57.8	51.3	79.8	63.6	47.0	32.3	63.8	53.9
Total Sample	67	76	81	73	77	81	92	87	69	80	88	80
Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	<i>(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)</i>											
Strong Democrat	64.6	83.5	96.9	97.3	39.1	34.7	96.2	95.3	57.1	80.9	98.1	93.0
Weak Democrat	63.6	65.5	93.9	88.3	51.5	42.5	90.3	87.1	53.0	60.2	94.2	89.2
Independent	49.4	53.4	89.5	75.6	45.8	46.4	91.8	83.4	54.5	46.8	88.1	83.1
Weak Republican	23.6	18.3	81.0	70.2	45.1	49.3	83.9	75.4	46.2	13.0	67.8	81.5
Strong Republican	6.0	17.2	67.6	56.4	59.4	48.5	71.3	69.5	29.3	21.4	70.8	61.6
Total Sample	48	52	89	83	48	44	89	86	43	49	88	85

than that which existed on the exceptional issue of ownership of industry in the Eisenhower period. By 1968, in other words, to judge by six important items, the majority of the electorate had become more aware of party differences and had come to agree that the Democratic party was the liberal party.²¹

The change in perceptions of the parties' positions is not affected by controls for party identification. In all partisan groups, the Democrats were increasingly recognized as the liberal party on each of the six issues. Democrats were most likely to make this judgment, but by 1964 a majority even of Strong Republicans recognized the liberal credentials of the opposition. In 1956, by contrast, the Strong Republicans conceded this "honor" to the Democrats on only two issues.

The acceptance of the liberalism of the Democrats is most evident in the 1964 survey, conducted during the contest of Goldwater and Johnson. It was not solely related to that election, however, for important changes in perceptions of party position on the economic issues of aid to education, medical care, and job guarantees can be located as early as the 1960 campaign. The identification of the Democratic party as the liberal faction largely persisted through the 1968 election as well, although there was some lessening of perceived party positions, most notably on the issue of federal aid to education.

The most striking change occurred on racial issues. In 1956, there was no consensus on the parties' stands on the issues of school integration and fair employment. Differences between the parties were less likely to be seen, and Republicans were as likely as Democrats to be perceived as favoring federal action on civil rights. A startling reversal occurred in 1964: all partisan groups recognized the existence of a difference on this issue, and all were convinced that the Democrats stood more for government programs on behalf of blacks. Even

²¹ The perceptions of party are asked in different ways in the four surveys, so the data must be handled differently. In 1956, respondents were asked which party "is closer to what you want." To locate those who believe the Democrats are liberal on federal aid to education, for example, one must combine those who favor the policy, and think the Democrats are closer to their own position, with those who oppose the policy, and think the Republicans are closer. In 1960, 1964 and 1968, the question was asked in a straightforward manner, which party is likely to favor federal aid to education. These data are located, in 1956: two columns to the right of the policy question; in 1960: in decks and columns 4/60, 4/70, 4/56, 4/64, 4/75, 4/62; in 1964: 4/45, 4/60, 4/63, 5/13, 5/18, 4/69; in 1968: immediately after the policy question, except for party stands on school integration, 4/79.

Strong Republicans conceded the point they argued in the Eisenhower years. The identification of the Democrats with civil rights was slightly attenuated in 1968, but it still was a clearer perception than existed on any other issue.

Sources of Change

What accounts for these shifts? The increasing correspondence of party identification with policy preferences is probably related to the new perceptions of the parties as holding relatively distinctive and identifiable positions on these issues. Generational change is one possible explanation of these new perceptions. Younger, more ideologically attuned voters may have replaced older and less sensitive electors. To test this possibility, age is controlled in Table 4. Respondents in the age groupings used in the 1956 survey are compared to cohorts four, eight, and twelve years older in 1960, 1964, and 1968, respectively. If generational replacement is the main reason for the changed perceptions, the greatest differences would be seen between the upper-right and lower-left segments of each set of percentages. If political aging is not an explanation, changes would be most evident among cohort groups.²²

The data show that both conclusions are valid to some extent but that generational turnover is not an adequate explanation in itself. In virtually all comparisons, the new generation (below age 23 in 1964 or below age 27 in 1968) was more aware of party differences and more perceptive of Democratic liberalism than those it replaced (those 65 or older in 1956, or 69 or older in 1960, who were too depleted by 1964 and 1968 to be included). These differences are not as large, however, as are the changes of matched cohorts. Some political learning occurred in all age groups. Thus, in 1956 and 1960, most age groups did not see a party difference on racial issues, and all of them tended to identify the Republicans as more liberal. Particularly in 1964 and even in 1968, all age groups came to perceive a party difference, and there was intergenerational agreement on the Democratic party's greater support of fair employment and school integration.

The data give some indication of greater growth in awareness among older, rather than younger voters. The cohort aged 25-34 in 1956 (37 to 46 in 1968) appears to have be-

²² On the method of cohort analysis, see Neal E. Cutler, "Generation, Maturation and Party Affiliation: A Cohort Analysis," *Public Opinion Quarterly*, 33 (Winter, 1969-70), 583-592.

come particularly aware of Democratic liberalism in 1964 and to have held to its new perceptions in 1968. Speculating on the source of this change, we could note that this group came of age politically just after the realignment of the New Deal. Hence this group may not have been immediately affected by the political events of that period but may have been particularly susceptible to the effect of similar issues in 1964.

Degree of educational achievement might be an alternative explanation. The average level of schooling has increased considerably in the United States in this twelve-year period. Greater perception might result from the intellectual upgrading of the population, with less informed grade school or high school graduates being replaced by informed college alumni. Controlling for education would reveal this effect by indicating relatively little change by educational level. This control is presented in Table 5, which clearly shows that educational upgrading does not explain the shift in perceptions. Awareness has increased at all educational levels.

In fact, the disparities in perceptions among persons of varying schooling have tended to lessen, particularly in regard to identifying the Democrats as the liberal party. This effect was most marked in the 1964 campaign; in that year, on every issue, respondents with only a grade school education were clearer in their perceptions of the parties than college graduates were in 1956. The educational gap widened slightly in 1968, but the level of awareness remained substantially higher in all educational strata. In one sense, therefore, the political events of this period provided a tax-free learning substitute for the political education that might otherwise have occurred in high school and college classrooms. (The vocational implications for political science faculty members might best be left aside.)

A final control may be made for region and race, as in Table 6, since the changed perceptions noted above may have been concentrated in particular segments of the population. Such differences are evident in Table 6, although the changes are also apparent in both the North and South, and among whites as well as blacks. Southern whites particularly stand out. Their perceptions varied widely over the course of the four elections, but the net result has been relatively little change in their awareness of party differences or in their ideological identifications of the parties. They changed almost exclusively in their identification of the Democrats as the more liberal party on issues of civil

rights. These changing perceptions may well be related to the growth of Republican voting in the white South.²³

Blacks' opinions also varied widely, but the net result has been a substantially increased awareness of party differences and a clear identification of the Democrats as the liberal party. While this change is most dramatically evident on the civil rights issues, and among southern blacks, it is not confined to these issues or to the former Confederacy. Negroes of both regions also became more cognizant of differences between the parties and of Democratic liberalism on economic issues. It is interesting to note, moreover, that within each region in all years, blacks tended to be more conscious of political differences than whites. Given their perceptions of increasing differences between the parties, it is not surprising that blacks now overwhelmingly identify with and vote for the Democrats.²⁴

There is no obvious demographic cause for the changed awareness of party differences by the electorate. Neither the passing of generations, nor improved education, nor regional and racial variations provides a simple explanation. The fact remains that, during these twelve years, all segments of the population displayed considerable political learning (or misperception, depending on one's view of the "real" character of the parties). The alternative to a demographic explanation is a directly political one: the events and campaigns of the 1960s, I suggest, made politics more relevant and more dramatic to the mass electorate. In the process, party differences were developed and perceived. Democrats divided from Republicans, Democrats became more liberal, and voters became more aware.

There are many correspondences between the events of the political world and the voters' perceptions of the parties. In the 1960 campaign, John Kennedy and Richard Nixon, and the Democratic and Republican platforms, dif-

²³ See: Bernard Cosman, "Republicanism in the South," *Southwestern Social Science Quarterly*, 48 (June, 1967), 13-23; Philip Converse, Warren Miller, Jerrold Rusk and Arthur Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), esp. 1095-1101. The Wallace Campaign in 1968 did not substantially affect the perceptions of the major parties' positions on civil rights. In noting the parties' stands, a respondent could answer that there was no difference between the major parties, but Wallace did represent a distinctive position. Few respondents chose this option; those who did were included in this analysis with the "no difference" group.

²⁴ Of the 1968 SRC sample, only 3 of the 149 blacks identified themselves as Republicans, and only 3 voted for Nixon.

Table 4. Perceptions of Parties on Policy Issues, by Age

Existence of Party Differences									
Group		Aid to Education				Medical Care			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
(Percentages of Those with Opinions Perceiving Party Differences)									
—	Under 27	—	—	60.0	52.2	—	—	84.7	64.6
Under 25	27-36	49.1	67.2	58.4	56.7	57.8	74.0	83.9	68.7
25-34	37-46	49.1	45.7	65.3	59.8	49.0	51.4	82.5	77.8
35-44	47-56	45.2	53.1	72.7	62.9	50.2	66.1	81.0	68.6
45-54	57-66	53.3	58.0	65.9	59.6	54.0	52.0	76.6	71.4
55-64	Over 66	52.2	58.8	55.2	56.5	59.0	58.0	78.1	74.9
65 and over	—	54.0	59.3	—	—	53.5	52.6	—	—
Job Guarantee									
Group		Job Guarantee				Fair Employment			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
(Percentages of Those with Opinions Perceiving Party Differences)									
—	Under 27	—	—	65.8	65.8	—	—	78.1	60.5
Under 25	27-36	46.6	68.7	66.0	61.5	50.7	57.3	69.9	64.3
25-34	37-46	55.3	66.3	73.0	70.8	51.5	37.8	68.1	63.3
35-44	47-56	49.5	66.0	68.2	74.6	44.1	39.0	66.3	65.0
45-54	57-66	58.0	62.3	71.8	62.2	50.2	48.5	66.8	54.0
55-64	Over 66	63.9	59.0	66.2	70.3	39.1	42.6	65.6	58.2
65 and over	—	58.8	54.5	—	—	52.7	46.7	—	—
School Integration									
Group		School Integration				Foreign Aid			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
(Percentages of Those with Opinions Perceiving Party Differences)									
—	Under 27	—	—	75.6	56.2	—	—	54.5	42.2
Under 25	27-36	58.2	41.1	67.1	59.6	42.5	42.3	55.1	39.2
25-34	37-46	52.7	38.4	61.6	65.4	48.8	38.9	60.6	49.3
35-44	47-56	50.2	46.6	64.9	57.7	40.4	37.4	55.0	47.5
45-54	57-66	49.3	40.6	61.9	61.3	44.5	37.8	55.6	47.2
55-64	Over 66	53.1	28.5	54.7	56.9	44.9	33.0	54.8	50.6
65 and over	—	50.0	34.4	—	—	47.2	46.6	—	—

ferred on federal aid to education²⁵—and the voters became more likely to regard the Democrats' position on the issue as the more liberal. Medicare became a major partisan issue during the Kennedy administration, resulting in a series of partisan Congressional roll calls on the issue, and a platform conflict during the 1964 campaign. Perceiving this disagreement, the voters were much more likely in 1964 to see a difference between the parties on the issue of medical care and to identify the Democrats as

²⁵ Kirk Porter and Donald Johnson, *National Party Platforms*, 3rd ed. (Urbana: University of Illinois Press, 1966), pp. 590-614.

more favorable to governmental action on the issue. When a Republican administration acted vigorously to promote school integration, as in Little Rock, the voters tended to believe that the G.O.P. was more favorable to this policy. This opinion was particularly common in 1956 and 1960 among Southerners and blacks. When a Democratic administration came to support new civil rights legislation, when it sent marshals and National Guardsmen to desegregate the University of Alabama and the University of Mississippi, when it reacted sympathetically to the protests of Martin Luther King in Birmingham and Selma, the voters drew the conclusion that there was a difference between

Table 4—(Continued)

Consensus on Party Positions									
Group		Aid to Education				Medical Care			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those Perceiving Differences Selecting Democrats as Liberal)</i>									
—	Under 27	—	—	89.9	65.6	—	—	96.7	86.0
Under 25	27-36	51.8	77.9	86.2	72.3	73.8	82.4	92.2	88.6
25-34	37-46	66.0	73.5	84.8	79.4	78.9	89.4	94.1	91.5
35-44	47-56	75.8	74.6	77.4	67.0	81.6	80.0	87.5	87.6
45-54	57-66	69.7	90.2	86.5	73.1	80.2	82.7	95.5	83.3
55-64	Over 66	67.1	72.6	74.0	73.1	72.0	71.3	90.6	80.8
65 and over	—	51.8	60.8	—	—	64.5	75.9	—	—
Group		Job Guarantee				Fair Employment			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those Perceiving Differences Selecting Democrats as Liberal)</i>									
—	Under 27	—	—	92.0	79.0	—	—	93.6	83.7
Under 25	27-36	76.0	82.6	92.7	78.3	54.7	55.4	93.0	83.0
25-34	37-46	65.0	82.4	88.5	85.9	43.6	52.5	88.3	83.5
35-44	47-56	72.2	81.6	85.4	66.7	42.7	45.3	85.8	84.7
45-54	57-66	65.8	85.2	89.0	83.9	53.0	63.1	91.5	82.0
55-64	Over 66	62.5	72.1	90.8	74.5	48.0	42.0	89.3	75.6
65 and over	—	71.4	64.9	—	—	47.7	56.2	—	—
Group		School Integration				Foreign Aid			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those Perceiving Differences Selecting Democrats as Liberal)</i>									
—	Under 27	—	—	91.2	85.5	—	—	87.5	85.2
Under 25	27-36	50.0	34.9	92.6	84.5	34.8	50.0	90.6	85.8
25-34	37-46	42.6	44.6	92.0	90.0	60.1	44.0	88.1	87.7
35-44	47-56	46.8	40.0	85.8	85.3	40.6	46.0	86.5	85.9
45-54	57-66	51.0	50.0	90.0	84.6	48.1	60.1	88.0	83.2
55-64	Over 66	57.2	35.2	86.9	79.6	47.3	50.0	83.7	80.5
65 and over	—	49.0	52.3	—	—	38.6	46.0	—	—

the parties and identified the Democrats as more favorable to the cause of the Negroes. When the Republican party adopted a "Southern strategy" in 1968 and its candidate largely ignored the issue of civil rights, the voters continued to hold this position and clearly to distinguish the parties.²⁶

The most important electoral event of this period appears to be the 1964 Presidential campaign. Senator Barry Goldwater consciously sought to clarify and widen the ideological dif-

ferences between the parties. The evidence presented here indicates that he accomplished his goal, although it did not benefit the Republican party. Voters, previously unable to see differences between the parties, learned the lesson of "a choice, not an echo." They accepted the Senator's characterization of the Republicans as conservative and the Democrats as liberal; and, on the specific issues involved, they preferred the liberal alternative.²⁷ This process of education was not confined to the insightful young, or to the formally-trained college popu-

²⁶ On the development of medicare, civil rights and other programs in this period, see James L. Sundquist, *Politics and Policy* (Washington: Brookings Institution, 1968).

²⁷ Also see Lloyd Free and Hadley Cantril, *The Political Beliefs of Americans* (New Brunswick: Rutgers University Press, 1967), Chap. 2.

Table 5. Perceptions of Parties on Policy Issues, by Education

Existence of Party Differences												
Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of those With Opinions Perceiving Party Differences)</i>												
Elementary School	45.7	51.8	61.6	56.1	49.2	53.4	76.0	70.7	59.9	65.3	68.8	63.7
High School	52.2	55.4	61.6	53.0	50.0	60.0	80.6	68.0	52.3	71.2	65.5	61.4
Some College	49.0	58.2	66.9	61.7	57.0	63.8	84.3	68.9	48.1	56.4	71.2	68.1
College Graduate	65.2	60.2	73.2	67.4	61.7	63.9	87.8	84.1	55.2	56.5	71.9	74.9
Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of those With Opinions Perceiving Party Differences)</i>												
Elementary School	47.2	45.9	68.2	62.5	50.0	35.2	63.3	61.9	40.2	39.0	51.0	46.3
High School	49.1	36.3	67.7	61.2	54.1	37.3	61.3	57.1	43.1	35.5	54.8	47.5
Some College	46.5	48.3	67.5	63.1	51.7	34.4	66.7	60.2	47.1	42.3	55.7	42.2
College Graduate	44.3	44.4	65.8	57.6	51.8	41.2	58.9	61.7	62.0	36.5	68.3	51.6
Consensus on Party Positions												
Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentage of those Perceiving Difference Selecting Democrats as Liberal)</i>												
Elementary School	65.7	71.8	86.4	75.0	70.9	75.3	92.7	84.0	68.8	78.0	90.1	82.0
High School	62.3	73.9	77.6	70.3	75.5	85.0	90.3	87.0	69.9	83.6	87.2	76.2
Some College	67.5	78.9	77.4	71.6	82.9	81.1	89.4	86.4	65.0	80.0	85.6	79.2
College Graduate	75.8	88.7	86.9	75.8	89.0	93.9	95.4	92.0	72.2	82.9	90.0	84.4
Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentage of those Perceiving Difference Selecting Democrats as Liberal)</i>												
Elementary School	49.7	53.7	91.6	79.7	45.5	51.5	87.2	84.8	46.5	47.8	88.4	84.8
High School	53.0	63.8	88.5	81.3	44.5	41.7	91.2	83.3	40.6	58.2	86.0	83.5
Some College	39.2	44.0	85.3	83.6	51.0	36.7	87.0	87.7	51.0	40.5	83.6	84.3
College Graduate	46.5	39.5	91.3	88.9	56.0	35.8	96.6	87.0	56.0	54.8	97.2	86.4

lation, or to committed White segregationists and Black integrationists. This political education was general and apparently persistent. The party characteristics which had been so clearly marked in 1964 remained relatively evident to the voters four years later. The lessons remained learned even though differences between the candidates had narrowed considerably, and although some of the previous issues, such as aid to education and medical care, had been partially resolved. Differences were not perceived as clearly in 1968 as in 1964, but

they were far more apparent to the voters than in the Eisenhower years.

Implications of the Data

The data developed in this article lead to three implications. First, the central importance of the 1964 campaign lends support to the supposition that this election was a critical election, initiating a new political era in the United States, rather than the aberrant event it appeared at the time. A critical election, such as that of the New Deal, is one in which a

Table 6. Perceptions of Parties on Policy Issues, by Region and Race

Existence of Party Differences												
Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those With Opinions Perceiving Party Differences)</i>												
North White	48.0	57.3	61.1	58.0	51.2	53.6	81.1	71.5	52.4	63.2	66.4	65.2
North Black	66.7	80.0	71.8	72.6	61.5	57.9	83.3	79.2	75.0	79.0	78.8	77.0
South White	51.0	49.3	68.8	50.2	54.6	57.6	78.3	65.3	61.5	62.2	69.8	61.0
South Black	62.4	50.8	80.8	87.7	54.7	45.7	83.3	88.1	64.5	68.3	81.8	90.9
Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentages of Those With Opinions Perceiving Party Differences)</i>												
North White	43.2	44.2	63.5	59.2	49.5	37.3	59.2	57.6	43.3	36.2	53.2	45.0
North Black	79.5	73.2	88.2	87.5	57.5	57.2	85.7	81.2	43.5	50.9	57.8	45.1
South White	52.7	36.4	67.4	56.1	57.0	30.6	64.3	56.0	49.7	39.1	57.9	44.9
South Black	70.0	60.0	89.3	89.9	58.5	46.0	82.2	88.1	61.8	38.1	63.3	75.0
Consensus on Party Positions												
Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)</i>												
North White	66.4	75.9	81.2	68.0	74.8	81.7	93.1	87.0	66.0	78.8	88.8	79.0
North Black	77.2	84.4	100.0	95.0	81.2	91.0	97.5	100.0	76.0	91.1	97.3	96.0
South White	63.8	80.8	74.3	70.0	81.9	79.3	83.6	79.0	67.4	82.5	81.3	72.4
South Black	75.0	53.3	98.4	100.0	82.3	71.4	98.7	98.2	80.0	86.1	98.6	100.0
Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
<i>(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)</i>												
North White	47.1	78.8	88.8	81.1	54.2	48.3	91.2	84.0	45.2	52.2	89.5	84.1
North Black	65.7	83.7	98.0	100.0	60.8	32.1	100.0	100.0	70.0	57.1	84.6	86.9
South White	34.8	48.0	79.0	75.0	34.4	37.4	77.5	79.6	45.7	41.4	83.2	82.4
South Black	60.6	53.8	100.0	100.0	35.3	17.6	100.0	100.0	61.5	43.8	100.0	100.0

deep and enduring cleavage in the electorate becomes evident.²⁸ Characteristic of such elections is increased voter consciousness of policy questions, and the later electoral persistence of group divisions based on the policy questions raised in the critical election.²⁹ These

²⁸ V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), pp. 3-18. The concept and its significance have been deeply researched in Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970).

²⁹ The point is elaborated well in Everett C. Ladd,

hallmarks of a critical period are evident in the upsurge of mass perceptions of party differences in 1964 and the persistence of these perceptions in 1968. While the voters did not respond ideologically in the full sense of the term, they did respond to the specific issues presented to them, and they did align their partisan loyalties far closer to their policy preferences. Such readjustments occur rarely, but

American Political Parties (New York: Norton, 1970), pp. 1-10, and illustrated historically in the body of this book.

they are the decisive moments in American political history.

Research at the time of the 1964 election led to the conclusion the electorate did not respond primarily in ideological terms,³⁰ although there already were signs available that deeper forces were moving the voters.³¹ Subsequent work by younger scholars, perhaps less committed to prevailing commonplaces, has shown the growth of ideological constraints and divergent "party images" during the turbulent decade of the 1960s.³² Moreover, the most current controversies are tending to emphasize the ideological division between the parties. Partisan conflicts on such issues as national health insurance, federal revenue sharing, or enforcement of equal employment opportunities are likely to reinforce the popular belief that Democrats favor increased federal government action and Republicans prefer private or state governmental action.³³ Perceptions of party differences are therefore likely to be confirmed and extended.

Confirming evidence is needed to demonstrate a critical election in 1964. Such evidence would include shifts in party identification among substantial groups of voters and the emergence of new issues. The data presented here do not speak to these questions. Indeed, they deal only with issues already evident in 1956. We do find, however, increased coherence of policy positions within each party following, and new sharp cleavages associated with racial issues. These findings are congruent with those expected in a period of realignment.

Clarification and realignment of the parties' policy positions leads to a second implication, the possible development of a "responsible two-party system" in the United States. Such a system was advocated by a committee of the

American Political Science Association in 1950.³⁴ That committee, however, expected more of voters than was possible at the time. To effectuate the system of party government advocated in the *APSA Report*, the voter must perceive a relationship between his policy preferences and his partisan choices. Furthermore, for the parties to serve as links between voter preferences and public policies, their programs must be perceived as somewhat distinct and reasonably clear. These are necessary but not sufficient conditions for developing a responsible party system in which parties put forth programs, receive popular approval of these programs, and then carry out the popular mandate.

That these conditions did not exist at the time of the *APSA Report* is indicated by our data for the 1956 election. On most issues, the voters did not relate their policy preferences to their partisan affiliations nor did they see a difference between the parties, nor did they agree on the relative positions of the two parties. In recent years, however, the situation has changed. There is a sizeable statistical correlation now between party identification and policy preference on the particular issues studied. Voters now do see a difference between Democrats and Republicans and agree on their ideological character.

Parties can now meaningfully stand as "groups of like-minded men" offering particular stances toward public issues. Their victories in elections can now reasonably be interpreted as related to the mass choice of one set of issue positions over another. To this extent, the conditions for a responsible party system have been fulfilled.

Other necessary conditions, however, have not been met. Greater voter awareness of party differences does not in itself create a mandating election system. It would also be necessary to show that votes are actually cast *because of* issue preferences. Moreover, a responsible party system requires not only a responsible electorate to provide direction, approval, or castigation for the parties. It also requires coherent, effective, and relatively disciplined parties. In fact, both in the *APSA Report* and in the British party system which its authors cited with admiration, party government is crucially dependent on the internal discipline of the parties, even more than on the electorate.³⁵ There is

³⁰ See Philip Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 330-335.

³¹ See: Walter Dean Burnham, "American Voting Behavior and the 1964 Election," *Midwest Journal of Political Science*, 12 (February, 1968), 1-40; John Kessel, *The Goldwater Coalition* (Indianapolis: Bobbs Merrill, 1968), pp. 301-308; Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), Chap. 5; David Segal, "Partisan Realignment in the United States: The Lesson of the 1964 Election," *Public Opinion Quarterly*, 32 (Fall, 1968), 441-444; RePass, "Issue Salience and Party Choice," 398-400.

³² See: David Nexon, "Hacks, Fanatics, and Responsible but Dense Voters," (Unpublished M.A. Thesis, University of Chicago, 1970); and Rick S. Piltz, "Mass support for the Political Parties: Bases for Realignment," (Ph.D. Dissertation, University of Michigan, in progress).

³³ See the roll calls listed in *Congressional Quarterly Weekly Report*, 29 (January 29, 1971), 220-222, dealing with the 91st Congress.

³⁴ American Political Science Association, Committee on Political Parties, "Toward a More Responsible Two-Party System," *American Political Science Review*, 44 (September, 1950), Supplement.

³⁵ On the British electorate, see David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1969), esp. Part IV.

comparatively little evidence of the development of cohesive parties in the United States.³⁶ Many developments in fact have tended in the opposite direction, such as the diffusion of party power to wealthy individuals, media specialists, and campaign managers; the challenge to entrenched party leaders through party primaries and the decentralizing reforms of national conventions; and the separation of national, state, and local elections. The electorate is more ready today than in the 1950s for a responsible party system, but the parties may be too weakened for the task.

The final implication of the data relates to the study of American voting behavior. Because of the excellence with which the Michigan studies have been conducted and presented, we have tended to overgeneralize the findings of such studies as *The American Voter*. We have assumed that this superb analysis of the 1950s is a study of the electorate of all time. Because voters of the Eisenhower period did not respond to the parties in ideological terms, we have often concluded that they could not respond in such terms. Yet, Key reminded us that "the voice of the people is but an echo . . . The people's verdict can be no more than a selective reflection from among the alternatives and outlooks presented to them."³⁷ If the parties do not emphasize issues, or do not present distinct and clear positions, the voters are unlikely to invent party programs. When there are party positions and differences, the voters can perceive them. Students of electoral behavior must be sensitive to these changing stimuli and reactions.

In observing electoral behavior and mass ideology, we particularly must beware of our own ideological biases. It is instructive to remember that *The American Voter* was written at the time of "the great American celebration," when the dominant academic ideology was that the United States had reached the "end of ideology" through a consensual agreement on twentieth-century liberalism.³⁸ It was therefore comforting and appropriate to find that ideology

was absent among the mass public. In the absence of ideology, basic social change was difficult to imagine. The existing political system was therefore seen as stable and relatively insulated from change. Throughout the Michigan studies, the emphasis is on the inertial elements of American politics, an emphasis that, in keeping with the tone of the era, plays down the possibility and desirability of basic social change.³⁹

In the last decade, after a series of political shocks, ideological conflict has been resurrected, and consensus has been severely disrupted, if not destroyed. Basic issues have been raised again, and the electorate has shown itself able to comprehend and respond to such conflicts. Furthermore, an emphasis on ideological passivity is itself seen to be ideological. Earlier findings on the electorate remind us of the dangers of overgeneralization from data derived from a limited temporal or spatial context.

More generally, we must remember to be aware of the political context of voting behavior. Perhaps the major fault of the Michigan studies has been the comparative neglect of the political environment as an independent variable. The methodology of survey research has brought an overemphasis on the individual behavior of isolated respondents. The influences upon these respondents have been studied only indirectly, through the voters' personal perceptions and actions.⁴⁰ But voters in fact are not isolated, for they are affected by their environment,⁴¹ the mass media, the economic system, and the prevailing ideology. More attention must be devoted to these shaping influences.

Most critically, we must emphasize in this context the effect upon voters of the stimuli they receive from the parties and other electoral actors. If these stimuli are issueless and static, as they largely were in the 1950s, the citizenry is likely to respond in the manner described in *The American Voter*. If these stimuli are more

³⁶ For example, note the concluding chapter of *The American Voter* or Chaps. 2, 8, 10, 12 of *Elections and the Political Order*, which are largely reprints of earlier articles.

³⁷ Note the criticisms of Kenneth Prewitt and Norman Nie, in "Revisiting the Election Studies of the Survey Research Center," a paper prepared for delivery at the 1970 meeting of the American Political Science Association, p. 18:

The SRC group has written persuasively regarding the implications for American politics of the findings about citizen information and awareness. They have less critically discussed the implications for voter rationality of their findings about election processes and alternatives.

³⁸ Warren Miller recognized this point in "One Party Politics and the Voter," *American Political Science Review*, 50 (Sept., 1956), 707-725.

³⁹ Sundquist, however, in *Politics and Policy*, Chaps. 9 and 12, argues that Democratic party actions in the 1950s and 1960s already constituted the creation of a responsible party system. In contrast, see Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in *Elections and the Political Order*, Chap. 11. Originally published in 1962, the latter work shows the absence of conditions for responsible parties, at least before the possibly critical election of 1964.

⁴⁰ Key, *The Responsible Electorate*, p. 2.

⁴¹ Daniel Bell, *The End of Ideology* (New York: The Free Press, 1960); Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace and World, 1955).

Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election*

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This paper presents two arguments. One argument is that the 1968 election bears witness to the importance of issues at critical points in political history. American surveys show that issues vary greatly over time in their total impact on elections. From 1936 to 1948 the question of the role of the federal government had a major effect on the flow of voters between the parties.¹ In contrast, studies conducted during the rather apolitical period of the 1950s demonstrated both a lack of ideological structure in people's beliefs and a marginal impact of issues in influencing their votes.² With the Goldwater and Wallace insurgencies of 1964 and 1968, issues became again important components of electoral decisions.³ This was particularly true

of Vietnam, urban unrest and race, social welfare, and Johnson's performance as president—the dominant issues of 1968.

A second argument is that issues having a similar impact on the election outcome can have very different consequences for popular control of public policy. On some issues the electorate exercises no effective constraints on leaders' policy choices. On others, the electorate permits political leaders a wide array of options when a policy is adopted but passes a retrospective judgment on the policy in subsequent elections. On still other issues, the public severely limits the options of leaders at the time policy is made. Race and public order, it is argued, are examples of issues on which the electorate places the most significant restraints on the policy initiatives of political leaders.

A Normal Vote Analysis of Issues in 1968

Converse's normal vote technique is a particularly useful means of relating issues to the vote.⁴ Essentially, the method provides an esti-

* Fred I. Greenstein, Herbert H. Hyman, and J. Donald Moon of Wesleyan University, C. Richard Hofstadter of Ohio State University, and Robert D. McCleure of Syracuse University criticized an earlier draft of this paper. Charles S. Hyneman of Indiana University first stimulated my interest in popular control of public policy; his books and his conversation have formed my ideas on the subject. Without committing these people to a defense of the product, I would like to acknowledge and thank them. The data utilized in this study were made available by the Inter-University Consortium for Political Research. The data were originally collected by the Survey Research Center of the University of Michigan. Neither the Center nor the Consortium bear any responsibility for the analyses or interpretations presented here.

¹ V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), Chap. 3.

² Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), Chaps. 8–10. Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David Apter, (New York: Free Press, 1964), pp. 206–61. Philip E. Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue," in Edward R. Tufte, ed., *The Quantitative Analysis of Social Problems* (Reading, Mass.: Addison-Wesley, 1970), pp. 168–89. Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406–27.

³ For analyses of issues in the 1964 election, see Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966), 19–28; John H. Kessel, *The Goldwater Coalition* (Indianapolis: Bobbs-Merrill, 1968), Chap. 9; Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 321–36; Walter Dean Burnham, "American Voting Behavior and the 1964 Election,"

Midwest Journal of Political Science, 12 (Feb., 1968), 1–40; and David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400. For 1968 see Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (Dec., 1969), 1083–105; Herbert F. Weisburg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (Dec., 1970), 1167–85; Seymour Martin Lipset and Earl Raab, "The Wallace Whitelash," *Transaction*, 1 (Dec., 1969), 23–35; Samuel A. Kirkpatrick and Melvir E. Jones, "Vote Direction and Issue Cleavage in 1968," *Social Science Quarterly*, 51 (Dec., 1970), 689–705; and Gerald M. Pomper, "Controls and Influence in American Elections (Even 1968)," *American Behavioral Scientist*, 13 (Nov./Dec., 1969), 215–30.

⁴ Philip E. Converse, "The Concept of a Normal Vote," in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: Wiley, 1966), pp. 9–35. Aside from this seminal article, which utilizes the normal vote to estimate the importance of the religious issue in 1960, the normal vote technique has seen limited application. Two exceptions are J. Merrill Shanks, "The Impact of Voters' Political Information on Electoral Change: A Reexamination of the Quality of American Electoral Decisions," (unpublished Ph.D. dissertation, University of Michigan, 1970), which presents an extension of Converse's work; and John H. Kessel, *Goldwater Coalition*, p. 294–97. Two other

mate of a party's *expected* proportion of the vote for any selected subgroup of the electorate in a normal election. This estimate is based on three factors: the balance of party identifiers within the group, the typical defection rates of identifiers to other parties, and the turnout rate. In the American context, a normal vote is defined as one in which the defection rates of Republicans and Democrats to the opposing parties are equal, with an adjustment made in the prediction in recognition of the greater tendency of Republicans to vote and to vote loyally for their party's candidate.⁵

Applying the concept of a normal vote to a political issue requires a calculation of normal (expected) votes for each group of persons holding different positions on the issue. Computing these expected votes is quite straightforward:⁶

(1) Responses to the issue are cross-classified against the Survey Research Center measure of party identification.

(2) The scores (+2, +1, 0, -1, -2) are assigned to the five classes of identifiers (Strong Democrat, Weak Democrat, Independent, Weak Republican, Strong Republican).

(3) From the scores, a mean party identification score is calculated for the distribution of party identifiers within each response category of the issue item.

(4) The obtained mean (M) is then inserted into the prediction equation,

$$V = .483 + .268M,$$

where V is the expected Democratic proportion of the vote. This linear equation is an approximation of a more complex equation. Converse

states the linear equation estimates the predictions of the more complex equation within +1 to -1 per cent for means within the range of +.8 to -.8. This confidence interval includes the range of expected Democratic vote percentages from 27.1 to 69.5.

The intuitive meaning of the normal vote equation is easily grasped. The constant (.483 per cent) is the expected Democratic vote for a group of identifiers that is perfectly balanced among Republicans and Democrats. (Thus, the mean [M] would be zero, and the predicted Democratic vote [V] would equal the constant.) The fact that the Republicans would win a majority from a group equally divided between Republicans and Democrats reflects the disadvantage the Democratic party suffers as a result of the greater relative loyalty and turnout of Republican partisans. A predominantly Democratic population yields a positive mean which adds to the constant, just as a Republican population yields a negative mean which diminishes it.

As the equation makes clear, the only factor that enters directly into the calculation of an expected vote for a group of people is the distribution of party identifiers within it. This equation, while cheap in data costs, does presume that the expected Democratic vote is the same for all subgroups of the electorate having the same distribution of party identifiers. This assumption is not as questionable as it might first appear. Shanks has begun an extension of Converse's procedures, adding to the prediction equation a more theoretically pleasing set of variables. Since Shanks's estimates of the expected Democratic vote for several subgroups are almost identical to those of Converse for the presidential elections of 1952-1964, I have used Converse's more parsimonious equation.⁷

The utility of the normal vote calculation is that it permits the analyst to separate issues into their *partisan* (long-term) components and their *short-term* components. Consider Figure 1 as an example of a normal vote analysis of an

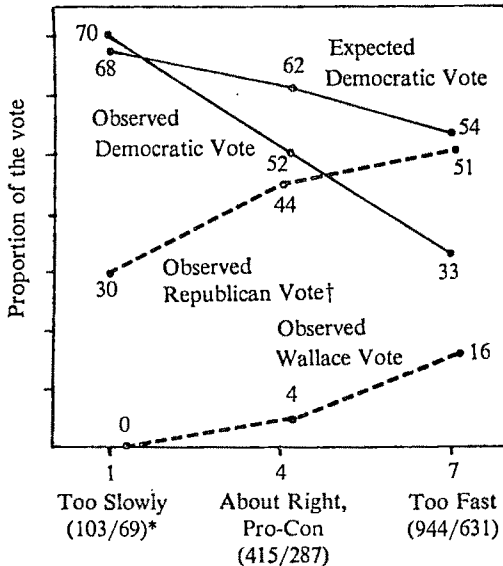
articles have used the intuitive notion of the normal vote, though the authors estimated the normal vote with aggregate data. See Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science*, 23 (Feb., 1969), 58-83; and Barbara Hinckley, "Incumbency and the Presidential Vote in Senate Elections: Defining the Parameters of Subpresidential Voting," *American Political Science Review*, 64 (Sept., 1970), 836-42.

⁵ Though Republicans vote at higher rates than Democrats, the effect of the *total* turnout rate on the two-party vote division is slight. Converse, "The Concept of a Normal Vote," pp. 28-30. Kabaker, "Estimating the Normal Vote . . .," pp. 78 and 83. Therefore, the assumption that Converse must make regarding the level of turnout underlying the normal party division is not critical.

⁶ Converse, "The Concept of a Normal Vote," pp. 34-39. A normal vote analysis for a particular issue requires only simple arithmetic and two cross-classification tables (one to compute the expected party vote and another to determine the actual party vote). Therefore, the technique is particularly suitable for undergraduate research exercises.

⁷ Shanks, "Impact of Voters' Political Information . . .," p. 149. Converse and Shanks define a normal vote differently. Converse defines a normal vote empirically, by averaging the results of several elections and by assuming that short-term forces are averaged out in the process. Shanks treats a normal election as a pure, hypothetical concept and obtains estimates of the propensity of individuals to vote for a party by a technique of simulation. Shanks's results are strongly affected by the constraints he places upon the simulation (a fact he is well aware of). An important consideration in my use of Converse's equations is that one of Shanks's constraints is that the simulation must produce results comparable to those of Converse.

Figure 1. The Normal Vote and Speed of Movement on Civil Rights



Item: "Some say that the civil rights people have been trying to push too fast. Others feel they haven't pushed fast enough. How about you: Do you think that civil rights leaders are trying to push too fast, are going too slowly, or are they moving about the right speed?"

* In the parentheses, the first figure is the frequency base for the computation of the expected Democratic vote. It includes all party identifiers and independents. The second figure is the frequency base for the computation of the actual vote percentages. It includes all respondents who cast votes for one of the three major candidates.

† The expected Republican vote is 100 minus the expected Democratic vote.

important issue in 1968—the speed with which civil rights leaders have been pushing their cause.

The Partisan Component of a Civil Rights Issue.

The partisan component is the degree of party polarization on an issue.⁸ Party polarization in turn is a function of two factors:

(1) One aspect of polarization is the degree of association of issue positions with party identification. Party identification is both stable over the lifetime of most voters and a major determinant of their electoral choices.⁹ There-

⁸ The careful reader will note that I am defining the concept of a partisan component differently than Converse does. In my usage the partisan component of an issue is simply its relation to party identification. Converse defines "partisan forces" as short-term stimuli, which are pro-Democratic or pro-Republican in varying degrees of strength and which induce defections from the normal party vote. Converse, "The Concept of a Normal Vote," p. 15.

⁹ Campbell et al., *The American Voter*, Chaps. 6-7.

fore, an issue on which mass party members are polarized sinks its roots into a stable, long-term division in the electorate. Knowing the balance of party loyalties of people holding some view on a particular issue, one can predict how these persons would vote in a normal election. These predictions are called *expected Democratic votes*.

The partisan component of an issue is manifest in changes in the expected Democratic proportion of the vote across issue positions—the greater the changes, the more partisan are people's attitudes on the issue. In Figure 1 the balance of party identification among persons believing that civil rights people have been pushing too slowly so favors the Democrats that the Democrats would expect to receive 68 per cent of the votes in a normal election. In contrast, among those who believe civil rights people are pushing too fast, the Democrats would normally expect to receive 54 per cent of the vote. This difference of 14 percentage points (68 minus 54) reflects the relative degree of polarization of the mass parties on the issue.

(2) A second aspect of polarization is the proportion of people advocating each of the issue positions. Polarization is born of conflict of opinions; as consensus is approached, an issue ceases to divide the parties. In Figure 1, nine times as many people think civil rights people are pushing too fast as think they are pushing too slowly. In Stokes's terms this is almost a valence issue, an issue on which there is no disagreement;¹⁰ as such, it cannot be a partisan issue.

The Short-Term Component of a Civil Rights Issue.

The short-term component of an issue is its relationship to defections from the normal party vote. In Figure 1 the short-term component is distinct. Humphrey received more than his full complement of expected votes from those who thought civil rights people were pushing too slowly. But those who believed civil rights leaders were pushing too fast gave Humphrey only 33 per cent of their votes, 21 percentage points below what Humphrey normally could have expected. The greatest proportion of the losses in this latter group went to Wallace. The expected Republican vote for this group is 100 minus the expected Democratic vote of 54 per cent, or 46 per cent. Nixon actually received 51 per cent of their votes, which was 5 points better than he could normally expect. The remainder

¹⁰ Donald E. Stokes, "Spatial Models of Party Competition," in Campbell et al., *Elections and the Political Order*, p. 170. The opposite of a valence issue is a position issue.

(21 minus 5) of Humphrey's 21 point loss is the 16 per cent Wallace vote in that category. Again, we note the distribution of opinion on the issue. The rate of defections of Democrats from Humphrey was greatest among those who believed civil rights leaders were pushing too fast, a group which easily held the majority position on this issue.

What can we conclude from the observation of the deviations from the normal Democratic vote? Certainly not that the issue was causally related to the vote or that the issue made some net contribution to the observed Democratic vote. The reason is that from survey data it is difficult to determine with precision the issues (if any) that lead each person to cast his vote for a particular party. Lacking a method of categorizing the electorate, normal vote analysis cannot ascertain the net gain or loss of an issue to a candidate.

What we can say is this: For respondents in this sample the distribution of party identification is such that the Democrats would expect to receive 57 per cent of a normal vote.¹¹ In fact Humphrey received 41 per cent, 16 percentage points below a normal Democratic vote. If

¹¹ On the basis of the five presidential and congressional elections from 1952 to 1960, Converse estimates the normal vote for the whole population to be 54 per cent Democratic. By 1968 the balance of party identification has become slightly more Democratic. Applying Converse's equation, $V = .483 + .268M$, to the 1968 sample gives an expected Democratic vote in 1968 of 57 per cent Democratic. Having recomputed the normal vote for the 1968 sample, I do not have to assume that the distribution of party identification was unchanged from the 1952-1960 period, i.e., that partisan realignment was not taking place in 1968. I am assuming that the probability of an individual party identifier voting for the candidate of his own party remained basically unchanged from the earlier period.

The use of normal vote analysis does present two problems of interpretation. The first problem is that short-term political factors slightly influence responses to the party identification measure. (See footnote 28, Kessel, *Goldwater Coalition*, pp. 295-96.) As a result some short-term political influences will be submerged in the expected party vote. The effect of this result is to make the short-term relationships of issues to votes slightly conservative underestimates of the true relationships. A second, more difficult problem is the Wallace candidacy in 1968. The estimates of the normal vote assume only two major candidates, not three. With Wallace in the race, the normal vote analyses can be interpreted as follows: The normal vote projects the expected vote in 1968 if short-term forces had been in balance and if only two major candidates had run. The Wallace candidacy is then interpretable as a short-term force responsible for deviations from the expected Democratic and Republican votes. In justification of this interpretation, there is scant evidence that many voters identified themselves as members of the American Independent Party of Wallace. On the 1968 party identification measure, only 0.2 per cent of the sample falls into the category reserved for people who think of themselves as members of a minor party.

Humphrey had lost this same 16 percentage points across every issue position, then this issue would not even be statistically related to short-term defections from the normal vote. Such is not the case with the civil rights issue in the previous example. Humphrey exceeded his expected vote among some civil rights groups and fell short in others. The variation of the actual gains and losses in each issue category (+2, -10, and -21) from the overall deficit (16 points) is a measure of the issue's strength of association with short-term defections from the normal vote. Thus, even if we cannot speak in causal terms of an issue's contribution to the vote, we can compare issues statistically in terms of their association with long-term party loyalties and short-term defections from the normal party vote. Appendix II provides the formulas for such comparisons.

With this explanation of normal vote analysis, we can now decompose a set of 15 issues in the 1968 SRC election study and compare them in terms of their long- and short-term components. This set of issues falls into four broad groupings: Vietnam, social welfare, urban unrest and race, and civil liberties and public protest. A final issue orientation, the electorate's evaluation of Johnson's performance as president, does not fit neatly into any of these categories; it will be considered separately.

Vietnam: The Vulnerability of the Status Quo Position. The 1968 election study includes two very useful items probing attitudes toward the Vietnam war: escalation vs. de-escalation, and immediate withdrawal vs. military victory. Both reveal similar patterns, illustrated by Figure 2, a presentation of preferences for escalation vs. de-escalation.

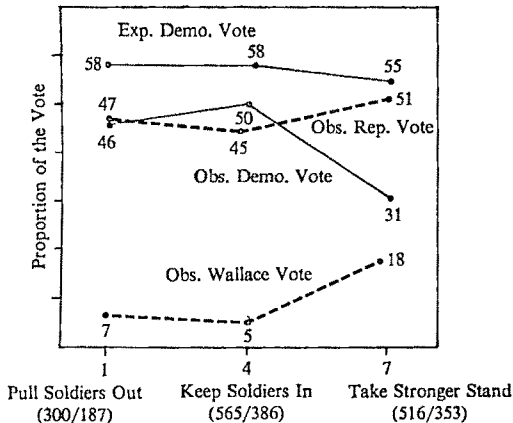
As one would predict on a matter of foreign policy, there are no long-term, partisan aspects of people's attitudes about Vietnam.¹² Republicans and Democrats are almost equally likely to endorse solutions ranging from pulling out of Vietnam entirely to taking a stronger stand even if it means invading North Vietnam. This is reflected in the similarity of the expected Democratic votes in the three categories of policy alternatives.¹³

In contrast, the short-term component of at-

¹² Campbell et al., *The American Voter*, Chap. 8; V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1964), Chap. 7.

¹³ The average of the deviations of the expected Democratic vote in the response categories from the grand expected Democratic vote is only 1.18 per cent. This small average deviation puts this item in last place among the 15 items when ranked according to the magnitude of their partisan components. See Appendix II for a fuller explanation.

Figure 2. The Normal Vote and Escalation/De-escalation

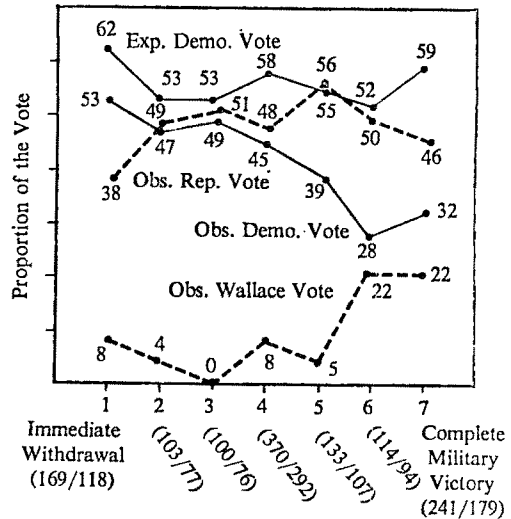


Item: "Which of the following do you think we should do now in Vietnam?"
 1. Pull out of Vietnam entirely.
 4. Keep our soldiers in Vietnam but try to end the fighting.
 7. Take a stronger stand even if it means invading North Vietnam.

titudes on escalation vs. de-escalation is evident. Among those who wanted to pull out entirely, Humphrey's vote is 12 percentage points lower than expected. Among those who would consider invading North Vietnam (34 per cent of the total sample), Humphrey's vote is almost 24 percentage points below its expected level. Humphrey's best showing is among the advocates of a policy of keeping American soldiers in Vietnam while trying to end the fighting. This one might interpret as a status quo position, a position with which the public apparently identified Humphrey because he was a member of an administration pursuing a similar policy.¹⁴

Obviously, Humphrey fared more poorly among the proponents of escalation than he did among those of de-escalation. His showing was weakest among the advocates of escalation, who outnumbered the advocates of de-escalation by nearly 2:1 among the voters. In addition, the proponents of escalation gave both

Figure 3. The Normal Vote and Immediate Withdrawal/Military Victory



Item: "There is much talk about 'hawks' and 'doves' in connection with Vietnam, and considerable disagreement as to what action the United States should take in Vietnam. Some people think we should do everything necessary to win a *complete military victory*, no matter what results. Some people think we should *withdraw completely* from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale (Show card #4 to R) at point number 1. And suppose the people who support a complete military victory are at the other end of the scale at point number 7. Where would you place yourself [on this scale]?"

Nixon and Wallace the greatest proportion of votes they received from any of the other opinion groups (51 and 18 per cent respectively).

The second Vietnam item yields results that are much like those of the first. This item asked the respondents to place themselves on a seven-point scale whose extreme positions are described as "immediate withdrawal" vs. "complete military victory." Figure 3 presents the normal vote breakdown for this issue. The expected or normal Democratic vote across the seven positions varies more or less randomly. As in the previous Vietnam item, the critical component of the issue is its relation to departures from the normal vote. Humphrey sustained his principal losses on the escalation flank; his fewest (49 versus an expected vote of 53 per cent), from a position just left of center. As in the case of the first Vietnam item, the advocates of immediate withdrawal gave Humphrey much more support than did the proponents of military victory. Figure 4 details the pattern of losses on the left and right with a

¹⁴ Rosenberg, Verba, and Converse argue, "... Vietnam as an issue played little role in deciding the election between Nixon and Humphrey ... because voters perceived little difference between the candidates." Milton J. Rosenberg, Sidney Verba, and Philip E. Converse, *Vietnam and the Silent Majority* (New York: Harper and Row, 1970), p. 50. Even though the voters saw little difference between the two candidates on Vietnam, the normal vote analysis shows that people at odds with the war policy still punished Humphrey at the polls. For an analysis of the relationship of views on the war to modes of political participation, see Sidney Verba and Richard Brody, "Participation, Policy Preferences, and the War in Vietnam," *Public Opinion Quarterly*, 34 (Fall, 1970), 325-32.

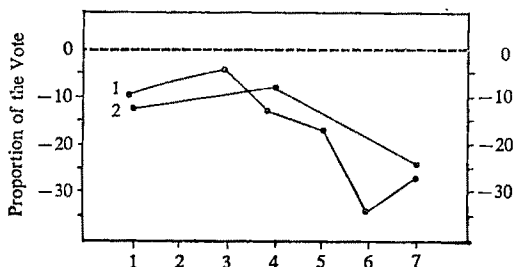
graph of the differences between the expected and the actual Democratic vote on both items.

Urban Unrest and Race: The Future of American Politics. Beliefs concerning urban unrest and race are central to an analysis of the 1968 election; these issues had an unmistakable short-term relationship to the vote, and they appear likely to dominate American politics for years to come. As has often been suggested, it is quite possible that the present Democratic party, based as it is upon a New Deal assemblage of class, ethnic, and regional groups, cannot withstand the impact of these issues on its blue-collar members. The problem of the Democratic party is evidenced in the following analysis.

Figure 1, the example used earlier to explain the concept of a normal vote, illustrates the loss of Democratic support among people who thought that civil rights leaders were pushing their cause too swiftly. The same voting pattern is manifested by people who advocated the use of all available force to quell urban unrest. Figure 5 presents a normal vote analysis of people who were asked to place themselves upon a seven-point continuum concerning alternatives for dealing with the urban problem. One end of the continuum is demarcated by "correct the problems of poverty and unemployment that give rise to the disturbances;" the other, by "use all available force to maintain law and order—no matter what results."¹⁵ As Figure 5

¹⁵ Two caveats about the item, itself. One is that it poses alternatives that may not stand as such in the minds of the voters. People may distinguish between long- and short-run solutions to riots, advocating use of force as an immediate measure and alleviation of poverty as a longer-run solution. A second difficulty

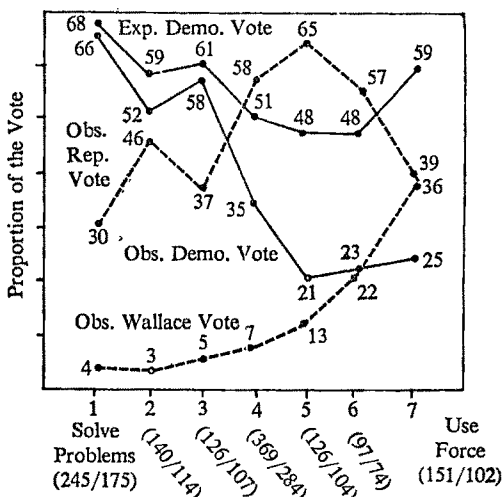
Figure 4. Deviations from the Expected Democratic Vote on Two Vietnam War Items.



Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote. The two lines represent the following items:

1. Escalation vs. de-escalation. See Figure 2 for the item.
2. Immediate withdrawal vs. military victory. See Figure 3 for the item.

Figure 5. The Normal Vote and Urban Unrest



Item: "There is much discussion about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to *use all available force* to maintain law and order—no matter what results. Others say it is more important to *correct the problems* of poverty and unemployment that give rise to the disturbances. And, of course, other people have opinions in between. Suppose the people who stress the use of force are at one end of this scale—at point number 7 (Show card #3 to R). And suppose the people who stress doing more about the problems of poverty and unemployment are at the other end—at point number 1. Where would you place [yourself] on this scale?"

demonstrates, Humphrey captured a substantial majority of the votes of those endorsing a "correct-the-problems" approach. In contrast, he suffered massive losses (34 percentage points) among the advocates of the polar position of force as the solution. Humphrey lost the bulk of these normally Democratic votes to Wallace rather than to Nixon. Nixon pulled his strongest support toward the midrange of the scale, while Wallace's strength peaked at the polar right of the continuum. Among the people of

with the item is its penumbra of social welfare content. It clearly presumes that poverty and unemployment are causes of riots and that solving these problems will still them. As anyone knows who has plunged into the ballooning literature on urban violence, competing explanations abound. The gamut ranges from the riff-raff theories of everyman to the exotic aggression theories of the animal behaviorists.

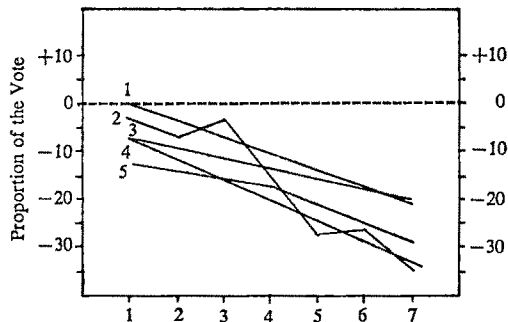
One case for the validity of the item is the pattern of the Wallace vote. The smooth, curvilinear increase in the Wallace vote in Figure 5 indicates that his supporters had no difficulty in interpreting the item. Second, as Figure 6 demonstrates, the vote on this item is quite similar to the vote on the other four items in this set of urban unrest and race issues. Appendix I presents the inter-item associations of this item with others in the race and urban unrest set.

the polar right, Wallace captured so many normally Republican votes that Nixon also fell short of his party's expected strength.

The 1968 SRC election study is rich in items on racial attitudes. In addition to the previous items on speed of movement on civil rights and urban unrest, other questions probe attitudes on segregation in general, attitudes on residential segregation in particular, and perceptions of Negro violence. Since all yield a consistent result under normal vote analysis, Figure 6 presents a composite picture of the vote on racial issues. Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote.

In sum, in an election in which many issues held the public's attention, race and urban attitudes were indisputably important. In competition with a number of salient issues, urban un-

Figure 6. Deviations from the Expected Democratic Vote on Five Racial and Urban Unrest Items.



Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote. The five lines represent the following items:

1. Moving too fast on civil rights. See Figure 1 for the item.
2. Urban unrest item. See Figure 5 for the item.
3. "During the past year or so, would you say that most of the actions Negroes have taken to get the things they want have been violent, or have most of these actions been peaceful?"
 1. Most have been peaceful. (318/217)
4. Some violent, some peaceful. (This category is omitted for want of a sufficient number of cases.)
7. Most have been violent. (1052/694)
4. "What about you: Are you in favor of desegregation, strict segregation, or something in between?"
 1. Desegregation. (539/399)
 4. In between. (689/462)
 7. Segregation. (232/131)
5. "Which of these statements would you agree with?"
 1. Negroes have a right to live wherever they can afford to, just like anybody else. (1042/733)
 4. Don't know; depends; can't decide. (147/94)
 7. White people have a right to keep Negroes out of their neighborhoods if they want to. (339/200)

rest ranks second, belief in segregation ranks fourth, and speed of movement on civil rights ranks sixth in the magnitude of their relationship to defections from the normal vote. Herein may be read the Wallace story. As Converse and others note, in both the North and the South, the Wallace vote came predominantly from Democrats. Nixon rather than Humphrey was the second preference for most of these Democratic Wallace voters. For a third it was at least their second consecutive defection from their national presidential ticket.¹⁶

In the South racial issues dominated all others in 1968. The items on belief in segregation, speed of movement on civil rights, and urban unrest rank first, second, and third, respectively, in their relation to short-term deviations from the normal party vote.¹⁷ Despite that fact the South also leads the nation in party polarization on racial issues, with Southern Republicans being located farther to the right of Southern Democrats than Northern Republicans are to the right of Northern Democrats.

Civil Liberties and Public Protest. It is instructive to compare the issues of race and civil rights with those of civil liberties and public protest. The protests against the war in Vietnam contributed to the chain of events leading to Johnson's renunciation; the riots in the streets of Chicago during the Democratic convention left the party demoralized.¹⁸ Thus, the protesters

¹⁶ Converse et al., "Continuity and Change in American Politics," p. 1091. Utilizing sample surveys conducted by the Opinion Research Corporation, Brody and his associates analyzed the effect on evaluations of the candidates in 1968 of urban unrest and immediate withdrawal vs. military victory items identical to those in Figures 3 and 5. Their findings are quite consistent with my own. Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, the Urban Crisis and the 1968 Presidential Election: A Preliminary Analysis," paper delivered at the American Sociological Association Meeting, 1969, pp. 1-32. Other analyses of race and the 1968 elections include Westbrook and Rusk, "Dimensions of Candidate Evaluation"; Seymour Martin Lipset and Earl Raab, "The Wallace Whitelash"; and Arthur G. Wolfe, "Challenges from the Right: The Basis of Voter Support for Wallace in 1968," paper delivered at the American Psychological Association Meeting, Washington, D.C., 1969, pp. 1-10.

¹⁷ The reader might wonder about contaminating interaction effects of sectionalism on many of these issues. Separate normal vote analyses were run for the South and non-South on every item. While the magnitude of the partisan and short-term components differed between the regions somewhat, the basic pattern of the components was remarkably similar on all the issues. That is, issues that were partisan in the non-South were also partisan in the South; defection from the normal vote occurred among the same attitude groups in both regions. This is further evidence for the contention of V. O. Key that, racial attitudes aside, the political atti-

may rightly call their effects major ones. But however much the protests may have cost Johnson and his party, they redounded against the protesters as well. Converse et al., and Robinson, underline the antipathy generated by the activities of the demonstrators. Even those who would say that we made a mistake in getting involved in Vietnam and that we ought to pull out immediately supported the actions of the Chicago police. Seventy per cent of this group rejected the suggestion that "too much force" was used by the police; 40 per cent of them would even say that "not enough force" was used to suppress the demonstrators.¹⁹ Wallace, of course, made these protesters the butt of his potent attacks. What then was the relation of these hostile reactions to the vote?

The 1968 election study includes a series of three items concerning the legitimacy of public protests. With respect to each form of protest the respondents are asked to state whether or not they approve its use. The series begins with "protest meetings and marches that are permitted by the local authorities" and ends with "sit-ins, mass meetings, and demonstrations," presuming "all other methods have failed." Figure 7 presents a normal vote analysis of these attitudes.

Surprisingly, the relationship of these issues to the vote is marginal—marginal at least in comparison with others. Of the 15 issues analyzed here, the items for legal meetings and marches, civil disobedience, and mass demonstrations rank respectively twelfth, fourteenth, and fifteenth in the magnitude of their relationships to defections from the normal vote. As one can see from Figure 7, Humphrey fared somewhat better among those who approved of public protest than among those who did not. In the main, however, he lost consistently across all categories of approval and disapproval of protest methods (the gap between the expected Democratic vote [ED] and his observed vote [OD]). The implication of this uniform loss is that other issues were responsible for his defeat.

The failure of civil liberties to surface as a short-term issue in 1968 parallels the fate of internal communism in the early 1950s. In 1952

the Republican party attacked the Democrats with the slogan of "Communism, Corruption and Korea." Yet, Campbell et al. found that only three per cent of the population mentioned in their evaluations of the candidates and parties the argument that the Democratic administration had been "soft on communism."²⁰ Similarly in a survey conducted in 1954, Stouffer reported that less than one per cent of the population said they were worried about the threat of Communists in the United States.²¹ This evidence from surveys is supported by Louis Bean's analyses of aggregate voting returns and Joseph McCarthy's campaigning. Bean concluded that McCarthy's campaigns in 13 states outside the South in 1952 had no effect on the presidential contest and, if anything, hurt the Republican senatorial candidates he had endorsed.²²

In all likelihood, the reason that these civil liberties and protest items are unrelated to defections from the normal vote is that they have rarely if ever been partisan issues. Democrats are slightly more willing to tolerate these acts than are Republicans. On the whole, however, the public speaks with a single voice against the protests. Not even 10 per cent of the college educated—the principal supporters of tolerance of civil liberties—approve, if asked, of mass demonstrations. Since attitudes on these valence issues are not linked to either parties or candidates, their impact does not extend to the vote itself, though the political system surely feels their force in other ways.

Social Welfare and the Federal Role: The Persistence of Traditional Issues. If the emerging issues of race, the cities, and the war were important to the 1968 election, what of the traditional issues of the post-Depression period? Since the realignment of 1928–36, much of the conflict between the parties has centered upon two broad issues: (1) What is the public responsibility to the private individual for the calamities that can befall him—from economic dislocation, sickness, unemployment, injury, indigence? (2) If there is a public responsibility in these matters, what level of government car-

tudes of Southerners are very similar to those of people outside the South. Key, *Public Opinion and American Democracy*, pp. 99–105.

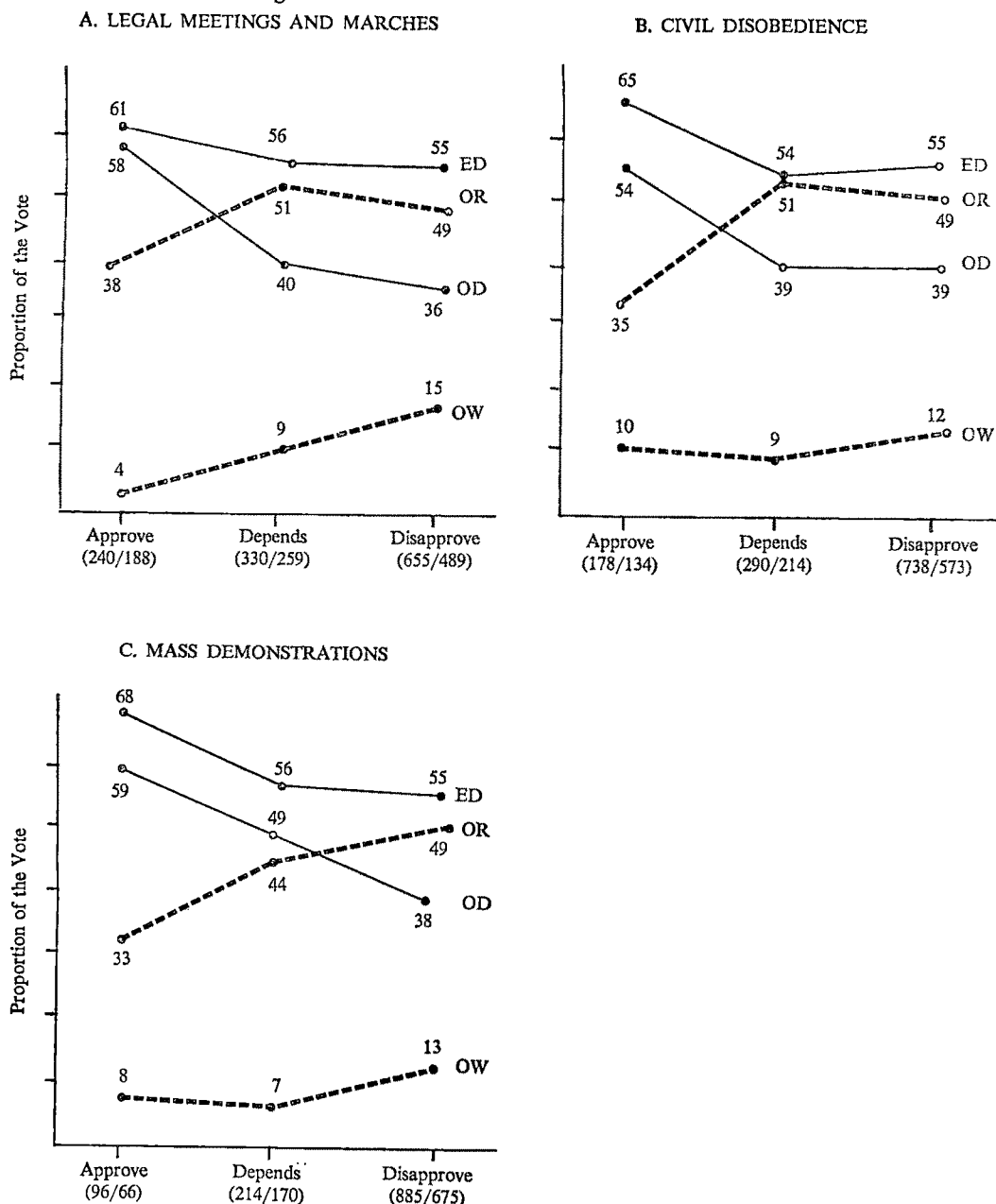
¹⁸ Theodore H. White, *The Making of the President 1968* (New York: Atheneum, 1969), Chaps. 4 and 9.

¹⁹ John P. Robinson, "Public Reaction to Political Protest: Chicago 1968," *Public Opinion Quarterly*, 24 (Spring, 1970), 1–9. Converse et al., "Continuity and Change in American Politics," p. 1087.

²⁰ Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954), p. 52.

²¹ Samuel A. Stouffer, *Communism, Conformity, and Civil Liberties* (Gloucester, Mass.: Peter Smith, 1963), p. 59.

²² Louis H. Bean, *Influences in the 1954 Mid-Term Elections* (Washington: Public Affairs Institute, 1954), p. 22. This conclusion is supported by Nelson W. Polsby, "Towards an Explanation of McCarthyism," *Political Studies*, 8 (Oct., 1960), 250–71.

Figure 7. The Normal Vote and Civil Liberties Issues

Item: "There are many possible ways for people to show their disapproval or disagreement with governmental policies and actions. I am going to describe three ways. We would like to know which ones you approve of as ways of showing dissatisfaction with the government, and which ones you disapprove of."

A. "How about taking part in protest meetings or marches that are permitted by the local authorities?"

B. "How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law?"

C. "Suppose all other methods have failed and the person decides to try to stop the government from going about its usual activities with sit-ins, mass meetings, demonstrations, and things like that?"

ries its burden? The answers of the majority Democratic party have been that there is a proper governmental role in these matters, a role of the national government. Out of this majority have come federal programs such as farm price supports, old age and survivors' insurance, unemployment compensation, and Medicare. The Republican party has, for the most part, resisted the expansion of the federal role at each stage. Because of this continuing conflict, these issues still stand out as the major, long-term issues dividing the parties' adherents.

Figure 8 underlines the long-term, partisan nature of these federal role issues. Again, the measure of the partisan component of an issue is the degree of change in the expected Democratic vote across various positions on the issue (line ED in Figure 8). For the four issues, the difference in the expected Democratic vote between those who would expand the federal government's role and those who would not averages almost 20 percentage points. From a 1958 sample, McClosky observed that the mass membership of the two parties did not differ on issues.²³ This was clearly not the case for federal role issues a decade later.

One might predict the partisan component of these traditional issues; more surprising is their uniformly high relationship to defections from the normal vote. Note, in Figure 8, Humphrey's consistent losses among voters endorsing a limited role for federal activities. In every case the difference between the expected (ED) and the observed Democratic vote (OD) exceeds 20 percentage points. In contrast, Humphrey did quite well among those supporters of a broad scope of federal activities—barely falling short of his expected strength and, in every case, capturing a comfortable majority of the vote. In an election in which race, law and order, and the war loomed on the scene, the more traditional issues seemed reluctant to leave quietly.

The presence of the short-run component of the federal role issues suggests the possibility of spurious association. The conservative positions on these federal role-social welfare issues are associated with four other positions: opposition to civil rights, use of force in dealing with urban unrest, escalation of the war, and a harsh evaluation of Johnson's performance as president.²⁴ All of the latter issues were highly re-

lated to short-term defections from the normal vote in 1968. To check against the possibility that the federal role issues manifest a short-term component only because of their association with the others, separate normal vote analyses of the federal role issues were run within categories of the other issues. Uniformly, the short-term component of the federal role issues remained as strong as in the uncontrolled case.

In sum, the traditional set of attitudes relating to the scope of the federal government persisted as a significant force in the 1968 election. In an election supposedly dominated by urban unrest and the war, why was this so? Perhaps one clue lies in the votes that Humphrey did *not* lose rather than those he did.

In late September, Humphrey's cause looked hopeless. A Gallup poll showed him trailing Nixon by a margin of 43 to 28 per cent and leading Wallace by only seven percentage points. Scarcely a month later, he lost the election by less than one per cent of the popular vote. As Theodore White chronicles the last month of the campaign, Humphrey attacked the Republicans with an echo of the 1964 campaign, the alleged Republican indifference to the traditional issues of social security and social welfare.²⁵

Meanwhile, the AFL/CIO had mounted a two-pronged assault on Wallace and Nixon. First, the AFL/CIO carried a message—Wallace and Nixon were no friends of labor. Second, COPE took over much of the effort of mobilizing the electorate, particularly the Black vote. In White's words,

The dimension of the AFL/CIO effort, unprecedented in American history, can be caught only by its final summary figures; the ultimate registration, by labor's efforts, of 4.6 million voters; the printing and distribution of 55 million pamphlets and leaflets out of Washington and 60 million more from local unions; telephone banks in 638 localities using 8,055 telephones, manned by 24,611 union men and women and their families; some 72,225 house-to-house canvassers; and, on election day, 94,457 volunteers serving as car-poolers, materials-distributors, baby-sitters, poll-watchers, telephoners.²⁶

A cross-sectional sample such as this one cannot establish who changed their minds during that last month of the campaign. Yet, the appeals of Humphrey were the traditional ones of the old Democratic coalition. Herein may rest the importance of the short-term effects of social welfare-federal role issues. Humphrey and organized labor used them to counter the new issues—successfully so among those Democrats

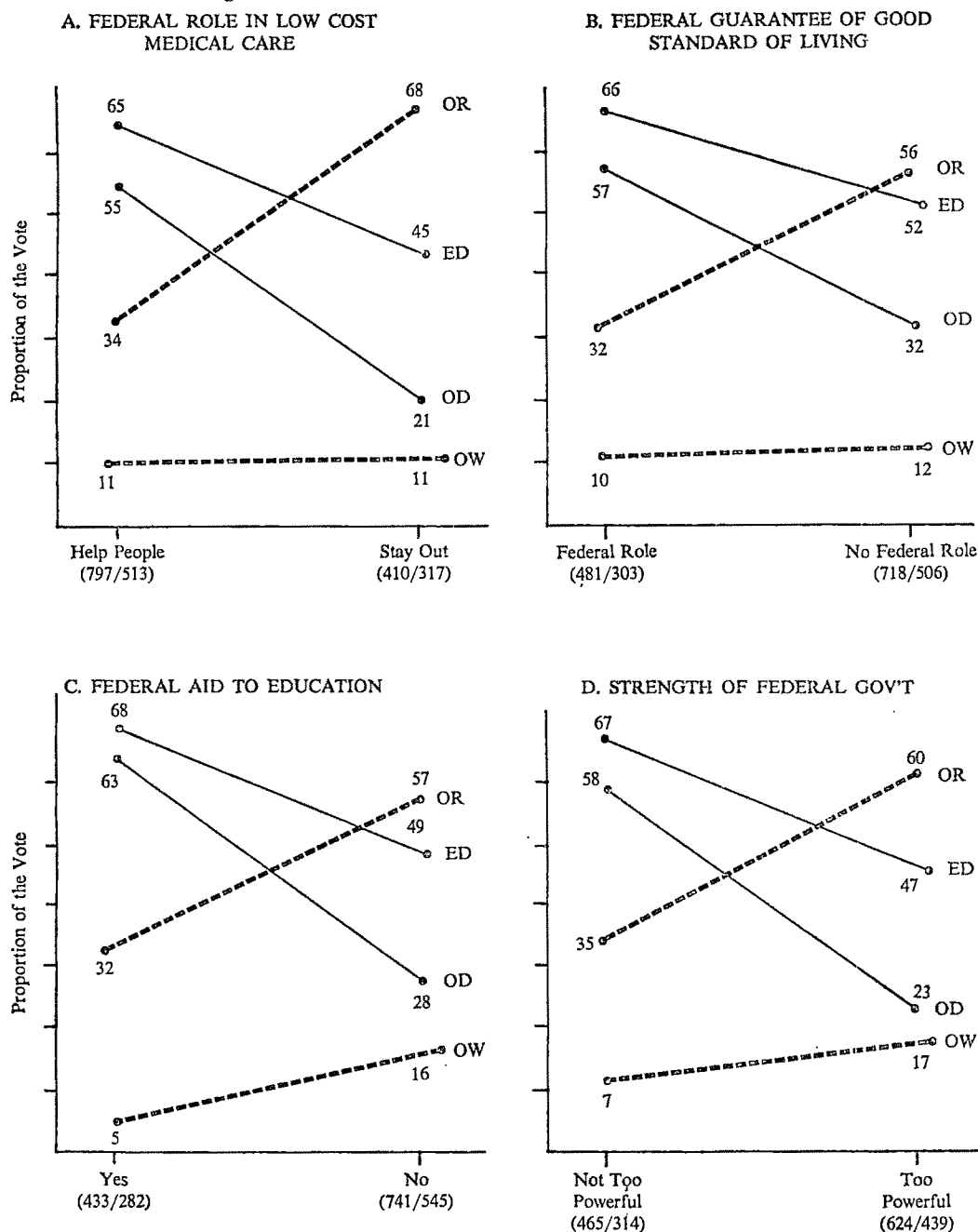
²³ McClosky et al., "Issue Conflict and Consensus Among Party Leaders and Followers," pp. 418-19. McClosky did find some differences between the party followings on "bread and butter" issues such as farm prices, business regulation, taxes, and minimum wages. These differences are minor compared to the differences presented in Figure 8, however.

²⁴ See Figures 1, 5, 2, and 9 respectively for the exact wordings of the items.

²⁵ White, *Making of the President, 1968*, p. 371.

²⁶ White, p. 365.

Figure 8. The Normal Vote and Federal Role in Social Welfare*



* Because of small cell sizes the intermediate response categories, "it depends" or "both," are omitted from this figure.

Item: A. "Some say the government in Washington ought to help people get doctors and hospital care at low cost; others say the government should not get into this . . . What is your position?"

B. "In general, some people feel the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on his own . . . What is your position?"

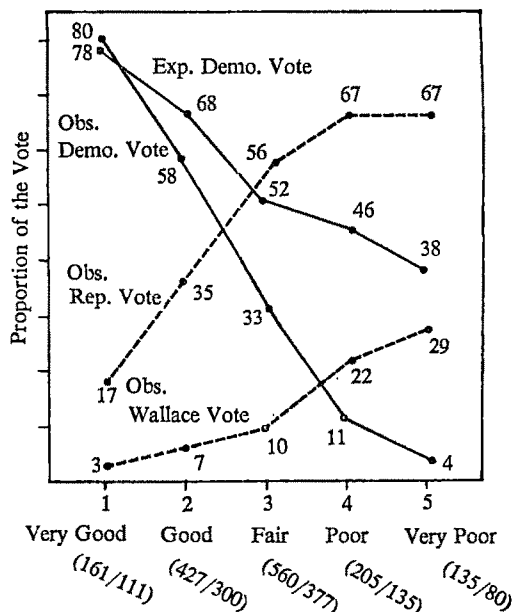
C. "Some people think the government in Washington should help towns and cities provide education for grade and high school children; others think this should be handled by the states and local communities . . . Which are you in favor of?"

D. "Some people are afraid the government in Washington is getting too powerful for the good of the country and the individual person. Others feel the government in Washington is not getting too strong for the good of the country . . . What is your feeling?"

for whom the traditional social welfare issues remained important.²⁷ In this respect the trend in 1968 bore a notable similarity to Truman's late surge in 1948. In their Elmira study Berelson et al. document the basis of Truman's "Fair Deal Rally." They found the swing to Truman to be sharpest among those for whom class issues were most salient.²⁸

Evaluations of Johnson's Performance as President: Humphrey in Johnson's Shadow. The final issue is the public's evaluation of Johnson's performance as president. The identification of Humphrey with Johnson in the public's mind proved to be one of the most intractable of Humphrey's problems. Politically, people saw them as a single person; indeed, their judgment of Humphrey correlated with that of Johnson at a level of .70, a figure that exceeded the association of any of the candidates with their own running mates.²⁹ Throughout the month of September, observers wrote of the price that Humphrey seemed to be paying for the unpopularity of Johnson and the war. According to Theodore White, he was heckled at nearly every speaking engagement during that month. The Salt Lake City speech of September 30 was Humphrey's major attempt to assert his independence of Johnson's policies. While the speech may have given him some respite from dis-sidence, his association with Johnson appeared to remain a major burden. The consequence of Humphrey's perceived identity with Johnson is graphically illustrated in Figure 9. People who characterized Johnson's performance as "very good" gave Humphrey even more than his expected share of the vote. From that point Humphrey's vote plummeted in an almost linear fashion as the severity of judgment of Johnson mounted. From those who judged Johnson's performance as "poor" or "very poor," Humphrey received only a small fraction of the normal Democratic strength. Of all the issues considered in this analysis, this one bears the great-

Figure 9. The Normal Vote and Evaluation of LBJ's Performance



Item: "In general, how do you feel about how President Johnson has done his job. Would you rate his handling of America's problems over the past four years as very good, good, fair, poor, or very poor?"

est relationship to defections from the normal vote.

Humphrey's fate may represent a case of a more general phenomenon—that the unpopularity of leaders transfers to their successors more readily than does their popularity. For example, those who thought well of Johnson's performance outnumbered those who thought poorly of it by a comfortable margin. However, Johnson's admirers gave Humphrey only a normal Democratic vote, while his detractors voted heavily against him.

Indeed, it may be a contribution to party responsibility and competition that blame's retribution survives credit's reward.³⁰ Parties are more likely to be sensitive to public opinion, if they believe popularity dissipates quickly, while political blunders endure in the public mind. In the process of governing, the party in power collects enemies faster than friends.

²⁷ Kevin Phillips's view is the same. "Fears that a Republican administration would undermine Social Security, Medicare, collective bargaining, and aid to education played a major part in keeping socially conservative blue-collar workers and senior citizens loyal to the 1968 Democratic candidate." Kevin P. Phillips, *The Emerging Republican Majority* (Rochelle, N.Y.: Arlington House, 1969), p. 464.

²⁸ Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: The University of Chicago Press, 1954), Chap. 12.

²⁹ The correlation is between evaluations of Johnson and Humphrey on the SRC "feeling thermometer." Weisburg and Rusk, "Dimensions of Candidate Evaluation," p. 1173.

³⁰ We might call this the Barkley Effect, after Alben Barkley's tale of a constituent he had befriended many times who expressed to Barkley an intention to vote against Barkley in a forthcoming election. When Barkley reminded him of the many favors he had given the man in the past, the constituent responded, "Yeah, but what in hell have you done for me lately?" The story is quoted in Donald Matthews, *U.S. Senators and Their World* (New York: Vintage Books, 1960), p. 218.

In this vein the authors of *The American Voter* rely on the persistence of negative attitudes to explain the inexorable decline of majorities.³¹ Thus, we have an explanation of the restoring forces that seem to create an equilibrium among competing American parties over time.³²

Popular Control of Public Policy

Thus far, I have described the relationship of a group of issues to the 1968 vote. The strongest of these were the public's evaluation of Johnson's performance as president, reaction to the Vietnam war, and attitudes toward race and urban unrest. At this time and place, a set of conditions converged creating the unusual in politics—a cluster of issues so salient and crystallized that the popular vote for the candidate of the majority Democratic party barely exceeded 40 per cent.

In a more speculative vein, it is interesting to ponder the second major inquiry of the paper—the relationship of public attitudes to popular control of governmental policy. The problem is posed by V. O. Key's classic question: If governmental policy corrodes the vitality of a society, who is the villain—the public or its political leaders?³³ To conclude that it is the public requires evidence, Key argues, that the public severely limits the discretion of policy makers. Yet, from survey data Key notes that substantial portions of the public are unconcerned

and uninformed about most concrete policies of government. Whatever limits opinion fixes, governments still enjoy wide latitude in the determination of whether to act, when to act, and what exactly to do. Thus Key concludes, leaders cannot excuse their actions by pleading the hampering restrictions of public opinion.

Key's formulation of the problem of responsibility for policy has its difficulties. One is the factual problem of distinguishing different degrees on a continuum of popular constraints on leaders' policy options. The difficulty of this judgment is lessened if the continuum is divided into three broad categories: (1) issues on which people exercise no important constraints and, thus, have no voice; (2) issues on which people have voice, but permit political leaders a wide array of policy options; and (3) issues on which voters do severely restrict the options of leaders.

Regardless of the extent of popular control over policy, a second, ethical issue remains. If we can agree that the public should not be held responsible for policy when leaders possess an array of options (categories 1 and 2), what of the case in which leaders' options are severely restricted? Many would surely argue that leaders are morally responsible for public policies no matter how much the public seems to force their hands. I will not take up this issue except to assert that severe public constraints on policy options is a *necessary* condition for holding the public responsible for a policy. Whether or not that condition is sufficient remains problematic. Caveats considered, let us turn to examples in the 1968 election of the three categories of popular control over policy.

(1) On many, perhaps most, governmental policies, the public as an electorate has *no voice*. On these policies, only the few have influence because the many have no interest and no information. Issues that do not impinge upon people's economic needs or ego defenses fall into this category, as do, no doubt, policies of abstruse complexity. Foreign policies in peace time, strategic decisions, esoteric regulatory rules—these sorts of issues are not likely to engage popular attention even though they may have a substantial impact on people's lives in the long run. Certainly political leaders must bear full responsibility for failures in policy in these areas.

(2) On other issues the public does find its voice, but it still leaves political leaders with so many options that leaders retain responsibility for the success of their choices. This interpretation of the relationship of popular preference and public policy is consistent with the views of

³¹ Campbell et al., *The American Voter*, pp. 554–55.

³² For further empirical evidence of party equilibrium, see the list compiled by Stokes and Iverson, "On the Existence of Forces Restoring Party Competition," in Campbell et al., *Elections and the Political Order*, pp. 180–93. For other explanations of party equilibrium, see the list compiled by Stokes and Iverson, p. 181. See also the discussion of E. E. Schattschneider, *Party Government* (New York: Holt, Rinehart and Winston, 1942) pp. 93–98. Schattschneider explains the recuperative powers of the second major party in terms of its monopoly of opposition and the disadvantages for the party in power of a large majority. This latter point is one of the early statements of the size principle derived by William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), pp. 32–76.

³³ V. O. Key, Jr., "Public Opinion and the Decay of Democracy," *Virginia Quarterly Review*, 37 (Autumn, 1961), 481–94. A shorter presentation of the argument appears in his book, *Public Opinion and American Democracy*, pp. 555–58. One might wonder what Key means by the failure of public policy: is failure defined by some objective standard or is it subjectively defined by people? This raises all the thorny problems of an interpersonal comparison of utilities and a social welfare function. Key seems to have some objective standard in mind, although he does not suggest who determines failure in any particular instance. I am more interested here in the issue of who influences policy making than the evaluation of the merits of policy. For the purposes of this discussion, evaluation is presumed to be the prerogative of each voter.

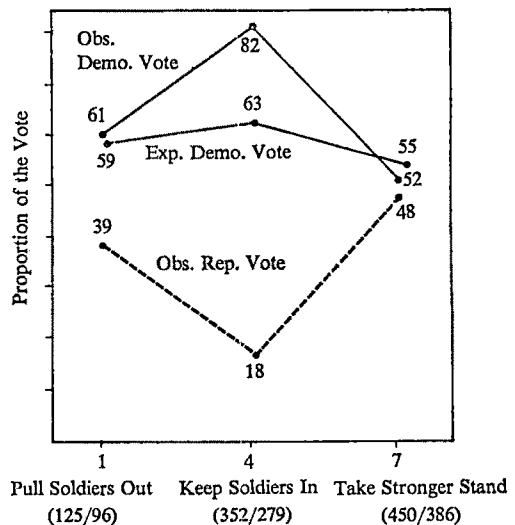
Key and many other voting analysts.³⁴ In the main, policy initiatives lie with the leaders, not voters. After a policy is enacted, voters make a retrospective evaluation of its success. The electorate's influence over such a policy is the leaders' anticipation of voters' reactions in the following election,³⁵ when voters will ask themselves, "How did the party in office do, and how likely is it that the opposition party would have done better?" An archetypal example is Humphrey's loss of support among voters who ranked Johnson's presidential performance as poor. It is in this sense that Key remarked, "The vocabulary of the people consists mainly of the words 'yes' and 'no'; and at times one cannot be certain which word is being uttered."³⁶

Vietnam may be another example of an issue on which the electorate retrospectively said "no" to governmental policy. This interpretation is necessarily tentative because people make voting decisions across a range of issues. As a result, a majority may vote for a man in spite of his position on an issue, not because of it.³⁷ Thus, even when one limits the interpretation of the vote to "yes" and "no," it is difficult to infer conclusively what the judgment is on any particular issue. On the matter of Vietnam policy alternatives, one can only speculate

about the nature of constraints posed by past and future elections.

On the eve of the 1964 election, only 20,000 American soldiers were in Vietnam, and President Johnson was campaigning on pledges against fighting Asian wars with American boys. What could the 1964 vote have told him about popular support for various war options? In brief, it could have told him anything he cared to believe.

Figure 10. The Normal Vote and Escalation/De-escalation in 1964



Item: "Which of the following do you think we should do now in Vietnam?"

1. Pull out of Vietnam entirely.
2. Keep our soldiers in Vietnam but try to end the fighting.
7. Take a stronger stand even if it means invading North Vietnam.

Figure 10 presents a normal vote analysis of the escalation/de-escalation issue in 1964. The data could be interpreted as support for "pull[ing] out of Vietnam entirely." Johnson received 62 per cent of the votes from the proponents of this option—a substantial majority, and three percentage points better than a normal vote would have predicted. On the other hand, the data also show strong support for "keep[ing] our soldiers in Vietnam but try[ing] to end the fighting." From this group he received a startling 82 per cent of the vote, 18 percentage points more than a normal vote would have predicted. Finally, the data also reveal popular support for the option of "tak[ing] a stronger stand even if it means invading North Vietnam." Although he failed to win the expected Democratic vote

³⁴ This presentation owes much to a lucid discussion by Gerald M. Pomper, *Elections in America: Control and Influence in Democratic Politics* (New York: Dodd, Mead, 1968), Chap. 10. To use Tingsten's phrase (if not all of the rationale that produced it), "The public . . . [is a] consumer rather than [a] producer of political demands and candidates." Herbert Tingsten, *The Problem of Democracy* (Totowa, N.J.: The Bedminster Press, 1965), p. 198. See also, Key, *Public Opinion and American Democracy*, pp. 472-78; Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown 1964), Chap. 7; Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), Chap. 3; and R. J. Benewick, A. H. Birch, J. G. Blumler, and Alison Ewbank, "The Floating Voter and the Liberal View of Representation," *Political Studies*, 17 (June, 1969), 177-195.

³⁵ Friedrich's Rule of Anticipated Reactions. Carl J. Friedrich, *Man and His Government* (New York: McGraw-Hill, 1963), pp. 199-215.

³⁶ V. O. Key, Jr., *Politics, Parties, and Pressure Groups*, 5th ed. (New York: Crowell, 1964), p. 544. The quote also appears in Pomper, *Elections in America*, p. 252.

³⁷ A party could win an electoral majority by systematically taking positions favored by a minority. Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: The University of Chicago Press, 1956), pp. 124-31. This situation Downs calls a "coalition of minorities." It stands as a contrast to a "passionate majority," which occurs only when there is a consensus on which issues are important as well as a consensus of preferences on the issues. Downs, *Economic Theory of Democracy*, pp. 64-69.

within this group, he still claimed a majority. And, it should be noted, the advocates of escalation easily numbered the largest of the war opinion groups. Looking at the 1964 data and noting Pomper's evidence that 1964 Johnson voters were still supporting Johnson's policies in 1966, one must conclude, as Pomper did, that Johnson did not violate an electoral mandate in spite of the fact that he ignored his own campaign pledges.³⁸ The 1964 election foreclosed none of Johnson's war options. He and other political leaders formulated the escalation policy and rightly should have borne full electoral responsibility for it.

By 1968, escalation had changed the context of the options confronting the electorate. Rather than 20,000 men in Vietnam, there were 530,000. Sixteen thousand men had died, and America's prestige was at stake. As we have seen, the public's reaction to a continuation of the existing war policy was negative. Humphrey won a majority of the votes of people endorsing a centrist, status quo alternative: "keep our soldiers in Vietnam but try to end the fighting." He fared less well among those who would pull out entirely; he fared disastrously among those who would escalate the war still further, a group of voters who gave both Wallace and Nixon the greatest proportions they received from any Vietnam attitude category. (See Figure 2.) The common thread of the views of those who deserted Humphrey—people who would withdraw rapidly or who would sharply escalate the fighting—would seem to be a desire to end the engagement quickly. As in the Korean war, an official Vietnam policy of a war of limited aims and lengthy duration confronted a public unwilling to support war on those terms.³⁹

Though one cannot marshal evidence about events that never happened, the Johnson Administration appeared to lose more electoral support by its pursuit of an extended limited war than it would have lost either by a policy of withdrawal (even after the buildup of men) or by a precipitous escalation of the conflict. This view of the electoral dangers of limited conflict is consistent with the views of Key, Kahn, and Brzezinski and Huntington.⁴⁰ The

public does not necessarily *prescribe* a particular policy, but it can *proscribe* a policy—e.g., the option of extended limited war. Even after the 1968 election, Rosenberg, Verba, and Converse argued that policy makers retained a wide array of options for ending the war.⁴¹

... the President is not particularly pressured by public opinion to engage in one kind of policy rather than another—to escalate or to de-escalate the war. Rather, the public would support a variety of initiatives. What the public wants is an end to the war. Thus it will judge the policies of the President retrospectively in terms of how successful they are in this direction.

If political leaders possessed a wide array of options in Vietnam, then are there issues on which the electorate does severely limit the options of political leaders? If there are, what must the structure of political attitudes on such an issue be? For this close relationship of opinion to policy to exist, the following conditions are surely necessary: (1) A substantial number of people must have intense views about specific policy alternatives. (2) People must have information about the views of competing candidates on the issue. (3) These attitudes must be fairly impervious to short-run manipulation by political leaders. (4) The balance of opinion on the issue must be such that the winning candidate's choice of an issue position significantly

cal Power: USA/USSR (New York: Viking Press, 1964), p. 414. These and other writers are discussed in a good article on this issue, Kenneth N. Waltz, "Electoral Punishment and Foreign Policy Crises," in Rosenau, pp. 263–294. Waltz does not agree with the general conclusion that the public will not support a limited war. Rather, he blames the lack of broad-based support for the Korean War on the failure of the Truman administration to explain its policy adequately to the public because of his administration's own uncertainty over its goals (pp. 279–80). Waltz does not, however, either have any direct evidence about the impact of opinion on votes or any indirect evidence about the intensity of opinion on these issues. Lacking this type of information, one cannot easily judge how governmental policy will affect votes or how votes will affect policy. This same problem confronts those who have found permissive majorities supporting the Vietnam war. See, for example, Sidney Verba, Richard A. Brody, Edwin B. Parker, Norman H. Nie, Nelson W. Polsby, Paul Ekman, and Gordon S. Black, "Public Opinion and the War in Vietnam," *American Political Science Review*, 61 (June, 1967), 321, and William R. Caspary, "The 'Mood Theory': A Study of Public Opinion and Foreign Policy," *American Political Science Review*, 64 (June, 1970), 546. For a sobering discussion of the difficulties of interpreting the significance of opinion polls and support for the Vietnam War, see Philip E. Converse and Howard Schuman, "'Silent Majorities' and the Vietnam War," *Scientific American*, 222 (June, 1970), 17–25.

⁴¹ Rosenberg, Converse, and Verba, *Vietnam and the Silent Majority*, p. 38.

³⁸ Pomper, *Elections in America*, p. 252.

³⁹ See the discussion of the electoral impact of the Korean War by Warren E. Miller, "Voting and Foreign Policy," in *Domestic Sources of Foreign Policy*, ed. James Rosenau, (New York: The Free Press, 1967), pp. 213–30.

⁴⁰ Key, *Public Opinion and American Democracy*, p. 261; Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1960), p. 418; Zbigniew Brzezinski and Samuel P. Huntington, *Politi-*

affects his probability of being elected. Clearly, these are necessary, not sufficient, conditions for influencing policy through the electoral process. There are other channels for influencing policy in our system, all with their own conditions.

What contemporary issues might satisfy these four conditions? The issues of race and public order seem the obvious candidates. Even in the quiescent period of the late 1950s, there was evidence of a close relationship between opinion and policy on racial attitudes. Miller and Stokes, for instance, found a correlation of .65 between congressional roll-call votes and constituency opinion on civil rights issues.⁴² Furthermore, the link between the two was the representatives' inclination to vote their *perceptions* of their constituencies' opinion; they were not simply voting their own attitudes, which happened to coincide with the majority opinion in their districts. Let us see how the race issue in the elections of 1964 and 1968 might satisfy the four conditions:

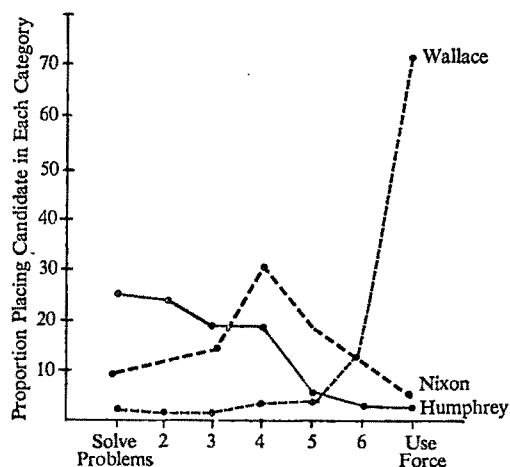
(1) *A substantial number of people must have intense views about specific policy alternatives.* Survey questions are rarely framed in a manner that presents reliable evidence on either the direction or the intensity of views on specific policies. One exception in the 1964 SRC election study is a set of items probing knowledge of and support for the 1964 Public Accommodations Law. All but 23 per cent said they knew of the new law (and as shown below, their knowledge of the candidates' stands indicates people were not simply giving the socially desirable answer). Of those having an opinion on the law, 90 per cent said their minds were made up. No such questions were asked in the SRC election studies on the 1965 Voting Rights Law or the 1968 Open Housing Law. Nevertheless, we can be reasonably confident that the controversies over their passage served to crystalize intense views on these policies. Negroes voted more than 90 per cent Democratic in 1964 and 1968, a display of cohesion that reflects the interests they saw at stake in the two elections.⁴³

⁴² Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," in Campbell et al., *Elections and the Political Order*, p. 359.

⁴³ Actually, the 1964 SRC sample does not include a single Negro Goldwater voter. Fred I. Greenstein, *The American Party System and the American People*, 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 26. In France, religion has been an issue having a similar degree of crystallization and polarization. See Philip E. Converse and Georges Dupeux, "Politicization of the Electorate in France and the United States," in Campbell et al., *Elections and the Political Order*, pp. 289-90.

(2) *People must have information about the views of competing candidates on the issue.* In 1964 the level of information on the public accommodations law was remarkably high. Of the 77 per cent who said they had heard of the law, 95 per cent knew Johnson favored it and 84 per cent knew Goldwater opposed it. Even in 1968, when differences between the Republican and the Democratic candidates were less striking, voters had little trouble sorting out the candidates' stands on racial issues. Figure 11 presents a distribution of people's perceptions of the candidates' positions on the urban unrest issue. The modal perceptions of Humphrey, Nixon, and Wallace were distinctly left, center, and right, respectively—perceptions that were quite accurate in view of the different positions of the parties and the candidates on issues of race and urban violence.⁴⁴ If one limits the sample to those who viewed the race issue as important in determining their vote, the distinctness of the perceived differences among the candidates is even more pronounced.

Figure 11. Distribution of Perceptions of Candidates' Stands on Urban Unrest



Item: See Figure 5.

(3) *Attitudes on the issue must be fairly impervious to short-run manipulation by elites.* Almost every survey of the literature on mass persuasion emphasizes the difficulty of chan-

⁴⁴ Sources of platform and campaign positions are White, *Making of the President 1968*, Lewis Chester, Godfrey Hodgson, and Bruce Page, *An American Melodrama* (New York: Viking Press, 1969); *Congressional Quarterly Almanac*, pp. 942-1046; and *The Presidential Nominating Convention: 1968* (Washington: Congressional Quarterly, 1968).

ging popular attitudes when attitudes are well formed and when the time available to the elites is reasonably short (as in a campaign).⁴⁵ Certainly, racial attitudes should be among the most difficult for opinion leaders to influence, for they are learned early in life and are often central to people's ego defenses.⁴⁶ Thus, we can be reasonably sure that if we discover a close relationship between popular attitudes and the enactment of public policies on race issues, the link cannot be explained away by arguing that political leaders persuaded voters to support policies the voters previously resisted.

(4) *The balance of opinion on the issue must be such that the winning candidate's choice of an issue position significantly affects his probability of being elected.* It is easier to state this condition than to present evidence appropriate to its test. In the first place, the importance of a particular issue in an election differs for each candidate (for example, whether, in Downs' terms, a candidate's victory is based on a "coalition of minorities" or on a "passionate majority").⁴⁷ One illustration of this problem is the difference between the importance of the race issue for Nixon and Humphrey in 1968. Nixon won the election even though he lost more than 90 per cent of the Negro vote. Presumably, Humphrey needed all of those votes to have had any prospect of victory. In the second place, we have no analytical technique for determining the effect of alternative issue positions on each candidate's electoral coalition.⁴⁸ Our only approach to the analysis of the fourth condition is an indirect one—examining the statistical relationship of beliefs on issues to votes.

In 1964 the relationships between civil rights issues and votes were substantial. The gamma association of the vote with support for the public accommodations law was .57; with the establishment of a fair employment practices commission for Negroes, .47; with views on federal enforcement for school integration, .37.

⁴⁵ Joseph T. Klapper, *The Effects of Mass Communications* (Glencoe: Free Press, 1960).

⁴⁶ The literature on this subject is mammoth. See Frank R. Westie, "Race and Ethnic Relations," in *Handbook of Modern Sociology*, ed. Robert E. L. Faris, (Chicago: Rand McNally, 1964), pp. 576-618.

⁴⁷ Downs, *Economic Theory of Democracy*, pp. 64-69.

⁴⁸ A pioneering attempt to measure the impact of issue positions on votes is found in Ithiel de Sola Pool, Robert P. Abelson, and Samuel Popkin, *Candidates, Issues, and Strategies: A Computer Simulation of the 1960 and 1964 Presidential Elections* (Cambridge: The M.I.T. Press, 1965). No one has completed a rigorous development of the technique, however.

These links between civil rights positions and votes continued in 1968, as I argued in the first section of this paper.

The position that racial attitudes are closely related to public policy involves an apparent contradiction. If Humphrey's losses in 1968 were statistically related to racial attitudes, why did Congress in that same year pass an open housing law covering 80 per cent of all housing units? One answer is that there is no survey evidence of an increase in the number of white people opposed to traditional civil rights goals such as public accommodations, voting rights, and open housing.⁴⁹ There is greater fear of Negro crime and urban violence. This attitude is mirrored in the fact that the open housing law contains an anti-riot section.⁵⁰ Most people apparently believe that Negroes now enjoy legal equality and that the remaining social and economic inequalities are the responsibility of Negroes themselves.⁵¹

The evidence on racial attitudes and civil rights laws is undoubtedly insufficient to support a contention that Congress scarcely had time to leap to the lead of a mob bent on liberal changes in civil rights laws. On the other hand, it is so commonly assumed that changes in public policy have preceded changes in public opinion on civil rights issues that a second look is warranted.⁵² The sum of the evidence indicates that racial issues were voting issues in 1964 and 1968, and that majorities of the voters supported the major civil rights laws passed in those years (though we note the warning of Converse and Schuman that the wording of an item has a major effect on which response category attracts majority support).⁵³ In my view the following argument merits attention: Congress ran few risks in its support for civil rights laws in the 1960s—not because an apathetic public paid little attention to policy proposals,

⁴⁹ Converse et al., "Continuity and Change in American Politics," p. 1086; Herbert H. Hyman and Paul B. Sheatsley, "Attitudes Toward Desegregation," *Scientific American*, 211 (July, 1964), 16-23; Paul B. Sheatsley, "White Attitudes Towards the Negro," *Daedalus*, 95 (Winter, 1966), 217-38. Perhaps a proper interpretation of the backlash is that racial opinions have become increasingly salient for the declining numbers of people holding prejudiced attitudes.

⁵⁰ *Congressional Quarterly Almanac*, pp. 152-68. This report contains a good description of the act and an account of the politics of its enactment.

⁵¹ *Gallup Opinion Index* (July, 1968), 19-20.

⁵² See, for example, Sheatsley, "White Attitudes Towards the Negro," p. 221. This position is rarely stated explicitly in the literature, but it is implicit in much of the evidence that political, business, and social leaders are more liberal than the public on civil rights issues.

⁵³ Converse and Schuman, "Silent Majorities' and the Vietnam War," pp. 17-25.

but because on these issues Congress carefully stayed within the relatively narrow constraints posed by a watchful electorate.

The time to test the argument on existing civil rights laws is gone, for we cannot reconstruct a public past. Yet, determining the linkage between public opinion and policy is so critical a problem that it behooves us to ask policy questions of the public. We need to ask more than general questions about political attitudes. We need to know what people think about particular policies—the levels of information, intensity, and support for specific options. Without succumbing to hopeless naiveté about the interest and information of the electorate, the opinion-policy linkage on the issues of race and public order is evidence for the rich payoff of such an approach.

Summary and Conclusions

Converse developed the concept of the normal vote in order to interpret the force of the religious issue in 1960. His challenge was to analyze voting changes between 1956 and 1960 without using 1956, a deviating election, as a baseline for the measurement of change. The normal vote, an empirical estimation of an abstract concept, became the baseline, and the impact of the religious issue was measured as a deviation from the normal vote.

The logic of the normal vote presumes that the force of issues constantly changes. This logic has been too often ignored by those who assume the immutability of political behavior. The electorate of 1956 was presented in *The American Voter* as being little moved by ideology or public issues. But 1956 was a deviating election in a quiescent political era, and it should be of little surprise to find a minimal

amount of issue voting in that apolitical period. The importance of issues in 1968 stands in striking contrast to 1956. Beliefs about Vietnam, race and urban unrest, and Johnson's performance as president were all highly related to the vote in 1968. Time has told us of the unusual nature of the 1956 election. Undoubtedly, the future will reveal the atypicality of 1968, as American parties seemed to be in a stage of realignment or disintegration. Surely we should expect issues to be more important in these times than in periods of stability in party strength. After what appears to be a transition period, issue voting may once again decline to the level of the 1950s.

A second, more speculative argument of the paper is that issues similar in the magnitude of their relationship to the vote can pose very different restrictions on the discretion of leaders in policy making. On some issues the electorate exercises no effective constraints on leaders' policy choices. On others, the electorate permits political leaders a wide array of options at the time of the adoption of policy, while passing a retrospective judgment on such choices in subsequent elections. It is argued that the issues of Vietnam and Johnson's performance as president are examples of this type of public control of policy. Finally, there may be issues on which the public rather severely limits the options of leaders at the time of the adoption of policy. It is difficult, surely, to amass the evidence necessary to determine which issues could properly be placed into this final category. If any issues of 1964 and 1968 meet the tests, race and public order do. It may be, then, that the electorate must assume final responsibility for the success with which the United States solves its racial problem.

Appendix I. Inter-Association Matrix of 15 Issues

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1		17	16	16	25	17	13	12	17	13	00	04	07	01	03
2	30		35	32	26	14	06	03	04	13	13	12	07	08	13
3	24	59		28	23	25	15	11	17	22	14	14	16	15	23
4	27	58	47		32	25	22	14	23	22	13	12	18	11	15
5	42	46	39	56		19	15	12	16	14	09	09	13	07	11
6	31	29	43	45	36		43	26	35	27	14	14	29	16	26
7	28	13	30	43	31	72		26	35	19	13	15	34	17	25
8	23	06	21	29	24	58	65		47	14	03	06	22	10	14
9	24	07	29	41	28	60	67	77		24	06	11	26	15	17
10	17	21	29	34	21	42	36	25	31		16	24	18	14	20
11	00	23	23	22	15	24	25	06	09	20		43	11	06	15
12	05	19	19	19	14	22	29	11	15	29	53		16	12	14
13	10	13	27	31	23	48	59	45	42	24	17	21		28	36
14	01	16	25	20	13	28	33	22	26	21	10	17	45		29
15	08	29	42	30	25	49	50	37	34	39	29	27	61	52	

The upper diagonal contains the Kendall Tau; the lower diagonal contains the Goodman-Kruskal Gamma. All associations are positive if the left position is defined as favoring Johnson, a strong federal role in social welfare, civil rights, de-escalation of the Vietnam war, and civil liberties. The items are the following:

1. LBJ's performance. Fig. 9.

2. Medical care. Fig. 8.

3. Standard of living. Fig. 8.

4. Aid to education. Fig. 8.

5. Power of government. Fig. 8.

6. Speed of civil rights. Fig. 1.

7. Negro violence. Fig. 6.

8. Residential segregation. Fig. 6.
9. Segregation in general. Fig. 6.

10. Urban unrest. Fig. 5.

11. Escalation/de-escalation. Fig. 2.

12. Immediate withdrawal/military victory. Fig. 3.

13. Legal marches. Fig. 7.

14. Civil disobedience. Fig. 7.

15. Mass demonstrations. Fig. 7.

Appendix II. The Comparative Importance of 15 Issues

Issue	Short-Term		Partisan or Long-Term	
	Magnitude	Rank	Magnitude	Rank
1. LBJ's performance	8.91	1	10.56	1
2. Medical care	6.40	7	9.60	2
3. Standard of living	4.73	13	6.39	6
4. Aid to education	7.03	5	8.45	4
5. Power of government	7.79	3	9.32	3
6. Speed of civil rights	6.16	8	4.13	8
7. Negro violence	5.78	10	2.90	9
8. Residential segregation	5.19	11	1.25	14
9. Segregation in general	7.01	6	4.29	7
10. Urban unrest	8.78	2	6.59	5
11. Escalation/de-escalation	7.21	4	1.18	15
12. Immediate withdrawal/military victory	6.12	9	2.70	10
13. Legal marches	4.80	12	1.65	13
14. Civil disobedience	1.06	15	2.53	11
15. Mass demonstrations	3.18	14	2.07	12

If we are to compare the importance of a series of issues, we must have a quantitative measure. The measures above, which are based on an analogue of analysis of variance, offer the advantages of having a straightforward interpretation and of reflecting the existence of different numbers of cases across issue categories.

(1) Partisan or long-term component. The long-term effect of an issue is manifest in a change in the expected Democratic proportion of the vote across issue categories. A nonlinear measure of this change is the average of the absolute values of the deviations of the expected Democratic vote in the response categories from the expected Democratic vote for the whole item sample. The importance of each category in the total average is weighted by the proportion of the total cases in that category. This simple, percentage-point average is computed in this fashion.

If:

N = Number response categories for an issue,

$E_j \dots n$ = Expected Democratic vote in each issue category j , and

$WG_j \dots n$ = Ratio of the number of party identifiers and independents in category j over the average number of party identifiers and independents in each category (the weighting factor),

Then:

$$GE = \sum_{j=1}^N E_j WG_j / N \text{ (the grand expected Democratic vote);}$$

$$L = \sum_{j=1}^N \text{Abs. Val. } (E_j - GE)(WG_j) / N \text{ (the partisan or long-term component).}$$

To use Figure 1, the long-term component of speed of movement on civil rights, as an example, the grand expected Democratic vote is 57.4 per cent. The average deviation of the expected Democratic vote in each category from 57.4 (weighted by the proportion of cases in each category) is 4.1 percentage points. If there were no change from category to category, the category expectation would be the same as the grand expectation, that is 0.0.

(2) Short-term component. The short-term component of an issue is manifest in changes in the deviation of the expected from the observed Democratic vote across issue categories. That is, for all the voters included in Figure 1, Humphrey actually received 41.0 per cent of the vote, 16.4 percentage points below his expected vote in a normal election. If Humphrey had lost this same 16.4 percentage points in every category, then other factors than this one would have to account for his deficit. Thus, the average variation of the actual losses in each category (0.5, 9.8, and 21.2 points) from the grand or overall deficit (16.4) is a measure of the

short-term component of this issue. As before, the average is weighted by the number of cases in each issue category.

If:

$WA_{j...n}$ = Ratio of the number of voters in category j to the average number of voters in each category (the weighting factor), and

$O_{j...n}$ = Observed Democratic vote in each category j ,

Then:

$$D_j = E_j - O_j;$$

$$GD = \sum_n^{j-1} (D_j)(WA_j)/N \text{ (grand deviation of the observed from the expected Democratic vote);}$$

$$S = \sum_n^{j-1} \text{Abs. Val. } (D_j - GD)(WA_j)/N \text{ (the short-term component).}$$

Comment: The Assessment of Policy Voting*

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The voter has the option of voting or of abstaining; if he votes, he chooses among candidates. By "policy voting" we mean the extent to which these behaviors are caused by the voter acting in accord with his policy preferences. The question with which we are here concerned is: How do we know whether and to what extent "policy voting" has taken place in a given election? The process of obtaining this knowledge is what we mean by the "assessment of policy voting."

We will argue that the assessment of policy voting depends upon the specification of necessary and sufficient conditions for the influence of policy attitudes in the voting decision. But prior to this specification and in order to justify it, we will review existing assessment techniques.

Policy Voting as a Resultant of Short-Term Forces

The causal significance of policy preferences for the vote is generally treated as inhering in a given election. The issues that affect one election do not necessarily affect other elections past or future. Policy preferences, the voter's attitude toward the issue, are by these criteria treated as "short-term" forces.

The categories of short-term forces are conceived of by most scholars as persisting from election to election. However, the specific content of the categories and the impact of a given short-term force have been found to be highly variable.¹ Because such evidence as we have from the voting studies indicates that policy preferences are extraordinarily labile,² our theories of voting do not employ such concepts as

"stable" foreign affairs or labor relations attitudes in the way that they employ the concept of "stable partisan attitudes."

There is a mutual reinforcement between the conception of short-term effects of issues and the cross-section or short panel sample survey design. The learning of policy satisfaction/dissatisfaction over time which hypothetically could affect the strength and eventually the direction of partisanship, cannot easily be studied with such a data-gathering procedure. It is unusual in the cross-section to see partisan identification treated as a response partly learned from the reinforcement of policy preferences. Gerald Pomper, for example, does not consider the possibility that partisan realignment would produce exactly the same correlations that he attributes to increasing "ideological clarity."³ We are certainly not asserting that if the panel data were available, they would show a massive effect on partisan identification of the feedback of policy satisfaction. Clearly, we do not have such data available.

In our leading models of voting, partisan identification is said to be learned early as a part of primary socialization into family and class. The issue content of partisan identification, to the extent there is any, reflects these familial and class sources.⁴ This content in turn affects the attraction to issues (their salience for the voter) as well as the voter's stands on new issues as they arise. David RePass's notion of "issue partisanship" exemplifies this conception.⁵

Our leading models, thus, conceive of partisan identification as a "standing decision" which may reflect or may have once reflected

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¹ Donald Stokes, "Some Dynamic Elements of Contexts for the Presidency," *American Political Science* 60 (March, 1966), 19-28.

² See, for example, Philip Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David Apter, (New York: The Free

Press, 1964), pp. 206-261 and David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martins, 1969).

³ Gerald Pomper, "From Confusion to Clarity: Issues and American Voters, 1956-1968," *American Political Science Review*, 66 (June, 1972), 415-428.

⁴ V. O. Key, *Politics Parties and Pressure Groups*, 4th ed. (New York: Crowell, 1958), p. 238.

⁵ David RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389-400.

Table 1. Vote for Humphrey by Party Foreign Policy Performance by Partisan Identification

Party Identification	Party Preferred on Foreign Policy ^a		
	Democrats	Same by Both	Republicans
Strong Democrats %	92 ^b	85	50
(N)	(72)	(102)	(16)
Medium Democrats %	87	56	29
(N)	(29)	(151)	(35)
Independents ^c %	72	28	4
(N)	(24)	(144)	(78)
Medium Republicans %	50	9	7
(N)	(6)	(74)	(68)
Strong Republicans %	—	4	2
(N)	(0)	(27)	(85)

^a "Looking ahead, do you think the problem of keeping us out of a bigger war would be handled better in the next four years by the Democrats, by the Republicans, or about the same by both?"

^b Entries are per cent of the two-party vote going to the Democratic candidate, Humphrey; data from the SRC/ICPR 1968 American National Election Study.

^c Those in this row responded "Independent" on the first probe.

preferences for a direction of policy (perhaps still viable and rational, perhaps not) as well as a persistence of habitual behavior not related to public policy.⁶ Even occupational mobility, which may bring the voter into a new relationship with the consequences of public policy, does not much affect the voter's partisanship.⁷

Thus as political attitudes go, partisan identification, as currently conceived, is an attitude that does not yield to experience.⁸

Assessing Short-Term Effects

The notion of stable partisanship gives rise to techniques for assessing the extent of policy voting: V. O. Key, for example, uses votes contrary to partisanship among those who express dissatisfaction with their party's past performance or in their expectation of its performance in the future to estimate the impact of issues on the vote.⁹

⁶ V. O. Key and Frank Munger, "Social Determinism and Electoral Decision: The Case of Indiana," in *Public Opinion and Politics*, ed. William J. Crotty (New York: Holt, Rinehart and Winston, 1970), p. 253.

⁷ James A. Barber, Jr., *Social Mobility and Voting Behavior* (Chicago: Rand McNally, 1970), p. 114.

⁸ Not all conceptions of partisanship reflect this point of view: See, for example, Arthur Goldberg's "Model V", "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review*, 60 (December, 1966), 920-921. In this model Goldberg shows partisanship to be affected by partisan attitudes which include issue orientations. James L. Sundquist (*Politics and Policy* [Washington: Brookings, 1968]) finds the "[p]arty allegiances of some voters changing" in response to presidential performance (p. 438).

⁹ *Public Opinion and American Democracy* (New York: Knopf, 1961), pp. 472 ff.

Table 1 is constructed in the manner of those Key offers as evidence that issues are considerations in the collective electoral decision.¹⁰ Since in this table the proportions voting for Humphrey decrease monotonically along both columns and rows, the table shows both partisanship and performance evaluation related to patterns of voting for or against the candidate of the Democratic Party. But Table 1 does not unambiguously offer evidence of the impact of the lack of confidence in President Johnson's performance in the field of foreign affairs.

The ambiguity in such tables arises from three sources: (1) From the lack of a null model against which to compare observations; (2) from uncertainty about the direction of causality between the issue assessment by the voter and his vote; and (3) from the fact that the same defecting votes may be explained by other variables.

We need a null model in order to assess the strength of the effect of an issue. Suppose there were no other factors in the voting decision except partisanship and a given issue. Under the null model, that is, under the circumstances in which the issue had no short-term effect, how would this table appear? Key's interpretations seem to imply that, for party identifiers, the six Democratic identifier cells should show a one hundred per cent Democratic vote and the six cells for Republican identifiers should show a zero per cent Democratic vote. It is not clear what the expected Democratic vote among In-

¹⁰ See, for example, V. O. Key's Tables 18.4 and 18.5. *Public Opinion* . . . , p. 473 and p. 475.

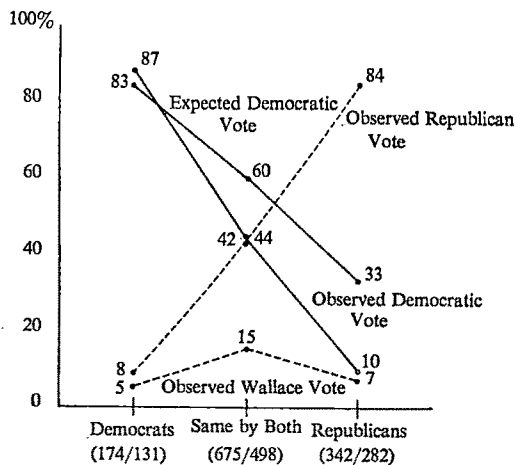


Figure 1. Attitudes toward war handling capabilities of the parties and presidential vote.

dependents would be. This null model cannot be the one Key intended. It denies any relevance to the strength of partisanship and Key uses this information. But how do we modify the null prediction to take account of the strength of partisanship? How much should we discount the expected vote of less than "strong" partisan identifiers?

A handy answer to our need for a null model is found with Converse's concept of the "normal vote."¹¹ Converse argues that the distribution of partisan loyalties among those with a given attitude or among those who subscribe to a given issue position, when weighted by the voting propensities associated with strength of partisanship, yields an expected percentage voting Democratic for the like-minded. Since this percentage reflects only the distribution of stable partisan loyalties, it is exactly the estimate of the expected vote in the absence of short-term forces that we need for the null model. The extent of the short-term effect of the issue is then estimated by the comparison of observed voting percentages with the percentages derived from the null model.

Applying this technique to the distribution of responses to the question of "which party can better handle the problem of keeping us out of a bigger war," we can see more clearly the processes underlying Table 1. Figure 1 displays this analysis. The distribution of partisan loyalties across positions on this issue leads us to expect a very substantial long-term effect. Compared to the fifteen issues examined in Richard

Boyd's article, this policy issue with a "magnitude of long-term effect" equal to 12.9 would rank first.¹² A short-term component is also present. Those voters who did not feel positive about the past or prospective performance of the Democrats in the field of foreign affairs defect at a rate higher than that predicted by the null model. Compared with the issues Boyd examines, the short-term component is modest. With a magnitude of $S = 4.77$, this issue ranks between the twelfth and thirteenth issues.¹³

"Normal vote" analyses help us to understand the effect of a given issue in the electorate at large. But, they share with V. O. Key's technique ambiguity about the direction of causality between attitudes and behavior and the problem of the possible continued re-explanation of the same voting acts. Both techniques assume that the issue attitudes exist prior to and thus can be a cause of the vote. Clearly, however, "rationalizing" of one's vote or intention to vote contrary to one's partisanship, through finding fault with the party's performance, would produce tables that look exactly like those produced by the process in which negative views of performance caused defecting votes. In fact, if causality runs from opinion to behavior for some citizens (the "policy voters") and from behavior to opinion for other citizens (the "rationalizers") tables such as this will still look the same. If, however, voters are simply rationalizing a defection which is otherwise caused, and if we cannot distinguish them from those who switch their vote on the basis of policy considerations, we are in danger of overestimating the extent of policy voting by the number of such voters.

Exploring Rationalization

We know of no ironclad way, within a cross-section sample survey on a given issue *considered by itself*, of disentangling these substantively different processes. But if we are willing to think of rationalizing as a general response set, its effects can be examined.

Think of the repeated naming of one political party as "best able" to solve important

¹² Richard Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), Appendix II.

¹³ It might be thought that there is a necessarily negative relationship between the magnitude of short and long-term effects. On the contrary, across the fifteen issues examined by Richard Boyd, the sizes of these two components are highly *positively* correlated ($r_s = .58$; $p < .025$). This suggests that, with some notable exceptions, previously politicized issues are the ones that get activated for and used by the voter during the campaign.

¹¹ Philip Converse, "The Concept of a Normal Vote," in Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *Elections and the Political Order* (New York: Wiley, 1966) pp. 9-39.

problems or as "closer" to one's own policy position as a measure of response consistency. Suppose we are willing to assume that those who rationalize their vote on one issue, by letting their vote dictate their stand, are apt to attempt to achieve psychological balance in the same way on a set of issues; then it is clear that the rationalizers will appear among those with high consistency scores.

If rationalization is *one way* of achieving a high party/issue consistency score, we can use this knowledge to test the *implausibility* of the hypothesis that rationalization causes the findings to be attributed to policy voting. Given an index of consistency, if the rationalization hypothesis is not justified, we will not find especially pronounced "anti-Democratic" issue consistency scores among those Democrats who vote against their party's candidate. We can only assess the *absence* of rationalization as a plausible explanation for the findings in the tables of the Key and Boyd type because other processes besides rationalization can lead to high consistency scores. If Democrats who defect are not pronouncedly more anti-Democratic than those who do not, rationalization is excluded; if they are, the question of causality is left unsettled.

Based on responses to two clusters of items in the 1968 SRC/ICPR pre- and post-election surveys, we have developed indices of partisan response consistency: The "Partisan Issue Alignment Index" reflects the degree to which the respondent felt the Democratic Party is the one to handle the "most important problems faced by the government in Washington." The index mirrors David RePass's approach of permitting the voter to identify the issues he feels are important.¹⁴ The index summarizes, on a 19-point scale—*viz.*, +9 (pro-Democratic) to -9 (anti-Democratic) the pattern of responses to the "first," "second," and "third," most important problems identified by the respondent.¹⁵

Our second index, "Expectation of Policy Satisfaction," summarizes the pattern of re-

sponse to 11 forced-choice policy items. It describes the degree on a 23-point scale—*viz.*, +11 (pro-Democratic) to -11 (anti-Democratic) to which a voter feels the positions he prefers will be carried out by the Democratic Party.¹⁶

Comparing the means and distributions of the two indices in the subsamples voting for and against Humphrey (Tables 2a and 2b), we find: (1) Those who vote against Humphrey are on the average less "pro-Democratic" in their assessment of the policy performance of the Democratic Party than are those who vote for him; this is true in every group of party identifiers. (2) The distributions about these means are such as to suggest that, within party identification groups, voters for and against Humphrey by virtue of their policy attitudes constitute separate and distinct subgroups. (3) These findings are true for salient issues and for those considered without regard to their salience for the voter.

These findings, of course, replicate David RePass's finding of the strong relationship between "issue partisanship (or alignment)" and voting choice in the 1964 Election. They do not, however, eliminate the ambiguity about whether attitudes are, as RePass contends, "a powerful influence . . . on voting choice"¹⁷ or whether voting choice is a powerful influence on attitudes.

When we control the comparison of Humphrey and Nixon voters' Partisan Issue Alignment by their opinion on "which party would better handle the problem of keeping us out of a bigger war," (Table 3) we find evidence which raises questions about the rationalization interpretation.

Strong Democrats who think the Republicans can handle the war problem better and

¹⁴ The Expectation of Policy Satisfaction Index expresses the degree to which the respondent, on eleven policy questions, saw the Democrats carrying out policies which he favored or associated the Republicans with policies of which he disapproved. The eleven policies covered: Aid to education, the growth of governmental power, federal medical programs, aid to employment, fair employment practices, integration, public accommodations, foreign aid, negotiations with Communist nations, trade with Communist nations, and Vietnam policy. On each issue, if the respondent favored a point of view and associated the Democrats with that point of view or if he expressed opposition to a policy and associated the Republicans with that policy position, he received a score of +1; if the opposite conditions obtained, he received a score of -1; if he did not respond to the policy or if he did not identify a party with one stand or the other, his score on that item was 0. The index sums these scores over the eleven issue areas.

¹⁵ David RePass, "Issue Salience . . ." pp. 399-400.

¹⁴ David RePass, "Issue Salience. . . ."

¹⁵ In the case of each of the three most important problems, the respondent was asked which party, if either, would carry out the policy he favored. The Partisan Issue Alignment Index sums the responses to these probes with weights 5, 3, and 1 attached to the first, second, and third most important problems, respectively. Thus, if a voter named the Democrats on all three probes, his weighted score would be +9; if he named the Republicans or Wallace, his score would be -9. On each probe, the "Democrats" response was scored +1, the "Both/Neither" response was scored 0, and the "Republicans" or "Wallace" response was scored -1. It is these scores which are weighted and summed to form the index.

Table 2a. Response Consistency, Party Identification and the Vote: Partisan Issue Alignment Index

Party Identification	Voters for Humphrey			Voters Against Humphrey ^a			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
Strong Democrats	4.70	4.3	188	-0.68	5.2	34	6.52	.005
Medium Democrats	1.82	4.4	139	-2.89	4.2	102	8.31	.005
Weak Democrats	3.14	4.6	49	-3.14	3.7	44	7.14	.005
Independents	1.35	2.7	20	-3.59	3.6	65	5.62	.005
Weak Republicans	-1.50	3.1	4	-5.27	4.0	97	1.85	.025
Medium Republicans	0.00	5.2	16	-4.30	4.1	147	3.88	.005
Strong Republicans	-3.67	7.5	3	-6.24	3.1	116	1.31	ns ^b

^a Combines voters for Nixon and Wallace.^b $p > .05$.Table 2b. Response Consistency, Party Identification and the Vote: Expectation of Policy Satisfaction Index^a

Party Identification	Voters for Humphrey			Voters Against Humphrey			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
Strong Democrats	3.44	3.5	188	0.27	4.3	34	4.62	.005
Medium Democrats	1.71	3.3	139	-1.02	2.5	102	6.99	.005
Weak Democrats	2.24	3.3	49	-0.31	3.1	44	3.81	.005
Independents	1.48	2.7	20	-0.87	2.1	65	4.02	.005
Weak Republicans	-0.88	1.4	4	-1.64	2.9	97	0.51	ns
Medium Republicans	2.19	3.4	16	-1.38	3.1	147	4.33	.005
Strong Republicans	-0.17	3.9	3	-1.71	3.3	116	0.78	ns

^a See footnotes to Table 2a.

who voted for Nixon are not significantly more prone to deprecate their party consistently than are Strong Democrats who share their foreign policy beliefs but who did not defect. These Democrats are on the average less pro-Democratic, but their scores are so variable as to make the mean an unreliable estimate of the response tendencies within the group. According to cognitive balance theories, this group of voters should have the greatest psychological incentive for rationalization; their failure to conform to its predictions is a source of doubt about the rationalization interpretation.

Our doubts increase when we consider that those Strong Democrats who do not distinguish the parties on their war-handling capability and who voted against their partisanship in this instance behaved contrary to their overall response tendency and passed up an opportunity to deprecate the performance of the Democratic Administration.

With such small N's within cells, these findings can hardly be considered conclusive. They should give pause to those who argue the rationalization of defection.

These analyses partly clear the Key and

Boyd approaches of the problem of rationalization. If we are satisfied on this score, we have, especially in the normal vote approach, techniques for establishing whether or not there is a correlation between policy preferences and voting choices in the electorate at large. But, as Boyd explicitly acknowledges, this does not permit us to conclude "that the issue was causally related to the vote or that the issue made some net contribution to the observed Democratic vote."¹⁸ Since it is necessary to establish precisely these facts in order to demonstrate that policy voting has taken place, we are not fully content with normal vote analysis as a technique.

While agreeing with Boyd's analysis of the shortcomings of the normal vote approach—and, incidentally, with his analysis of its strengths, as well—we have reservations about his contention that these shortcomings are inherent in survey data.¹⁹ We believe the difficulty lies with aggregating across the electorate, rather than with modeling the individual voting decision, and with treating each policy issue in

¹⁸ Richard Boyd, "Popular Control . . ." p. 432.¹⁹ Richard Boyd, "Popular Control . . ." p. 432.

Table 3. Response Consistency of Democratic Identifiers By Strength of Partisanship, Presidential Vote, and Beliefs about Which Party Can Better Handle the Problem of War: Partisan Issue Alignment Index

Which Party Can Handle War/ Partisan Strength	Voters for Humphrey			Voters for Nixon ^a			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
“Democrats”								
Strong Democrats	6.57	3.6	67	3.50	5.9	4	1.56	ns ^b
Medium Democrats	5.96	3.5	26	0.00	—	1	— ^c	—
Weak Democrats	6.50	2.2	14	−8.00	—	1	— ^c	—
“Same by Both”								
Strong Democrats	4.06	4.0	87	−0.17	2.9	6	2.53	.01
Medium Democrats	1.26	4.1	85	−3.11	3.5	36	5.49	.005
Weak Democrats	2.11	4.4	27	−3.06	2.8	17	4.19	.005
“Republicans”								
Strong Democrats	−1.88	4.9	8	−5.17	5.4	6	1.10	ns ^b
Medium Democrats	−2.90	4.2	10	−5.70	4.1	23	1.73	.05
Weak Democrats	8.00	—	1	−3.71	4.1	7	— ^c	—

^a Since the "Republicans" was a specific category in the item, 56 Wallace voters are not considered here.

^b $p > .05$.

^c Too few cases for comparison.

isolation from every other. Taking the issues one at a time means that the approach is continually reapportioning the same observed variation in the vote. In this way many factors can appear as indispensable in explaining the vote. Given the multifaceted nature of political reality, disciplined statements about the causes of behavior will be necessarily multivariate.

There is no way to avoid modeling the individual voting decision and still estimate policy voting. The analysis must bring the respondents by means of their own testimony into a direct relationship with the issues. The analyst must determine that the voter viewed the candidates or parties in relation to the policy issues. In short, the analyst has to ascertain whether the voter has satisfied the necessary and sufficient conditions for policy voting.

The Requirements of Policy Voting

We have from diverse sources more or less consonant lists of the elements required for policy voting.²⁰ The elements of such lists describe a decision process which includes as independent variables: (1) The salience of the policy issue (or issue area) for the voter. Butler and Stokes, term it a "link" between the issue and the self.²¹ Davis, Hinich, and Ordeshook make attention to the issue and responsiveness to the

candidates a requirement.²² (2) The voter's position on the issue or his preferences for a line of policy. (3) The voter's perception of the candidates' or parties' position on the policy issue.²³

Given these elements, an influence of policy on voting is said to be present when the voter's comparative evaluation of the candidates or his vote or both are independently affected by the relative proximity of the candidates to his position on issues that are for him salient. Figure 2 idealizes this decision process as a recursive arrow diagram. It reflects the assumption that *ceteris paribus* the voter will prefer (i.e., evaluate more positively), the candidate closer to him on salient issues and that he will, if he votes, vote for the preferred candidate.

Assessing the Requirements of Policy Voting

From the perspective of assessment, this model gives rise to the need for measures of the salience of issues and of the perceived position of the candidates on the issues.

"Salience," which can be measured as Re-Pass suggests by respondents' identification of the "most important problems the government . . . should try to take care of,"²⁴ is an indication of an *affective* link between the voter and the

²⁰ See: Butler and Stokes, *Political Change* . . . , "The Analysis of Issues," pp. 173-92 and Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June, 1970), pp. 426-428.

²¹ David Butler and Donald Stokes, *Political Change* . . . , p. 174.

²² Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . ." p. 429.

²³ Those familiar with the Davis et al., "An Expository Development . . ." article, will recognize that in singling out the policy or issue component of these elements, we have made policy voting a special case of their broader class of "rational voting."

²⁴ See: Items 16 through 16p on the SRC/ICPR 1968 Pre-Election Questionnaire.

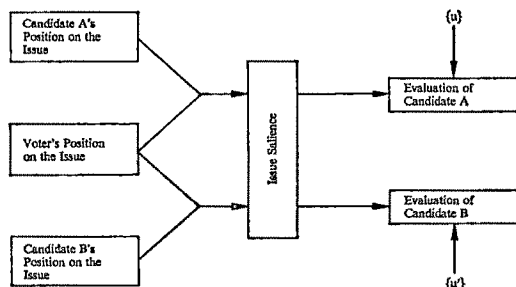


Figure 2. Necessary and sufficient conditions for policy voting.

issue. Salience does not, however, ensure a *cognitive* link. Simply because voters are attracted to an issue or think it an important problem is no guarantee that they will necessarily seek or retain information about it. If this generalization needs support, consider, for example, the fact that in 1967 more than one-third of those who thought the war in Vietnam important enough to engage in the unusual political act of trying to change someone's opinion on the problem could not identify Hanoi as the capital of North Vietnam.²⁵

Partly because of this lack of cognitive fidelity, we feel it is essential to measure the voter's perception of the candidates' issue positions. Davis, Hinich, and Ordeshook assume that "*all citizens make identical estimates*" of a candidate's issue position.²⁶ However expedient such an assumption may be in modeling candidate behavior, it is not necessary in order to model voting behavior. Singularity of perception of the candidates' stands is, of course, counterindicated by an impressive amount of data.²⁷ But the assumption is not required on analytic grounds either.

Relaxing the assumption that voters share a common perception of where the candidates stand in "issue space" entails admitting causes of that perception beside or in addition to candidate behavior. Once it is established where the candidates are *perceived* to stand in issue space, the voter's individual loss function²⁸ or

²⁵ Richard A. Brody and Sidney Verba, "Hawk and Dove: The Search for an Explanation of Vietnam Policy Preferences," *Acta Politica*, forthcoming.

²⁶ Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . .," p. 431, emphasis in the original.

²⁷ See, for example, Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954) and Benjamin I. Page and Richard A. Brody, "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review*, 66 (September, 1972), forthcoming.

²⁸ Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . .," p. 432.

other psychological distance measure²⁹ can be computed and related to his voting behavior. In other words, the basic hypothesis that the voter will vote for the candidate most proximal to him on the issues is common to both approaches. The hypothesis does not depend upon the candidate's being perceived at one and only one point.

As formulated, the hypothesis is undoubtedly true. It is true of the electorate on particular issues, and it is true when issues are aggregated and a composite index is generated.

Consider Figure 3, for example: In this graph the abscissa is an attenuated version of a scale measuring the signed difference of the perceived distance from the voter to Humphrey and the voter to Nixon, on a seven-point "Vietnam policy" scale.³⁰ Using the "normal vote" component as our null model, we find that

²⁹ The 1968 election was the occasion for at least three independent efforts to relate psychological distance/proximity to voting behavior. Michael Shapiro's study of voters in Hawaii is reported in "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), 1106-1119. The two based on national samples are represented in preliminary reports in convention papers: See: Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, The Urban Crisis, and the 1968 Presidential Election: A Preliminary Analysis," paper delivered at the Annual Meeting of the American Sociological Association, San Francisco, California, September, 1969; and David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party, Ideology, Issues, and Candidate Choice: A Preliminary Theory-Relevant Analysis of the 1968 American Presidential Election," paper delivered at the Eighth World Congress, International Political Science Association, Munich, Germany, August 31-September 5, 1970.

³⁰ See: SRC/ICPR, 1968 Post-Election Questionnaire, questions 67, 67b, 67c, and 67e.

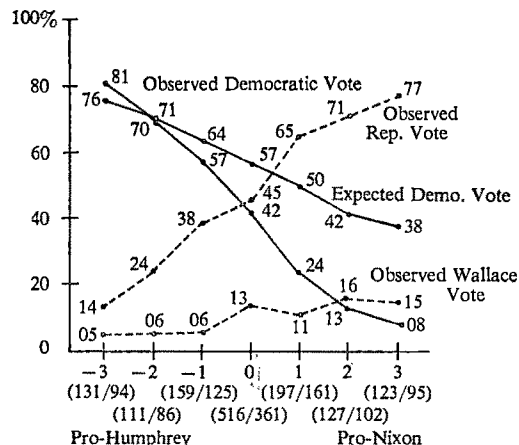


Figure 3. Vietnam issue distance and the vote in 1968.

there is a long-term effect which reflects the correlation of the Vietnam distance comparison with partisan identification. An even larger short-term component can also be seen in these data;³¹ proximity on this issue and voting behavior clearly covary.

Similarly, in Figure 4, we can see that a composite measure of distances across salient issue areas³² shows substantial long and short-term components. A long-term effect of this magnitude ($L = 14.5$) would rank first among the fifteen issues examined by Boyd. A short-term effect of this magnitude ($S = 8.37$) would rank just below the second most potent of Boyd's issues. These data unambiguously show that issue proximities covary with voting behavior.

A Question of What Causes What

If it is true, as we have shown, that voters vote for the candidates proximal to them on the issues, then the processes relevant to the perception of that proximity become pertinent in assessing the extent of policy voting. These processes are pertinent because we may wish to exclude from our definition of policy voting some behaviors which they describe.

Consider the elements of the model depicted in Figure 2: If we relax the assumption of recursivity and permit the voter's evaluations of the candidates (or his vote) to cause his perceptions of their stands or his own policy position or both, we can distinguish three "proximity" processes. These processes, singly or in combination, could produce graphs like those in Figure 3 or 4.

The first process, "Policy Oriented Evaluation," is depicted in Figure 2. It describes the cases in which the prior held policy position of the voter and his perceptions of the candidates' positions (which are presumably independent of each other) cause his evaluation. Thus, in the slice of time considered by this model or covered in a cross-sectional survey, the voter's position and perception are invariant, and evaluations vary in accord with the issue proximities.

³¹ Compared with the fifteen issues examined by Richard Boyd ("Popular Control . . .," Appendix II), comparative distance on Vietnam policy, with $L = 7.91$ and $S = 8.65$, ranks fifth on long-term effect and third on its short-term component.

³² Our composite measure is the Partisan Issue Alignment Index (see Footnote 15). We interpret the response to "which party do you think would be most likely to do what you want on this . . .?" as a dichotomous (near, not near) distance measure. In this graph, the abscissa values are a grouping of the +9 to -9 index; -9 to -6 = -2; -5 to -1 = -1; 0 = 0; 1 to 5 = 1; and 6 to 9 = 2.

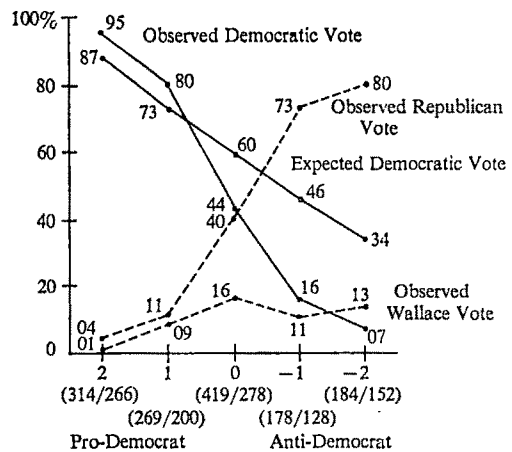


Figure 4. Issue alignment and the vote in 1968: Salient issue distances.

Suppose we change the invariant elements of this model: If perceptions and evaluations were invariant and the respondent's own position yielded to or formed around the position he believed was held by the candidate he favored (or intended to vote for), we would say that the voter had been "persuaded" by the candidate. "Persuasion" may be a perfectly legitimate political process, but in the slice of time being considered here, it is an effect of, not a cause of, candidate evaluation. In these terms, we cannot say that the voter who is persuaded is evaluating or voting on the basis of policy.

The third process stems from allowing the perceived position of the candidate to vary with the voter's own position and his evaluation of the candidate. Under this model, a voter could see a candidate as close to himself on an issue because he otherwise felt positive about the candidate, and he could see other candidates as distant from him on the issue(s) because he felt negative about them for other reasons. In other words, a voter would "project" his own (or a nearby) stand onto candidates he favored. We are reluctant to call those who project "policy voters."

Both persuasion and projection have been found to operate in the American electorate. Berelson, Lazarsfeld, and McPhee, found these processes operating on policy stands as clear as President Truman's position on Taft-Hartley repeal³³; we found them operating on the less clear Vietnam policy positions of candidates Nixon and Humphrey in 1968.³⁴

³³ Bernard Berelson, Paul Lazarsfeld, William McPhee, *Voting*, pp. 220-222.

³⁴ Benjamin I. Page and Richard A. Brody, "Policy Voting . . ."

The presence of these two "alternate" processes in the electoral system makes it inappropriate to declare policy-oriented evaluation the cause of the correspondence between issue proximity and voting behavior. We need some means for examining the potential for "persuasion" and for "projection" and of estimating them as separate processes.³⁵

If we confine ourselves to the "position," "perception," and "evaluation" variables in a given survey, there is no way to estimate the separate effect of each of the three proximity processes. The solution to the problem lies with panel data within a given election year, in the manner of the Lazarsfeld studies, or with expanding the set of variables considered. This latter approach would entail finding causes of persuasion which are not also causes of projection or policy-oriented evaluation; finding causes of projection which are not causes of either persuasion or policy-oriented evaluation; and finding causes of policy-oriented evaluation which are not causes of the other proximity processes.³⁶ The specification of such causal factors would permit us to use a technique of causal modeling which explicitly takes account of the possibility of all three processes working

and estimates the extent of each; we refer, of course, to the econometrician's simultaneous equation estimation procedures.

In the case of policy-oriented evaluation of a candidate, the simultaneous equation approach suggests a three-equation model; in this model, each of the central variables (own position, perception of candidates' positions, and evaluation of the candidate) has a single equation in which it is the dependent variable and hence a function of the other two. This would give us one equation for each proximity process. Up to this point without the independent (exogenous) causal factors, no estimates can be derived; once the independent causal factors are found, we can at last achieve an estimate of the extent of policy voting.³⁷

The discovery of these independent causal factors is no easy matter. There is no statistical procedure which will uncover them. They must come from our theories of behavior and our knowledge about the act of voting. However difficult it is to specify such causal factors, that is exactly where the problem is. If the estimation of policy voting is important to the understanding of the role of the citizen in a democracy—and theorists of democracy certainly write as if it is—then any procedure which fails to control for projection and persuasion will be an undependable base upon which to build our understanding.

Research such as that of Boyd, Key, Kovenock, Jackson, Pomper, RePass, and Shapiro—and, we hope, our work, too—should unsettle those who are complacent in their belief that policy and issues play no role in the electoral decisions of the American voter. Having unsettled the complacent, let us take the steps necessary to settle the question.

³⁵ David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party . . .," pp. 29–31, discuss this problem and refer to reasons for believing it not to be important. Their reasoning is based on the number of issues examined, the nature of their data gathering procedures, the fact that the items most clearly associated with the several candidates are different for each of them, and the fact that on some issues those who vote for one candidate are closer to another. Such plausible evidence encourages rather than satisfies the search for estimates of the three proximity processes.

³⁶ Those acquainted with econometric approaches to nonrecursive causal modeling will recognize these causal sets as the exogenous variables required to "identify" a system of simultaneous equations. See, for example, Franklin Fisher, "Statistical Identifiability," in *International Encyclopedia of the Social Sciences*, Volume 15, ed. David Sills, (New York: The Macmillan Co. and Free Press, 1968), pp. 201–206.

³⁷ For example, see: Michael Shapiro, "Rational Political Man . . ." and John Jackson, "The Importance of Issues and Issue Importance in Presidential Elections," Discussion Paper #3, Program of Quantitative Analysis in Political Science, Government Department, Harvard University, January, 1972.

Comment: The Issues in Issue Voting*

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Issue voting, it appears, has been rediscovered once again. The articles by Professors Boyd and Pomper join a rapidly growing literature. Some thirty books, articles, and convention papers¹ have appeared in recent years, all

*I should like to thank my colleagues Aage R. Clausen, C. Richard Hofstetter, and Theodore W. Meckstroth for very helpful reactions to an earlier draft of this comment.

¹The literature on this topic includes: Richard W. Boyd, "Presidential Elections: An Explanation of Voting Defection," *American Political Science Review*, 63 (June, 1969), pp. 498-514; Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), pp. 429-449; Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, the Urban Crisis, and the 1968 Presidential Election: A Preliminary Analysis," a paper prepared for the 1969 meeting of the American Sociological Association, September, 1969; Richard A. Brody and Benjamin I. Page, "Policy Voting and the Electoral Process: The Vietnam War Issue," a paper prepared for delivery at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7-11, 1971; Richard A. Brody and Benjamin I. Page, "Comment: The Assessment of Policy Voting," *American Political Science Review*, 66 (June, 1972), pp. 450-458; Comparative State Election Project, *Explaining the Vote: Presidential Choices in the Nation and the States, 1968* (Chapel Hill, N.C.: Univ. of North Carolina Press, 1972); Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), pp. 321-336; Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), pp. 1083-1105; John Osgood Field and Ronald E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly*, 33 (Fall, 1969), pp. 380-398; Martin Fishbein and Fred S. Coombs, "Modern Attitude Theory and the Explanation of Voting Choice," a paper prepared for delivery at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7-11, 1971; William H. Flanigan, *Political Behavior of the American Electorate*, 2nd ed. (Boston: Allyn and Bacon, 1972); Carl C. Hetrick, "Issues and Politics: An Exploration in Policy-Motivated Political Behavior," a paper prepared for the 1968 Annual Meeting of the American Political Science Association, Washington, D.C., September 2-7, 1968; John H. Kessel, *The Goldwater Coalition: Republican Strategies in 1964* (Indianapolis: Bobbs-Merrill, 1968); John H. Kessel, "Pennsylvania, 1968: Analysis of a Belief System," a paper prepared for delivery at the 1971 Annual Meeting of the Midwest Political Science Association, April 29-May 1, 1971, Chicago, Illinois; V. O. Key, Jr. (with the

suggesting that issues, in one way or another, have had consequences with respect to presidential voting choice.

assistance of Milton C. Cummings, Jr.), *The Responsible Electorate: Rationality in Presidential Voting, 1936-1960* (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1966); Samuel A. Kirkpatrick, "Political Attitudes and Behavior: Some Consequences of Attitudinal Ordering," *Midwest Journal of Political Science*, 14 (February, 1970), pp. 1-24; Samuel A. Kirkpatrick, "Political Attitude Structure and Component Change," *Public Opinion Quarterly*, 34 (Fall, 1970), pp. 403-407; David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party, Ideology, and Candidate Choice: A Preliminary Theory-Relevant Analysis of the 1968 American Presidential Election," a working paper prepared for the Eighth World Congress of the International Political Science Association, Munich, Germany, August 31-September 5, 1970; Richard M. Merelman, "Electoral Instability and the American Party System," *The Journal of Politics*, 32 (February, 1970), pp. 115-139; Peter B. Natchez and Irvin C. Bupp, "Candidates, Issues and Voters," *Public Policy*, 1968, pp. 409-437; John C. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970), pp. 25-42; Gerald M. Pomper, "After Twenty Years: The Report of the APSA Committee on Political Parties," a paper prepared for delivery at the 1970 Annual Meeting of the American Political Science Association, Los Angeles, California, September 8-12, 1970; Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956-1968," *American Political Science Review*, 66 (June, 1972), pp. 415-428; David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), pp. 389-400; John P. Robinson, "Public Reaction to Political Protest: Chicago, 1968," *Public Opinion Quarterly*, 34 (Spring, 1970), pp. 1-9; Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), pp. 1106-1119; J. Merrill Shanks, "The Quality of Electoral Change, 1952-1964," a paper prepared for delivery at the 1969 Annual Meeting of the American Political Science Association, New York, N.Y., September 2-6, 1969; Peter W. Sperlich, *Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior* (Chicago: Rand-McNally, 1971); Donald E. Stokes, "Some Dynamic Elements of Contexts for the Presidency," *American Political Science Review*, 60 (March, 1966), pp. 19-28; Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluations," *American Political Science Review*, 64 (December, 1970), pp. 1167-1185; Herbert F. Weisberg and Jerrold G. Rusk, "Perceptions of Presidential Candidates: A Midterm Report," a paper presented at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7-11, 1971.

There are reasons for all this intellectual effort. Some attend any scientific enterprise—researchers working independently who come to similar conclusions; the redundancy necessary to communicate through a “noisy channel” to a busy professional audience—but reasons can also be found in the complexity of the subject matter. The inquiry does not take the form of a simple debate over whether issues are or are not consequential. The proper questions are: (1) How important are attitudes about issues among several factors that motivate a person to vote for one candidate rather than another? (2) How important is any given issue within the set of issues that does affect the voting decision? (3) Under what specific conditions is issue voting, thus defined, enhanced or depressed? The answers one obtains to these questions, in turn, depend on measurement considerations: (1) To what degree are issues reflected in the data set? (2) What kind of measures of the importance of issues are employed in the analysis? (3) What is the “true level” (assuming no measurement error) of issue voting among the particular population from which the data has been collected?

In their articles, Richard Boyd and Gerald Pomper also join those² who have asserted that *The American Voter*³ needs to be updated with respect to issue voting. The portrait of an electorate that has little concern for issues, they tell us, is no longer valid. Even granting that a twelve-year-old book probably is in need of revision, this claim adds two more questions to the list we already have: (1) What did *The American Voter* actually say about the extent of issue voting? (2) How much change has taken place in the “true level” of issue voting from the 1950s to the present? In sum, we must deal with several interrelated considerations to get any clear idea of how much revision is needed in the conclusions presented in *The American Voter* about the extent of issue voting. Let us look at each of these in turn.

What did “The American Voter” say?

The statements by Professors Boyd and Pomper are standard interpretations of the received doctrine. Most of us, myself included, have had such statements in our lecture notes

² Field and Anderson, “Ideology in the 1964 Election;” Kovenock, Beardsley and Prothro, “Status, Party, Ideology, and Candidate Choice;” Natchez and Bupp, “Candidates, Issues and Voters;” Pierce, “Changing Role of Ideology;” and RePass, “Issue Salience and Party Choice.”

³ Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960).

ever since *The American Voter* appeared. It is important to remember, however, that three different analyses of issue voting were made in the book. The first, cited by both Boyd and Pomper, sets up three tests for “issue-oriented partisan behavior”—expression of an opinion, perception of what the government is doing, and perception of party differences. The book states that between 18 per cent and 36 per cent of the respondents pass these tests, depending on the issue involved.⁴

The second analysis was based on the measure the authors developed for levels of conceptualization; they reported that only 3½ per cent of the voters fell into the “full ideologue” category, but went on to say that “these are the people who best fit the assumptions about ‘political man.’”⁵ A small segment of the population, in other words, but one capable of playing a crucial and ideologically motivated role in deciding an election.

The third analysis is the six-component model of individual voting choice. Here, if one looks at the standardized regression coefficients⁶ as measures of the importance of the components, he discovers that issues were more important in 1952 than in 1956. In 1952, domestic issues ranked second among the six components, while attitudes on foreign issues were more important than were attitudes toward the Democratic nominee, Adlai Stevenson. In short, *The American Voter* presented a mixed verdict on the importance of issues. Thus, frequent statements that the book suggested issues were not of great consequence say more about the loss of information when a complex phenomenon is reduced to a dichotomous variable (important/unimportant) than they do about the content of the book itself.

The Nature of the Data Set

Four different sets of questions have been used for the analysis of issue voting; each has characteristics which affect the results of the analysis. The first are the precoded questions used by Professors Boyd and Pomper. These questions facilitate comparisons between issues and make longitudinal comparisons possible for repeated items. They do not, however, tell anything about the importance of issues as compared to parties, candidates, or respondents’ life

⁴ Campbell et al., *American Voter*, chap. 8.

⁵ Campbell et al., *American Voter*, chap. 10.

⁶ Donald E. Stokes, Angus Campbell, and Warren E. Miller, “Components of Electoral Decision,” *American Political Science Review*, 52 (June, 1958), pp. 367–387.

circumstances. Nor can they capture references to issues other than the finite set about which questions are asked.

The second type of question that has been used is the open-ended question about parties and candidates. These standard queries—Is there anything in particular that you like/don't like about the Democratic/Republican party? Is there anything in particular about the Democratic/Republican candidate that might make you want to vote for/against him?—elicit a certain amount of material about issues, allow analysis along several important cognitive dimensions, and certainly provide our most comprehensive longitudinal data set. But since these questions do not ask specifically about issues, they stimulate fewer issue responses than comments about candidates and parties. Therefore analyses based upon this data set tend to underestimate the effects of issues.

The third data set was added to the Survey Research Center interview schedule in 1960. The key question in this open-ended sequence was: "What would you personally feel are the most important problems the government should try to take care of when the new President and Congress take office in January?" The strengths and weaknesses of this question lie in the focus on issues. It elicits richly detailed issue data. It opens real possibilities for comparisons between issues, but inhibits comparisons of the importance of issues vis-à-vis attitudes on other topics.

The fourth data set consists of precoded questions about the respondent's stand on given issues and his perceptions of the candidates' positions on the same issues. These data make it possible to calculate attitudinal proximity between the respondent and the candidates. These items were first used in the Brody-Page-Verba study and the Comparative State Election Project in 1968, and were added to the election study of what is now called the Center for Political Studies in 1970. These relatively new attitudinal proximity scores show attitudes about issues to be more important than was evident when our estimates were based on the respondent's position alone.

I do not wish to overstate the differences among the four data sets. A careful analyst will be able to discern effects of the issues by using any one of them; if he is sensitive to the differences among the data sets, he can move from one to another in the course of his analysis. Since the responses contain greater or lesser amounts of issue data, however, they do affect one's findings about the importance of issues as compared to other factors.

The Nature of the Analysis

What one does with the data after they have been collected will also affect one's measures of the importance of the issues. In general, two lines of the analysis have been developed in the recent literature on issue voting: One group of scholars has concentrated on those voters who are particularly likely to be sensitive to issues. This would include Natchez and Bupp's⁷ work on issue publics, Hetrick's⁸ on policy-motivated voters, and Field and Anderson's⁹ on ideologues. All isolated those voters whose responses to open-ended questions showed a concern with the issues. Other scholars perfected new indices or used new concepts such as Converse's¹⁰ normal vote to monitor the effect of issues. John Pierce¹¹ suggested measures for "informational ideologues" and "affective ideologues" to add to the "conceptual ideologues" who appeared in *The American Voter*. Richard Boyd¹² employed indices of party compatibility, candidate compatibility, and issue compatibility to analyze vote defection. David RePass¹³ worked out a fine measure of issue partisanship. Merrill Shanks¹⁴ and Richard Boyd¹⁵ have shown new ways in which Converse's normal vote measure could be employed. Both of these lines of analysis—the isolation of issue-motivated voters and the use of issue-sensitive measures—have naturally tended to heighten our awareness of issue voting.

Change in the Level of Issue Voting

Assuming now that all these measurement questions have been solved and that we have satisfactory evidence of a change in the level of issue voting, what could account for this change? There are at least three possibilities. First, attitudes about issues may have become relatively more important as compared with other attitudes in the electorate as a whole. Second, the proportion of voters to whom attitudes about issues are relatively important may have grown sufficiently to be reflected in the overall figures. The third possibility is that the voters are no more and no less aware of issues than

⁷ Natchez and Bupp, "Candidates, Issues and Voters."

⁸ Hetrick, "Issues and Politics."

⁹ Field and Anderson, "Ideology in the 1964 Election."

¹⁰ Philip E. Converse, "The Concept of a Normal Vote," in *Elections and the Political Order*, ed. Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, (New York: Wiley, 1966), chap. 2.

¹¹ Pierce, *Changing Role of Ideology*.

¹² Boyd, "Voting Defection."

¹³ RePass, "Issue Salience and Party Choice."

¹⁴ Shanks, "Quality of Electoral Change."

¹⁵ Boyd, "Normal Vote Analysis of 1968 Election."

they ever have been, but instead the parties may have moved farther apart on some ideological dimensions so that voters are more likely to perceive the issue consequences of their votes. The difficulty, of course, is that these processes are not mutually exclusive. All three of them may have been going on at the same time, in addition to the methodological innovation that has also been taking place. Hence the problem is to unravel multiple causes for change in the observed level of issue voting.

Elementary prudence would suggest analytical procedures that separate substantive and methodological causes for such change. The substantive argument is that there have been changes in the real world of American politics, that the 1950s were quiescent in contrast to the turbulent 1960s and/or that the relatively younger, relatively better educated electorate we now have is more aware of issues. To demonstrate this type of change, one should use the same analytical technique with the same data set over a series of elections. The methodological arguments hold that the earlier data sets, or techniques of exploiting them, were not as sensitive to issues as are some newer data sets or techniques. To show this, one should compare the two data sets with the same technique in the same election, or compare the two analytical procedures with the same data set in the same election. If one wishes to make both a methodological and a substantive argument, he should first make this methodological case, then proceed to longitudinal analysis. If he wishes, in addition, to argue that *The American Voter* is inadequate, he must anchor the beginning of his time series in the data sets and measures in that book.

A Closer Look at Some Specific Problems

The unhappy fact is that the methodological problems are of such a character, and the number of comparable data sets is so limited, that even the most careful scholars are unable to avoid pitfalls. Pomper's and Boyd's analyses are quite different. Pomper's is a longitudinal analysis which has the great virtue of joining Stokes,¹⁶ Field and Anderson,¹⁷ and Pierce¹⁸ in presenting comparable data over time. Boyd's cross-sectional analysis of the 1968 election employs a normal vote analysis to discriminate between long-term and short-term effects. Both

analyses, however, have important limitations. Pomper's work was hampered by the limited number of precoded questions that had been asked over time, and by the nature of these items. Boyd's application of the normal vote is inhibited because these calculations produce estimates for a Democratic candidate running against a Republican candidate, and 1968 was a year in which three candidates were in the race.

Leaving aside the question of foreign aid (something of a special case and not central to his analysis), Gerald Pomper presents evidence that there is an increasing correlation between party identification and a "liberal" position on five issues (aid to education, medical care, job guarantee, fair employment, and school integration) as well as an increasing awareness of party differences and an increasing consensus as to how the parties differ. He has done what he could with the data available to him. He has exhausted the set of questions that were asked over time, and he has exploited the follow-up questions about the parties. But to judge the adequacy of his analysis, one must note the wording of the questions themselves. The 1956 item on aid to education read:

If cities and towns around the country need help to build more schools, the government in Washington ought to give them the money they need. Do you have an opinion on this or not? (IF YES) Do you agree that the government should do this or do you think the government should not do it?

The wording of the questions varied slightly from issue to issue, and a slightly different form was used in 1964 and 1968, but each of these items asked about an increase in the power of the federal government. Now it happens that attitudes on this issue—the desirability of increasing the power of the federal government—are also strongly related to party identification. In fact, the zero-order relationships between attitudes on the power of the federal government and party identification are stronger than those on the five specific issues reported by Professor Pomper. This suggests that the original relationships reported stem from a combination of two attitudes: that about the specific issue and that about the federal government. Table 1 presents the results of a test to determine how much of the relationship was caused by the association between party identification and attitudes about the federal government. Kendall's Tau-b measure of association has been calculated for the relationships reported by Pomper, as have the first-order partials in which the relationship has been

¹⁶ Stokes, "Dynamic Elements of Contests for Presidency."

¹⁷ Field and Anderson, "Ideology in the 1964 Election."

¹⁸ Pierce, "Changing Role of Ideology."

Table 1. Reanalysis of Pomper's Data*

Issue	Kendall's Tau-b			
	1964		1968	
	No Control	Control	No Control	Control
Aid to Education	.224	.132	.233	.163
Medicare	.311	.216	.269	.216
Job Guarantee	.218	.154	.175	.122
F.E.P.C.	.157	.063	.160	.116
School Integration	.063	-.027	.141	.080

* Kendall's Tau-b measures of association between specific issues and party identification, with and without control for respondent's attitude toward increasing power of federal government.

controlled for the respondent's attitude toward increasing the power of the federal government. When this control is introduced, a different pattern is evident. With the social welfare issues, half of the variance purportedly accounted for by the original relationship is eliminated; for the two civil rights items, the reduction in explanatory power is even greater. Much of the temporal variation is eliminated in three social welfare items, but it is even more pronounced with the integration questions. It seems clear that we are dealing with a covariation of two attitudes and that we must untangle the effects to find out what has been happening. But such analysis is blocked by another limitation of the data set: The question that asked directly about federal power was not included prior to 1964. Hence we know only that the single dimension of federal power affects the relationship in 1964 and 1968. We are limited to inference and speculation about 1956 and 1960.

Since Professor Pomper's analysis depends so heavily on attitudes about the power of the federal government, I wish this dependence had been stressed in his narration. There is literary convenience in using the single words "liberal" and "conservative," but there is also distortion. It requires longer phrases to refer to Democratic emphasis on Washington, D.C., and Republican emphasis on state capitols, but these references capture one of the most important partisan distinctions of our era. I also think the analysis loses force by reporting the voters' perceptions of the parties as "liberal" and "conservative." Just as the Lokele drummers of the Congo refused to believe that Europeans did not communicate by drums, so many political scientists seem to insist that most Americans use

the concepts "liberal" and "conservative" in spite of abundant evidence that these terms are too abstract to be grasped by people of average education. In fact, the ability to discern which party "is closer to what you want" (the point requested in the original interview item) is more vital for voter rationality than a capacity to understand the denotation of abstract concepts.

Although Professor Pomper's finding is limited, the limiting derives from the nature of the data set rather than from lack of skill in exploiting the data. Indeed, he has greatly strengthened his study by introducing controls for age, education, and race/region. Although changed perceptions of party differences have been most apt to occur among blacks and Southern whites, Pomper's analysis permits the general conclusion that the increased sensitivity to issues is characteristic of the electorate as a whole. This, of course, is a different kind of electoral shift from one that occurs because of changes in the demographic structure of the electorate.

Turning to Boyd, we note that his analysis employs one of the most fruitful ideas to come out of the voting studies, Philip Converse's concept of the normal vote. The question is whether the real-world situation to which Boyd applies this concept is sufficiently similar to that from which the concept was abstracted. The normal vote assumes a contest in which one Republican is running against one Democrat. Professor Boyd properly calls attention to the difficult challenge posed to such normal vote analysis by Wallace's candidacy in 1968, a year in which three candidates were able to win votes in the Electoral College.

Boyd handles this difficulty by assuming that there is one Republican (Richard Nixon), one Democrat (Hubert Humphrey), and one "short-term force" (George Wallace). An equally plausible assumption is that the normal Democratic vote is split between the non-Southern and Southern Democratic candidates. This simplification is reasonably consistent with the facts. Wallace's votes came principally from Independents (half of whose votes are assigned to the Democratic candidate by the normal vote calculations) and from Democrats. And the normal Democratic vote gives a good estimate of the combined Humphrey-Wallace vote. To take the statistics on the pace of progress toward civil rights from Boyd's Figure 1, for example, the normal Democratic vote is 68 per cent among those who think civil rights is going too slowly, 62 per cent among those who think the speed is about right, and 54 per cent among

Table 2. Reanalysis of Boyd's Data*

Issue	Short-Term Effect Scores	
	Assuming Norm. Dem. Vote to Humphrey Only	Assuming Norm. Dem. Vote to Humphrey + Wallace
Aid to Education	7.03	2.25
Mass Demonstrations	3.18	1.43
Escalation-De-escalation	7.21	1.27
Speed of Civil Rights	6.16	.92

* Short-term effect scores based on assumption that Humphrey was sole recipient of normal Democratic vote and on assumption that normal Democratic vote was split between Humphrey and Wallace.

those who think things are moving too fast. The combined Humphrey-Wallace votes for these three groupings are 70 per cent, 56 per cent, and 49 per cent, respectively. The estimates are rather close, and the deviations—+2, -6, and -5, respectively—accord with suspicions about the probable impact of the civil rights issue.

If we assume that the normal Democratic vote was split between Vice President Humphrey and Governor Wallace, we get a very different picture of the impact of the issues. Table 2 presents the results of a recalculation of Professor Boyd's "short-term component" measure. His figures are compared with those obtained by assuming that Humphrey and Wallace split the normal Democratic vote. The table presents these data for one issue from each of Boyd's four issue areas: social welfare, mass protest, Vietnam, and civil rights. One gets a very different estimate of the average magnitude of the issue effects, and by inference, a different notion of how important issues were when compared with other possible motivations. Changing the assumption about what happened to the normal Democratic vote also leads to a different ordering of the importance of the issues with respect to each other.

The problems we have been discussing relate to Boyd's estimates of short-term effects in 1968. They do not handicap his normal vote analysis of the escalation/de-escalation issue in 1964. Nor do they hamper in any way his ingenious computation of a long-term component associated with various issues (also presented in Appendix II). It is most useful to be able to measure the extent to which issues are or are not associated with traditional partisan divisions.

This leads to a paradox: The normal vote was conceived in order to be able to distinguish the *short-term* forces operating in a particular election; yet its best 1968 application resulted in a good measure of the *long-term* partisanship associated with given issues. One reason for this seeming inconsistency—the three-man race in 1968—is fairly obvious. Another is less apparent. It has to do with two ways of understanding the normal vote. One is the *conceptual distinction* between long-term and short-term forces. The other is the *operational selection* of party identification to represent the long-term forces. If long-term forces are to be represented by an attitude, then party identification is the prime candidate. This choice is clear enough, but the temporal distinction then implies that all other attitudes, may be considered as short-term stimuli. This inference is dubious. Attitudes concerning candidates who are not renominated are important only in single elections. Other attitudes, such as those about renominated candidates and issues that are salient for perhaps a decade, affect the outcome of two or three consecutive elections. Still other attitudes, such as those concerning economic policy and the desired locus of governmental power, have influenced voting choices for much more than a generation. The normal vote calculations control for the effects of party identification, the single most important long-term influence. This is essential. Still, we should be mindful that what is left is an admixture of short-term, medium-term, and long-term influences.

Distinguishing between attitudes on the basis of whether the self is an object is equally useful and more consistent. The party identification question—Generally speaking, do you consider yourself a Republican, a Democrat, an Independent, or what?—invokes both the self and a reference group. Since both the self and a salient group are objects, party identification should be regarded as a central organizing attitude.¹⁹ Other attitudes about candidates, parties, and issues tend to be less ego-involved and hence are less central. I find it useful to extend the metaphor of centrality and think of party identification as a nucleus surrounded by clusters of satellite attitudes, with each cluster composed of attitudes about closely related objects.²⁰

¹⁹ On centrality, see Theodore Newcomb, Ralph Turner, and Philip E. Converse, *Social Psychology* (New York: Holt, Rinehart and Winston, 1965), pp. 141-145; and Sperlich, *Conflict and Harmony*, chap. 6.

²⁰ The spatial metaphor rests upon the definition of "nucleus" as "a central point, group, or mass about which a gathering, concentration or accretion takes place" and "satellite" as "something attendant, subordinate or dependent." I want to avoid the concept

Each of the attitudes, as valenced cognitions, carries some positive or negative affect with respect to the immediate objects of the attitudes, and frequently toward the parties as well. The majority of the partisan valences on the satellite attitudes are of the same sign as the nuclear party identification attitude. There tend to be high correlations within clusters, for example, between attitudes about presidential and vice-presidential candidates of the same party, or between one's attitudes about school integration and job integration; but there tend to be low correlations between clusters: for example, one may think highly of a presidential candidate while disapproving of his position on civil rights. For the average voter, this cognitive structure is incomplete and inconsistent. Only those with a high level of political involvement are likely to have attitudes about a full range of political objects; only strong partisans are likely to have the same partisan valence on each of the attitudes.

Conceiving of a cognitive structure as a nucleus with satellites has some useful implications. So far as voting choice is concerned, it implies a difference between a first-order calculus, in which the vote is largely determined by party identification, and a second-order calculus used when party identification does not provide a guide (as with Independents who do not

lean toward either party) or when the net effect of the satellite attitudes leads one to depart from his normal voting habits. This suggests some effects of issues and our consequent need to pay attention to them. We do not—on grounds of parsimony—need to know about them when their effect is simply to reinforce a first-order decision to vote in accord with one's party identification. We very much need to know about issues—and their relative significance within the total set of satellite attitudes—when a second-order decision is being made.

This conception also hints at a long-range effect of issues. Party identification is resistant to change. Many have commented on this, but Hugh Douglas Price has made the additional observation that it is a lagging indicator.²¹ This suggests that the valence on the nuclear partisan attitude may be changed after most of the satellite attitudes come to have opposite valences. And in this respect, no one has better interpreted the effect of issues than Richard Boyd in a previous *Review* article:²²

In short, the impact of a candidate is substantial but of short duration. The impact of issues, while rarely great at any moment, accumulates over a period of time. Overall, issues may outweigh candidates in affecting the outcome of elections, for issues have the capacity to alter the greatest single determinant of a vote, party identification.

of "system" in discussing the cognitive structure of the average voter. It now appears there is too much inconsistency between the clusters, and too little ability to predict how a change in one cluster will affect other clusters, to justify the use of the system concept.

²¹ Hugh Douglas Price, "Micro- and Macro-politics: Notes on Research Strategy" in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge, Mass.: Harvard University Press, 1968), p. 121.

²² Boyd, "Voting Defection," p. 510.

Rejoinder to "Comments" by Richard A. Brody and Benjamin I. Page and John H. Kessel

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Only two presidential elections ago, V. O. Key worried about the effect of theories of electoral behavior on practicing politicians. "If they see voters as most certainly responsive to nonsense," he warned, "they will give them nonsense. If they see voters as susceptible to delusion, they will delude them."¹ Perhaps in response to Key's concern, the contributors to this symposium and other writers have sought to discover, and frankly to emphasize, the importance of issues in voting. I believe we have accomplished this central purpose. It is no longer "perverse and unorthodox," as in Key's time, to argue that "voters are not fools," and that elections are meaningful democratic acts.

This basic conclusion should be marked before discussing remaining and detailed points of contention. After reading two articles, two commentaries, and two rejoinders, one is likely to be overwhelmed by problems of operationalizing the "normal vote," or defining "liberalism," or calculating the "partisan issue alignment index." Of course, these are important scholarly problems, and Brody and Page particularly have outlined the immediate needs in future investigations of the subject. Rather than explore the subject further at this point, however, I want to give the patient reader some relief, and will respond only to a limited number of the comments.

John Kessel has presented an able summary of the recent literature on issue voting, and of the concomitant problems in analysis. I believe, however, that he finds more emphasis on the subject in *The American Voter* than truly exists. That book's six-component model of voter choice is not an independent test of issue voting, as Kessel states. Rather, it is based upon the same open-ended questions used to demonstrate low ideological awareness among the electorate. Moreover, these six components are devalued as only short-term factors, "the attitudes immediately supporting voting choice."² In the Michigan group's overall analysis, voter attitudes are clearly subordinated to party identification. To quote from the gospel, "We are

convinced that the relationships in our data reflect primarily the role of enduring partisan commitments in shaping attitudes toward political objects."³

Despite this emphatic conclusion, it is important to note that the relative importance of party identification and policy attitudes was not statistically tested in *The American Voter*. When Arthur Goldberg did perform such a test, using the same 1956 data, he found the Michigan model inadequate. Attitudes were seen to exercise an independent influence upon the vote.⁴

As to Professor Kessel's specific comments on my article, I need only respond to two points. First, I do not find the strictly statistical objections important. The absence of temporal variation from 1964 to 1968, when controls are introduced, only strengthens my argument that major political change occurred in the 1964 election and persisted in 1968. It is true that the partial correlations are lower than the zero-order relationships, but even the control generally reduces these figures by less than half.

More important is Kessel's substantive complaint that the questions employed (and my subsequent analysis) commingle two separate attitudes—the specific policy issues, and the general question of federal governmental power. He is certainly correct. Medical care or racial integration and the power of the national government are indeed joined in these survey questions. At more than one point in my article, I have noted that "liberal" and "conservative" denote attitudes toward the use of federal power for certain policy objectives.⁵

¹ Angus Campbell, Philip Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), p. 135.

² Arthur Goldberg, "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review*, 60 (December, 1966), 913-922. Independents are omitted from Goldberg's analysis, thereby increasing the relative importance of party identification.

³ The point was made in the original draft of this article which I sent privately to Professor Kessel for his comments. In his kind reply, he noted the overlap of the two attitudes, and I subsequently tried further to clarify the operational use of "liberalism." In his comments here, he is probably referring to my earlier draft, which has been improved, I hope, by attention to his private critique.

¹ V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1964), pp. 6ff.

² Donald E. Stokes, Angus Campbell, and Warren E. Miller, "Components of Electoral Decision," *American Political Science Review*, 52 (June, 1958), 368.

But these equivalences are more than stylistic conveniences, for they are joined in the real political world, where only the federal government has shown the ability and occasional willingness to insure minimum standards of health care for all, or to open segregated schools to black children. The vanishing breed of liberal Republicans may have a more sophisticated philosophy, but segregationists and medical lobbyists know that their political interests are well-served by the championing of states' rights. Moreover, to the not insignificant extent that the mass public is aware of these terms, its explanation parallels my use of these terms.⁶

Brody and Page make only one specific criticism of my article: that I do not consider the possibility of partisan realignment. To the contrary, I raised this very possibility by hypothesizing that 1964 represented a "critical election," in which significant numbers of vot-

ers changed their party loyalties to conform with their policy preferences, and Table 1 in my article presents data on this point. Admittedly, I have not demonstrated realignment, but I have hardly neglected its likelihood; indeed, I am more likely to have exaggerated it.⁷

A final, more general point on issue voting. In many electoral analyses, issues are considered only as short-term influences, distinct in their effect from partisan loyalty. *The American Voter* and Brody and Page's comments are partially illustrative. This assumption is analytically convenient, but it underestimates the long-term effect of issues. One of the sources of party identification itself is policy attitude, and attitudes first evident in one election may continue to be relevant and influential in succeeding contests. The obvious example is the contemporary Democratic majority, which can be traced to policy attitudes formed during the New Deal. The point is inherent and treated well in Boyd's analysis. It should be remembered by all electoral researchers, lest they again treat American party loyalists as unthinking prisoners of tradition and habit.

⁶See Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed., David E. Apter (London: The Free Press, 1964), pp. 219-223. A quick analysis of some other 1968 data indicated that over two-thirds of the sample could determine the relative conservatism or liberalism of the two major parties, and that 87 per cent agreed that the Democrats were the more liberal party (including 94 per cent of the strong Republicans). See deck 12, column 49 of the 1968 survey.

⁷See Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), pp. 119-122 and Donald J. Devine, *The Attentive Public* (Chicago: Rand McNally, 1970), pp. 95-99.

Rejoinder to "Comments" by Richard A. Brody and Benjamin I. Page and John H. Kessel

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The commentaries of Kessel and of Brody and Page raise fundamental questions of normal vote analysis. Kessel is concerned with the legitimacy of applying the technique to an election contested by three major candidates, given that we have normal votes only for a Democrat and a Republican. Brody and Page emphasize the problems of disentangling cause and effect in the relationships of issue positions and vote choices. Since a reply to Brody and Page would be moot if the objection of Kessel cannot be satisfied, I shall consider Kessel's commentary first.

Two Parties, Three Candidates

If we have normal vote estimates only for the Republican and Democratic parties, how should we treat the Wallace candidacy? My solution (defended in footnote 11) was to consider the Wallace vote a short-term deviation from the expected two-party vote and to keep the observed votes of the three candidates separate. An equally plausible assumption, Kessel argues, is that the normal Democratic vote was split between non-Southern and Southern Democratic candidates and that the expected Democratic vote should be compared with the *combined* vote of Wallace and Humphrey. While adding the Humphrey and Wallace votes might seem attractive (particularly to Hubert Humphrey), this solution has its own problems.

First, Kessel's treatment of Wallace as a Southern Democratic candidate is based on a premise that Wallace's votes came predominantly from Democrats and Independents. Although Independents did vote for Wallace in large numbers, a Wallace voter was no more likely to be a Democrat than was any randomly chosen number of the electorate (see Table 1). In fact, Wallace's party coalition no more resembled Humphrey's coalition than Nixon's.

Second, treating Wallace as a Democratic candidate not only distorts the candidates' party coalitions, it conceals important differences in their *issue* coalitions as well. Note, for example, the differences in the civil rights attitudes of those who voted for the three candidates. In all, 61 per cent of the electorate endorsed the view that civil rights leaders were pushing too fast. Eighty-seven per cent of the

Table 1. The Party Coalitions in 1968

	Demo- crats	Inde- pen- dents	Repub- licans		N
Nixon Voters	17	32	51	100%	490
Humphrey Voters	78	17	5	100%	421
Wallace Voters	46	41	13	100%	116
All Party Identifiers	46	30	24	100%	1531

Data Source: 1968 SRC Election Study

Wallace voters endorsed this view, as did 66 per cent of the Nixon voters; only 49 per cent of the Humphrey voters concurred. Adding the Humphrey and the Wallace vote obscures the fact that the racial attitudes of the Wallace voters were closer to Nixon voters than to Humphrey voters.¹

Finally, Kessel notes that if the Humphrey and Wallace votes are combined, the issue of the speed of civil rights progress almost disappears as an important issue in 1968. That conclusion in itself should make one chary of not distinguishing the Wallace and the Humphrey vote. Quite obviously, without the racial issue Wallace would not even have been a candidate in 1968. Thus, while I do not wish to minimize the problem of applying the normal vote technique to the 1968 election, I would argue that Kessel's particular method of reanalyzing the data is inappropriate.

Issue Voting or Rationalization

Brody and Page are concerned with another of the intractable problems of the analysis of issue voting. In their view the evidence that issues importantly influence people's votes can be explained away by what they term rationalization. Though they never define the term precisely, rationalization appears indistinguishable from what they later call projection and persuasion. Projection occurs when a voter falsely perceives his preferred candidate's position to be his own. Persuasion takes place if a voter adopts the issue position of his preferred candidate. In both

¹ This is true of all of the issues included in Figure 5 of my original article.

cases vote choices are influencing issue positions, rather than issues determining votes. Conceiving of rationalization as a response set, Brody and Page construct a test to determine whether or not rationalization in fact contaminates the evidence for issue voting. While their test tends to refute the rationalization hypothesis in the 1968 data, they have left the status of V.O. Key's work in doubt. To discuss Key's evidence, it is important to distinguish the cognitive and the affective components of attitudes.

Consider, for example, the following item from the Brody and Page commentary: "Which party can better handle the problem of keeping us out of a bigger war?" For most people, the question may be so heavily laden with affect that it has little cognitive content. It is like asking a person which of the parties he likes better in different words. Answering the question in such a way that the response correlates with one's vote requires neither interest in the issue nor any knowledge of the history of the two parties in committing the United States to war. A voter who had fixed on a voting choice would simply respond that this preferred party was better able to keep the United States out of war.

Contrast the above item with the following one, drawn from Key's analysis of the New Deal realignment: "Do you think the Wagner Act should be revised, repealed or left unchanged?"² The item gives no clues of the positions of the parties on this issue. If responses to this item are systematically related to the vote (i.e., if Republicans who favored the act voted Democratic and if Democrats who opposed it voted Republican), it is probably because the voters knew the differing Republican and Democratic positions on collective bargaining. An item such as this, then, has a substantial cognitive component. If an issue is salient enough that people become familiar with the parties' stands, then the issue position probably influenced their voting choices rather than vice versa.

In the 1968 data I presented, only one item is cast in such a way as to be vulnerable to a rationalization response set. It is the one which asks people, "In general, how do you feel about how President Johnson has done his job." The remainder of the items give away no clues of the positions of candidates or parties. But what of Key's data in *The Responsible Electorate*? In Chapter 3 Key outlines the class and issue bases of the emerging New Deal Democratic major-

ity. In the main his evidence depends on items that had a clear cognitive content. This portion of his argument stands up well on reexamination. Unfortunately, this is not as true of the later chapters on elections of the 1950s and 1960s. Here, many more of his items are of a form in which affect toward the parties or the candidates is a central component, e.g., "Which presidential candidate—Stevenson or Eisenhower—do you think could handle the Korean situation best?"³ Much of this evidence is suspect because of its possible contamination by rationalization.

The Normal Vote and Party Realignment

Brody and Page sound a second warning against normal-vote analysis. They point out that the technique considers issues "one by one in isolation from each other [so] . . . that the approach is continually reapportioning the same observed variation in the vote." Yet, in a sense this is one of its advantages. At a glance, one can see the degree to which voters who share a common position on a selected issue also support the same party and candidate. Differences in the expected party vote between groups with different opinions reveal the degree of ideological coherence of the parties on the issue at that point in time. If one then examines changes in the partisan components of an issue across a series of elections, one can show, as Pomper recently has, the pace at which the electorate has moved toward ideological clarity in its party attachments over time.⁴

In addition, the deviations of the observed vote from the expected vote point to the issues that appear important in changes in these party alignments. That is, in one election an issue may emerge which is associated with deviations from the expected party vote. By the next election enough people may have either changed their opinions or changed their party identification so that the issue cleavage is now manifest in a new party cleavage. This seems to have been the case with civil rights attitudes in the United States from 1956 to 1968.⁵ Normal vote analysis can therefore clearly demarcate the issues that underlie party realignments.

Brody and Page recommend loss-function models as an alternative to the normal vote. I conclude by posing one question of the most recent variants of loss-function equations pro-

² Key, p. 78.

⁴ Gerald M. Pomper, "Toward a More Responsible Two-Party System? What, Again?" *Journal of Politics*, 33 (Nov., 1971), pp. 916-40.

⁵ Pomper, pp. 925-26.

² V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), p. 47.

posed by Brody and Page and by Shapiro.⁶ These models, particularly Shapiro's, permit each voter substantial latitude in selecting the criteria on which he evaluates the candidates. They also allow each voter to make his own determination of the salience of these criteria and permit voters to misperceive the candidates' stands on issues. Undoubtedly, these equations should predict the votes of each individual with accuracy, but accuracy is not the only criterion for the evaluation of a theory. The cost of this increased accuracy of predicting individual votes may be that we cannot apply these theories to normative democratic theory or to analytical theories of party competition and elec-

toral change. For example, what can we say about democratic control of policy through elections, if our theories of the individual vote decision do not presume substantial agreement on the criteria by which people evaluate candidates. Similarly, what interesting propositions about party competition and electoral change can we generate if we cannot assume that people know with reasonable accuracy the policy positions of the parties.⁷ Unless the theories of the psychology of the vote decision are cast so that they can be integrated into broader theories of democratic control and party competition, then we may have solved less important problems at the expense of more critical ones.

⁶ Richard A. Brody and Benjamin I. Page, "The Assessment of Policy Voting: A Commentary," *American Political Science Review*, 66 (June, 1972), 450-458, and Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), 1106-19.

⁷ This undoubtedly is why Davis, Hinich, and Ordeshook assumed that "all citizens make identical estimates" of candidates' stands on issues. Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review* 64 (June, 1970), 431.

A Theory of Neighborhood Problem Solving: Political Action vs. Residential Mobility*

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People who don't like the way things are going in their neighborhood have three broad alternatives. First, they can resign themselves to the situation and, doing nothing, just tolerate it. This is no doubt what most people do in response to the minor problems of city life—things that irritate them but don't seriously inconvenience them—since finding a solution is very often costly in time, energy and money.

When problems are sufficiently bad or long-standing, however, they can (second) leave the neighborhood altogether, or (third) they can stay and make some attempt to change things. This paper is about what people decide to do in response to neighborhood problems, why they decide to do it, and the consequences of their decisions for the neighborhood itself. It is based on a simple choice model that is elaborated using survey data about people facing neighborhood problems in different parts of the metropolitan area.

Albert Hirschman has developed a general theory of response to decline in organizations that is based on the latter two options.¹ He points out that "exit" and "voice" are the alternative ways an individual can respond if he wishes to maintain some level of performance from an organization whose performance is de-

clining. Exit is simply quitting the organization or switching to a competing product. Voice is

... any attempt at all to change, rather than to escape from, an objectionable state of affairs, whether through individual or collective petition to the management directly in charge, through appeal to a higher authority with the intention of forcing a change in management, or through various types of actions and protests, including those that are meant to mobilize public opinion.²

Since Hirschman's terminology fits the options we have in mind exactly, we have adopted it here. A resident exiting from a metropolitan neighborhood simply decides that some other location offers him more of what he wants, so he moves to take up residence in the new place. A voicing resident, in the more usual terminology of political analysis, is "making demands," "articulating his interests," or "participating in political action." As Hirschman says, voice is a more "messy" option than exit;³ moving out of an area is one simple act, while voice can be protracted and complex. Voice can be individualistic, as in writing to newspapers, contacting public officials, signing petitions and voting, or it can be collective, as when people work through existing groups or form new ones.

Hirschman deals with cities only in passing, but many of his ideas are directly applicable to them. His main concern is with the consequences for the organization of the particular response its "customers" make to decline in performance; will it tend to further the decline, or will it tend to bring the organization back to better performance levels? This is our main concern with cities, too. Is neighborhood exit or neighborhood voice more likely to improve things that are going bad in a neighborhood? If one works better than the other, then it be-

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¹ Albert O. Hirschman, *Exit, Voice, and Loyalty: Response to Decline in Firms, Organizations, and States* (Cambridge: Harvard University Press, 1970).

² Hirschman, p. 30. Notice that the economic perspective adopted by Hirschman is paralleled by a social psychological perspective. "Avoidance" is essentially exiting behavior; "confrontation/attack" is essentially voice; and "tolerance" is equivalent to our term "resignation"—the situation when people experience problems but do nothing about them.

³ Hirschman, p. 16.

comes important to know about the conditions under which people are likely to do either.

Our general thesis is that voice, not exit, is the mechanism most likely to bring about improvement in metropolitan neighborhoods. This principle is not something we can demonstrate from our data, but it does have *prima facie* plausibility. When neighborhood problems arise from *density*, then mobility out of the area is likely to help things somewhat. But in most cases, exit will deplete a neighborhood of exactly those resources that are required to effect an improvement. Money is one of those resources; people who have it often use it to make the move to a more satisfactory neighborhood, thereby removing part of the area's tax base and, in general, weakening the area's ability to effect change.

Energy and concern are other resources lost to neighborhoods by exit. Hirshman makes this point in discussing differences among people in concern about quality and about price. Quality-conscious people, he argues, will be the first to exit when quality declines and—compared to price-conscious people—will be willing to pay higher costs for a different product. He illustrates this proposition in the case of neighborhoods: those who are most concerned about quality in schools, services, and neighborhood conditions will be the first to migrate to areas where those things are better, assuming, of course, that such areas exist. But these are the people most likely to get involved in community action if they *do* decide to remain. After they are gone, they “will be lost to the citizen's groups and community action programs that would attempt to stem and reverse the tide of deterioration.”⁴

On the other hand, we cannot make the easy assumption that neighborhood problems will be solved *to the extent* that residents make demands on their local governments for change. Governments vary greatly in their ability to take effective action, assuming that they are motivated to do so.⁵ Given the ability, they may not have the motivation. Michael Parenti cites case studies of attempts by ghetto blacks to get action on slum rentals and a needed traffic light to illustrate how well-organized col-

lective action can have no consequences at all.⁶ Crain and Rosenthal have argued that high levels of community participation can actually *decrease* the prospects for action on something the majority favors since the opposing minority is also likely to become mobilized and can often block action.⁷

Similarly, neighborhood problems are sometimes eliminated without any demands on the part of local residents; voice is neither a sufficient *nor* a necessary condition for change. “Spillover” from action taken by one local administration can improve things in another's territory,⁸ or changes can come about through the undirected working of economic forces. A “public-regarding” city elite might be expected to work for improvement without prodding from the residents of a neighborhood.⁹ Similarly, voice by people in one area can help people in another area who do nothing or exit. For example, if the residents of one suburb are affected by air pollution and are successful in attempts to curb it at the source, people in all areas benefit.

On the other hand, if the same suburban residents manage to increase the number of police patrols in their neighborhood, this will have little effect on crime in other areas—indeed, it might *increase* crime elsewhere by reducing police protection in order to meet the demand in the first area. The matter is complicated still further because demands are made on a number of different governments within the metropolitan area. Nevertheless, demands can make

⁴ Michael Parenti, “Power and Pluralism: A View from the Bottom,” *Journal of Politics*, 32 (August, 1970), 582.

⁵ Robert L. Crain and Donald B. Rosenthal, “Community Status as a Dimension of Local Decision-making,” *American Sociological Review*, 32 (December, 1967), 970–984.

⁶ See Alan Williams, “The Optimal Provision of Public Goods in a System of Local Government,” *Journal of Political Economy*, 74 (October, 1956), 18–33 for a discussion of “spillover effects” among local governments.

⁷ For a discussion of the concept “public-regarding elite” see James Q. Wilson and Edward Banfield, “Public-Regardingness as a Value Premise in Voting Behavior,” *American Political Science Review*, 58, December, 1964), 876–887. An important critique of the concept and analysis based on it is contained in Raymond E. Wolfinger and John Osgood Field, “Political Ethos and the Structure of City Government,” *American Political Science Review*, 60 (June, 1966), 306–326. We should remember that some kinds of neighborhood voice—using the term in the broad sense adopted by Hirschman—do not require action of any kind by city elites. Neighborhood beautification groups, for example, can make the required improvements quite independently of community power structures. But, in general, problems that can be solved in this way will be relatively minor.

⁴ Hirschman, p. 51.

⁵ We are not talking here about symbolic actions that are often a substitute for real change. This propensity of governments to respond symbolically to demands for change has been discussed with considerable insight by Michael Lipsky, “Protest as a Political Resource,” *American Political Science Review*, 62 (December, 1968), 1144–1158 and Murray Edelman, *The Symbolic Uses of Politics* (Chicago: University of Illinois Press, 1964).

an important difference. This view has been a basic assumption of liberal-democratic theory for years, and it will be our assumption here. *How much* the chances of action are increased by demand making depends on factors such as those just discussed.

Assumptions about Choice

The importance of Hirschman's work derives, in part, from his recognition that voice and exit are *alternatives* for a "customer" faced with "decline" in a "product." A large body of literature deals with the reasons why people leave a neighborhood, and another deals with participation in urban politics; but in neither of these—to our knowledge—is it recognized that these are alternatives, that a person might take political action *instead* of leaving, or vice versa. Studies of mobility usually see the only alternative to a move as accepting the present state of affairs in the current neighborhood, and studies of participation usually see the only alternative to action as apathy or resignation.¹⁰

Our data will document the propensity of people to exit or to voice when they are faced with a neighborhood problem. Unfortunately, we do not have direct data about why people in actual decision-making situations do one thing and not another. We *assume* that a person decides whether to exit, to voice, or to do nothing by weighing the costs of a particular action against the benefits likely to accrue from it. As a rational decision maker, he does that which offers him the greatest benefit for the least cost. For example, suppose someone living in a central district dislikes conditions in his neighborhood. For one reason or another, he decides that an attempt to improve things there would be very costly (in time, energy, and money) and, in the long run, would make little difference. While a move to the suburbs would involve an expensive house and perhaps higher

taxes, the difference in conditions there is vast. Rationally, he makes the move.

If these considerations were all the decision involved it is unlikely voice would ever be used much; exit is just too simple and "clean" a solution to a bad neighborhood. But in fact certain constraints limit the ability of people to use the exit option. Money is one such constraint. People with high incomes can choose from the wide variety of residential areas available in the metropolitan area, while people with low incomes have far fewer options. We usually think of high income people "escaping" the problems of the city—as often as not the "problems" are invading blacks—by a retreat to the suburbs, but if they wish they can also escape the problems of their suburb by a move to a different suburb, or perhaps to a city apartment. On the other hand, people with low incomes are confined to the options available in low-income areas (excluding high-rise luxury apartments), and a move is far less likely to involve a change in neighborhood conditions. Race also constrains the ability to exit. Substantial differences in neighborhood type within black ghettos have been documented,¹¹ but blacks are limited to the options offered by the ghetto.

The viability of the voice option differs among types of people and among types of neighborhoods. Pluralists and antipluralists differ about the extent to which political resources are distributed in the population,¹² but they generally agree that income and education confer substantial advantages. Lipsky has qualified this tenet somewhat by pointing out that "relatively powerless groups" can sometimes get much of what they want through protest; the skillful use of protest can mobilize other groups which have political resources to help a group that does not.¹³ But as Lipsky points out,

¹⁰ See, for example, Wilfred G. Marston, "Socio-economic Differentiation Within Negro Areas of American Cities," *Social Forces*, 48 (December, 1969), 165-176.

¹¹ Classic statements of the pluralist position are found in: Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961); Arnold Rose, *The Power Structure* (New York: Oxford University Press, 1967); Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); Edward Banfield, *Political Influence* (New York: Free Press, 1961). In addition to the earlier work of C. W. Mills and Floyd Hunter, antipluralist arguments are found in: Peter Bachrach and Morton Baratz, "Two Faces of Power," *American Political Science Review*, 56 (December, 1962); 950; Grant McConnell, *Private Power and American Democracy* (New York: Alfred A. Knopf, 1966); Theodore Lowi, "The Public Philosophy: Interest-Group Liberalism," *American Political Science Review*, 61 (March, 1967), 5-24.

¹² Lipsky, "Protest as a Political Resource," pp. 144-158.

¹⁰ Hirschman points out that both economists and political scientists have a "trained incapacity" to recognize the importance of one of the two mechanisms. Economists, he says, have a bias in favor of exit and against voice: "The economist tends naturally to think that his mechanism is far more efficient and is in fact the only one to be taken seriously" (p. 16). On the other hand,

Political scientists have long dealt systematically with [the function of interest articulation] and its various manifestations. But in doing so they have ordinarily confined their attention to situations in which the only alternative to articulation is acquiescence or indifference (rather than exit), while economists have refused to consider that the discontented consumer might be anything but dumbly faithful or outright traitorous (to the firm he used to do business with (pp. 30-31).

a successful protest requires formidable organizational and leadership resources, quite apart from some support in the general population. The political weakness of poor people is well summed up by Parenti:

Party politicians are inclined to respond positively not to group *needs* but to group *demands*, and in political life as in economic life, *needs* do not become *marketable demands* until they are backed by "buying power" or "exchange power" for only then is it in the "producer's" interest to respond. The problem with most lower-strata groups is that they have few political resources of their own to exchange.¹⁴

But some problems are beyond the resources governments have available, no matter what political resources or "purchasing power" a group manages to mobilize. Most obviously, this is the case for city governments in areas drained of resources by the movement of wealth to the suburbs; problems in such areas are normally far worse than those in the suburbs, and resources for dealing with them far smaller. Even with the greatest governmental good-will, voice may still be an ineffective strategy in such cases.¹⁵

Stated in these terms, the model implies that poor people are at a double disadvantage: they are handicapped in their ability to use both exit and voice for solving their neighborhood problems. In such cases passivity can be rational since a dispassionate assessment of the other alternatives could suggest that action of any kind would be a waste of time, energy, and money. And, of course, the situation is worse for blacks, whose ability to use exit or voice is also constrained by their race. Anthony Downs makes the point that political action (voting) can be irrational if policy changes have no net effect on individual welfare;¹⁶ we can add that exit, too, can be irrational if it makes no difference.

The Social Sciences on Exit and Voice in Cities

The literature on urban mobility has paid the greatest attention to movement from urban

areas to the suburbs; the dominant thesis is that migrants in this direction are mainly people who find urban life incompatible with their "family-oriented" life-style. Wendell Bell, for example, has shown that the main reason given for a move to the suburbs is that "it would be better for the children there"—more space, bigger houses, better schools, "nice" children to play with and so on.¹⁷ The conclusion is supported by a great deal of other research.¹⁸ The suburban move is best seen as one outcome of an interaction between the demands an individual places on his metropolitan neighborhood and what that neighborhood—and "competing" ones elsewhere—can offer.

Despite this emphasis on movement toward the suburbs (and other literature dealing with the "rootlessness" of suburban populations), urban areas appear to have higher rates of mobility than suburban ones, and most of the movement is *between* urban neighborhoods rather than *toward* the suburbs.¹⁹ General theories of metropolitan mobility differentiate between "push" and "pull" factors—things having to do, respectively, with negative characteristics of the present area and positive characteristics of the new one.²⁰ In these terms, Lansing and Barth conclude that dislike of the present neighborhood contributes most to a family's intention to move, followed by dissatisfaction with its present housing, overcrowding, and a desire to move either farther out or farther in.²¹ Sabagh and his colleagues conclude: "Needs

¹⁴ Wendell Bell, "Familism and Suburbanization: One Test of the Social Choice Hypothesis," *Rural Sociology*, 21 (September-December, 1956), 276-283.

¹⁵ For other literature dealing with movement toward the suburbs and familism, see: Roy H. Rodgers, "Some Factors Associated with Homogeneous Role Patterns in Family Life Cycle Careers," *Pacific Sociological Review*, 7 (Spring, 1964), 38-48; Sylvia Fleis Fava, "Suburbanism as a Way of Life," *American Sociological Review*, 21 (February, 1956), 34-37.

¹⁶ See John B. Lansing and Nancy Barth, *Residential Location and Urban Mobility: A Multivariate Analysis* (Report prepared for the U.S. Department of Commerce, Bureau of Public Roads, by the Institute for Social Research, University of Michigan, 1964). Richard Sennett also points out this greater "exit-proneness" of urban residents in *The Uses of Disorder*, (New York: Alfred Knopf, 1970), pp. 152-153, in his discussion of loose social controls in such areas.

¹⁷ For a useful summary of the findings in these terms, see George Sabagh, Maurice D. Van Arsdol, Jr., and Edgar W. Butler, "Some Determinants of Intrametropolitan Residential Mobility: Conceptual Considerations," *Social Forces*, 48 (September, 1969), 88-98. Charles M. Tiebout's "The Pure Theory of Local Expenditure," *Journal of Political Economy*, 64 (October, 1956), 416-424, is useful from an economic perspective.

¹⁸ Lansing and Barth, p. 14.

¹⁴ Parenti, "Power and Pluralism." Italics in the original.

¹⁵ Cf. Edward C. Banfield's assertion in *The Unheavenly City* (Boston: Little Brown, 1970): "... the 'revenue crisis' mainly reflects the fact that people hate to pay taxes and that they think that by crying poverty they can shift some of the bill to someone else" (p. 9). Crying poverty is, at least, more reasonable for most city governments than for most suburban governments.

¹⁶ Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), p. 43.

and aspirations pertaining to the dwelling and the neighborhood are one of the most important determinants of residential mobility."²² "Problems in the neighborhood" do seem to be the prime "pushing" factor behind exit from metropolitan neighborhoods; more attractive neighborhoods exercise "pull" relative to those problems.²³

One reason that exit has not been seen as an alternative to voice may be that the "political participation" literature characteristically fails to treat "participation" as a problem-solving act. Of course, there is good evidence that the act of voting is often better explained by the norm of civic duty than by any expectation that particular situations will be changed by it.²⁴ But people participate in politics in ways other than voting, and ample evidence suggests that there are strong norms *against* some of them, such as protest activity and demonstrations.²⁵ These more "difficult" behaviors seem more likely to be genuine problem-solving acts, and they are what we are chiefly interested in. By failing to emphasize this problem-solving character of much political participation, researchers have failed to consider that other kinds of actions—nonpolitical actions—might be a far more efficient way of doing things.

Although perhaps broad theoretical works do imply a problem-solving motive when they talk about "demand making," empirical studies of participation characteristically focus on status and psychological-attitudinal "determinants" of voting. High-status people are found to participate more than low-status people, men more than women, the middle-aged more than the

young and the old—and so on.²⁶ Alienation is often seen as a prior cause of low participation,²⁷ and the same role is seen for apathy. People fail to take part in political action because they are apathetic, or perhaps because they have a low sense of their political efficacy. *The American Voter*, for example, argues for a causal sequence from education "through ego strength to political efficacy and on to increased political participation."²⁸

The literature about black participation contains some anomalies that might, in part, be explained by changing historical circumstances. Although there are some exceptions, most research conducted in the 'fifties and early 'sixties found that blacks participate less in politics than whites do.²⁹ Later research, however, has shown that when socioeconomic status is controlled, blacks actually participate more than whites. Marvin Olsen found that this greater black participation occurred in a wide variety of social activities as well as politics, and also that it increased between 1957 and 1968.³⁰ The slowly changing racial climate of the 'sixties, coupled with increasing black dominance of the central areas, is probably changing traditional black political passivity.

From the perspective of our model, the usual explanations of political participation tell only part of the story. By *starting* with variables that are "inside the individual's head"³¹ they do not

²² Sabagh et al., p. 95.

²³ The literature suggests that "problems" that push people into mobility are, by comparison, seldom unique to a particular family in a neighborhood. For example, having to commute a long way to work is infrequently given as a reason for movement and the same goes for having friends in another part of the city. See: Peter Rossi, *Why Families Move*, (New York: Free Press, 1955), 90-92; Sabagh et al., "Some Determinants of Intrametropolitan Residential Mobility."

²⁴ See especially, Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1960) for a discussion of civic duty and similar variables related to voter participation.

²⁵ See: John P. Robinson, "Public Reaction to Political Protest: Chicago, 1968," *Public Opinion Quarterly*, 34 (Spring, 1970), 1-9; Philip E. Converse et al., "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), 1083-1105; Marvin Olsen, "Perceived Legitimacy of Social Protest Action," *Social Problems* (Winter, 1968), 297-310; Ralph Turner, "The Public Perception of Protest," *American Sociological Review* (December, 1969), 815-830.

²⁶ For a summary of findings regarding political participation, see Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1963). See also Angus Campbell et al., *The American Voter*; Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963).

²⁷ For an excellent summary of the alienated voter hypothesis and a critique, see Robert L. Crain, Elihu Katz, and Donald B. Rosenthal, *The Politics of Community Conflict* (Indianapolis: Bobbs Merrill, 1969).

²⁸ Campbell et al., p. 518.

²⁹ Campbell et al., p. 316. Morris Janowitz and Dwaine Marvick, *Competitive Pressures and Democratic Consent*, (Ann Arbor: University of Michigan Press, Michigan Governmental Studies, No. 32), p. 26.

³⁰ Marvin E. Olsen, "Social and Political Participation of Blacks," *American Sociological Review*, 35 (August, 1970), 682-697; See also Nicholas Babchuck and Ralph V. Thompson, "Voluntary Associations of Negroes," *American Sociological Review*, 27 (October, 1962), 647-655.

³¹ The phrase belongs to James S. Coleman, "Relational Analysis: The Study of Social Organization with Survey Methods," *Human Organization*, 17 (Winter, 1958-1959), 28-36. Coleman suggests that this shortcoming might reflect the heavy reliance these studies usually place on survey methodology. He points out the tendency of studies based on survey research to theorize about things that can be readily measured by this technique (that is to say, characteristics of individuals rather than of their environments); Coleman also systematizes the various modifications of

give due recognition to the social and political context of participation and how his perception of that context influences what the individual does. That is the crux of our model: An individual's participation is seen as a response to a need he experiences in his environment (or at least the neighborhood part of it), and as directed toward some agency, usually government, that has the power to do something about it. We suspect that attitudes fit most satisfactorily into an explanatory scheme as *consequences* of the way an individual assesses his social and political environment: that these assessments influence *both* attitudes *and* behavior, and that they are prior to both—although reciprocal relationships undoubtedly exist.³²

The behavioral literature on participation tells us little about how people assess different social and political environments. More generally, we get only sketchy impressions from the literature about the aggregate consequences of the multitude of individual exiting and voicing acts. The mobility literature does suggest that exit decisions result in a patchwork of more or less homogeneous neighborhoods throughout the metropolitan area: people obviously tend to seek out neighbors who are similar to themselves in status, race, and lifestyle. As the earlier discussion suggested, it is a far more complex process to link individual demand making to the outcomes of political systems and change in neighborhood conditions.³³

Subsequent sections of this paper ask: What neighborhood conditions produce exit, and what conditions produce voice? What will keep voice-oriented people in a declining neighborhood? Are there any differences between racial and status groups in their propensity to use one means of coping or the other? Are there any

survey technology that take account of the individual's social context.

³² Thus, if we think about alienation as a variable "inside the individual's head," at least two alternative models are possible: (1) a developmental sequence from assessments of the situation (the calculus) through attitude (alienation) to behavior (participation); (2) a spurious relationship in which the calculus influences alienation *and* participation, but alienation *in itself* does not influence participation. See Herbert Hyman, *Survey Design and Analysis* (New York: Free Press, 1955) pp. 254–263 for distinctions between these two types of process. See also Hayward Alker, *Mathematics and Politics* (New York: Macmillan, 1965), pp. 122–125.

³³ One important study suggests that decisions made by various metropolitan governments tend to maintain the social homogeneity of metropolitan areas. The argument implies that mobility and governmental action tend to work in the same direction of system maintenance. See Oliver P. Williams et al., *Suburban Differences and Metropolitan Policies* (Philadelphia: University of Pennsylvania Press, 1965).

differences between neighborhoods in this respect? Do people who relate differently to their neighborhood respond differently when faced with problems there? Micro-macro theorizing is, in general, underdeveloped in the social sciences, but several authors have pointed out its centrality to sociology and political science.³⁴ We cannot demonstrate the effect of different responses on neighborhood conditions themselves, but the concluding section draws on the data and the simple assumptions about aggregation that we outlined earlier to develop a model of problem solving in different metropolitan neighborhoods.

Data Collection and Coding

We used two types of data in this study—survey data to measure individual responses to problems, and aggregate data (from census tracts) to measure neighborhood differences. The survey was conducted in Columbus, Ohio in the spring of 1966.³⁵ In order to determine

³⁴ See, for example: Hubert M. Blalock, *Theory Construction* (New York: Prentice-Hall, 1969). Blalock points out that one might have to use weighted averages rather than arithmetic means in order to allow for the fact that, in collective decision making, some individuals count more than others (p. 6). James S. Coleman in *Introduction to Mathematical Sociology* (New York: Free Press, 1964) writes:

... it is precisely *because* there is some varying structure of relations between individuals that sociology has a reason for existence. Sociology would otherwise be nothing more than an "aggregate psychology," as thermodynamics is an "aggregate statistical mechanics," and would be concerned only with distributions of motives, needs, beliefs, and so on. Many sociologists today, in fact, forget that sociology is different from aggregate psychology and base their "sociological" explanations on such distributions" (pp. 88–89).

One author who does make a formal attempt to spell out such linkages is Allen Barton, *Communities in Disaster* (New York: Doubleday, 1969), especially Chapter 5, "The Altruistic Community." See also: Heinz Eulau, *Micro-Macro Political Analysis*, (Chicago: Aldine, 1969); and Donald E. Stokes, "Analytic Reduction in the Study of Institutions," a paper delivered to the 1966 Annual Meeting of the American Political Science Association in New York.

³⁵ The best available sample frame was the *Columbus City Directory*, which proved to be a reasonably reliable listing of households in the metropolitan area; our survey is representative of the entire metropolitan area. Households were sampled from this frame using a table of random numbers, and individuals were sampled from households using the respondent selection key detailed in Charles H. Backstrom and Gerald D. Hursh, *Survey Research* (Chicago: Northwestern University Press, 1963), pp. 50–59. Interviews were conducted by graduate students and advanced undergraduates in political science. Extensive telephone checking revealed complete reliability of the interviewers. Eighty-three per cent of the interviews attempted were completed.

whether people were experiencing problems in their present neighborhood, we asked: "Are there any problems facing this area of Columbus?" We coded a variety of types of problems in addition to the simple yes/no dichotomy. The question used to investigate voice was drawn from the set used by Almond and Verba to measure civic competence:³⁶ "Suppose a regulation were being considered by the Columbus City Government that you considered unjust or harmful. What do you think you could do about it?" The answers were coded into such categories as: work through informal groups; work through a political party; vote; work through formal groups (churches, unions etc.); write letters; talk to powerful people; consult a lawyer, and so on. The answers were also dichotomized: all those who indicated some kind of action were coded in a "voice" category, and those who said they would do nothing or who could not mention anything were coded "no voice." We have already pointed out that our questions do not relate to particular actions directed to particular problems—data that would have been ideal. Responses to this question do indicate *proneeness* to take action, however, and that is what we are interested in.

To measure "exit-proneess" another straightforward question was used: "Do you have any intention of moving in the next year or two?" Notice that the intention to move is not necessarily the same thing as actually making a move; it is possible that many such intentions were never carried out. But data from Rossi's study of urban mobility suggest that, in fact, a high proportion of people who indicate such intentions do act on them.³⁷ Once again the questions do not tie the individual's action to the particular problems he mentions in the area (as the model does), but, once again, *proneeness* to act tells us much of what we want to know.

The survey data were combined with census data in order to locate people in particular metropolitan contexts. A factor analysis across the 150 census tracts in Columbus identified a variable loaded on characteristics usually associated with the "urban-suburban" dimension, and each individual in the sample was assigned a factor score on this variable for the tract in which he lived.³⁸ This lets us define the individ-

ual's social context with some precision and also permits us to make some inferences about the political environment. We have trichotomized urbanism into "urban," "mixed" and "suburban" areas by cutting the range at plus or minus one-half standard deviation from the mean.³⁹ We should point out that these three "types" of neighborhoods are not necessarily contiguous areas. Mapping these factor scores shows, however, that the "urban" neighborhoods are, in fact, in the central part of the metropolitan area, the "suburban" ones are scattered around the periphery, and the "mixed" ones are—generally—between these two. We are measuring neighborhood *type*, not distance from the central business district, but the two are correlated. Notice also that "neighborhood" is defined operationally for each indi-

	1960	1950
% population Negro	.5695	.4992
% employees white collar	-.6710	-.5786
% homes owner occupied	-.7882	-.8300
Median years of schooling	-.6885	-.5969
Median income	-.8410	-.8042
% houses built since 1950	-.6639	-.6406
Female employees per 1000 male employees	.5832	.5665
% population married	-.7397	-.7260
Sex ratio (females per 100 males)	-.2294	-.3427

As will be noted, there is a strong correlation ($r = .92$) between the loadings based on the 1960 census and the 1950 census. The 1950 census was used to define the urbanism value of the area a respondent had *last* lived in "for any length of time" when he moved before Jan. 1st, 1956; the 1960 census was always used to define the urbanism value of the present place of residence.

This variable should not be confused with the Shevky and Bell urbanization or family status variable, although it is undoubtedly correlated with it. That measure is loaded on familism and life-style variables while ours, in addition, has status and ethnicity components. See: Eshref Shevky and Wendell Bell, *Social Area Analysis* (Stanford: Stanford University Press, 1955).

³⁹ Notice that this combination of survey and census data permits "contextual analysis" relating environmental variables to the individual's own responses. See James S. Coleman, "Relational Analysis." For other examples of contextual analysis, see: Peter M. Blau, "Structural Effects," *American Sociological Review*, 25 (1960), 178-193; James A. Davis, Joe L. Spaeth, and Carolyn Husen, "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review*, 26 (1961), 220; Ernest Q. Campbell and C. Norman Alexander, "Structural Effects and Interpersonal Relationships," *American Journal of Sociology*, 71 (1965), 284-289; Arnold S. Tannenbaum and Gerald G. Bachman, "Structural Versus Individual Effects," *American Journal of Sociology*, 69 (May, 1964), 585-595; Warren E. Miller, "One-Party Politics and the Voter," *American Political Science Review*, 50 (September, 1956), 707-725; Robert D. Putnam, "Political Attitudes and the Local Community," *American Political Science Review*, 60 (September, 1966), 640-654.

³⁶ Almond and Verba, *The Civil Culture*.

³⁷ Peter Rossi, *Why Families Move*. We used the stronger question "intention to move" in preference to the weaker one "desire to move."

³⁸ A standard factor analysis was performed. The loadings on the unrotated urbanism factor were as follows:

vidual in the study as the census tract in which he lives.

Parts of the analysis to follow require that people be located in neighborhoods they have lived in previously. Respondents were asked the exact address at which they had last lived "for any length of time," and the urbanism score for the appropriate census tract was added to their survey responses. The problem of measurement error, of course, comes up in both of the urbanism indices, but it is most acute for past urbanism. The interviews were conducted in the winter and spring of 1966, and the census data on which the "present" values were based were drawn from the 1960 census. The time lag no doubt meant that the structure of some areas was different when the respondents were interviewed from what it had been when the census was taken six years earlier. Little can be done but admit this possibility. For the "past" scores, the possibility was that the move took place some time *before* the data were collected. To cope with this problem, we adopted the rule that if the respondent said he had moved before January 1st, 1956, his past urbanism score was coded from the 1950 census; if he moved after that date the 1960 census was used. The factor analysis of the 1950 data revealed essentially the same factor as did the analysis of the 1960 data, and the same coding procedures were used to define "urban," "mixed," and "suburban" areas.

Findings

Since the fact of residential segregation makes important differences among the options available to whites and blacks when faced with neighborhood problems, we have analyzed the choice patterns of the two races separately. We have used education as an indicator of status, dichotomizing for simplicity so that those with a high school diploma or more are considered "higher-status" and those with less than that, "lower-status."

Our first major findings for the white population are:

A. Whites:

1. *Higher-status whites are prone both to voice and exit in the face of neighborhood problems, but somewhat more to voice.*
2. *Lower-status whites are more prone to exit than to voice in the face of neighborhood problems.*

The data are given in Table 1. We have correlated the dichotomous "problem perception" variable with two other dichotomous variables—exit-proneness and voice-proneness. For

Table 1. Exit and Voice among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Intending to Move and 'Political' Action Taking

	Problems and Exit	Problems and Voice
High Educated* (n=328)	.263 (p < .05)*	.321 (p < .05)
Low Educated (n=115)	.333 (p < .13)	.134 (p < .30)

* Education is dichotomized so that 'high' is high school diploma and 'low' is less than that.

* In this and subsequent tables Yule's Q is used to represent an association between two dichotomous nominal variables. The one-tail test was used to determine the significance after the Q value was transformed into a normal distribution Z score.

high-status people, voice-proneness and exit-proneness are both associated with perception of problems, but for low-status people only exit-proneness is associated with problem seeing. Our model suggested that high-status people have reason to expect both voice and exit to be reasonably efficient mechanisms, and the data support the argument that, in fact, they do attempt both when faced with problems. But the opposite was also suggested for low-status people: neither exit nor voice seemed like very efficient mechanisms for them. In order to explain why low-status people so decisively reject voice in favor of exit, we need to know more about the situation they are responding to. Individual status might not be as important as the type of neighborhood people live in or as the objective basis for expecting that exit might work and voice might not.

Tables 2 and 3 show the relation between experiencing problems in a neighborhood and, respectively, exit and voice when *both* education and urbanism are held constant. In other words, they show the propensity of people of different status to respond in different ways when they live in different areas. Response does depend, in part, on where a person lives. Specifically, for urban areas:

3. *The tendency for low-status whites to exit rather than voice is greatest in urban areas;*
4. *High-status whites are more prone to exit in urban areas than elsewhere, although they are also prone to voice in such areas.*

The data make it quite clear that high-status and low-status whites living in the urban areas reject the "political" solution in favor of the "non-political" one. Yet it is not that they do not *want* government to do anything; we asked

**Table 2. Exit among Whites in Different Areas;
Correlation (Yule's Q) Between Seeing
Problems and Intending to Move**

	Suburban Areas	Mixed Areas	Urban Areas
High educated	.184 (160) ($p < .22$)	-.081 (114) ($p < .39$)	.831 (51) ($p < .0001$)
Low educated	.316 (36) ($p < .27$)	.351 (45) ($p < .24$)	.287 (33) ($p < .29$)

respondents to name things they thought the city government should start doing—things it was not doing now—and the gamma between number of items mentioned and seeing problems in the neighborhood was .571 for the less educated population and .236 for the more educated one. These citizens have problems in their environment that might be dealt with by government, they believe government should be doing more than it presently is, and yet they withdraw from taking various political actions that, according to democratic theory, might be expected to get corrective actions out of government. Instead, they are prepared to move out of the district and try to find a better place to live. "Politically alienated" is a reasonable label to attach to such people; their alienation from government could be fully rational under some circumstances.

Hirschman's general theory of exit and voice suggests several possible reasons for this political alienation. First, sheer *number* of problems facing the resident of urban areas might make voice seem an unreasonably costly strategy in comparison to exit:

As voice tends to be costly in comparison to exit, the consumer will become less able to afford voice as the number of goods and services over which

he spreads his purchases increases—the cost of devoting even a modicum of time to correcting the faults of any one of the entities he is involved with is likely to exceed his estimate of the expected benefits for a large number of them.⁴⁰

We will return to the number and kind of problems people experience in different areas later; the point here is that a large outlay for voice in central areas might not go very far toward changing things in the direction wanted.

Second, we must take into account differing expectations that centers of power and decision making will respond to voice at all. In Hirschman's words: "... the decision whether to exit will often be taken *in the light of the prospects for the effective use of voice*. If customers are sufficiently convinced that voice will be effective, then they may well *postpone exit*."⁴¹ Parenti has shown that there is often good reason for the residents of central areas to be cynical about the prospects of voice bringing about the changes people want;⁴² we can reasonably argue that the comparative reluctance of both status groups to use voice in the central areas is a reflection of the same assessment. Whether because the problems are seen as too big for government, or because government is seen as unresponsive to voice, people simply do not believe that government will help them very much. In light of this argument, it is interesting to notice that low-status voice *outside* the urban areas is comparable to high-status voice; in the mixed areas low-status people are only slightly less willing to use voice, and in the suburban areas they are somewhat *more* willing. We have already mentioned the standard finding that low-status people participate less in politics than high-status people do. The present data suggest that this finding might be a consequence of the fact that low-status people live disproportionately in urban areas where political action taking is simply less rational than elsewhere. When the *situation* is different, low-status people appear to take such action as frequently as high-status people.

While voice is rejected by these urban residents, exit is an attractive alternative. It is usually far less costly to try to find a better neighborhood than to try to change an impossible situation in the present neighborhood through problematic political mechanisms. The decision to move is more easily made by high-status residents who have the financial resources to pick

**Table 3. Voice among Whites in Different Areas;
Correlation (Yule's Q) Between Seeing Problems
and "Political" Action Taking**

	Suburban Areas	Mixed Areas	Urban Areas
High educated	.146 (159) ($p < .464$)	.432 (112) ($p < .06$)	.476 (50) ($p < .16$)
Low educated	.247 (36) ($p < .30$)	.286 (44) ($p < .25$)	-.256 (32) ($p < .29$)

⁴⁰ Hirschman, *Exit, Voice and Loyalty*, p. 40.

⁴¹ Hirschman, p. 37. Italics in the original.

⁴² Parenti, "Power and Pluralism." See also Stanley Milgram, "The Experience of Living in Cities," *Science*, 167 (13 March, 1970).

Table 4. Relation between Exit and Voice among Whites; Correlation (Yule's Q) Between Intending to Move and "Political" Action Taking

	Suburban Areas	Mixed Areas	Urban Areas
High educated	-.504 (85) ($p < .09$)	.534 (58) ($p < .15$)	.533 (34) ($p < .20$)
Low educated	-.379 (18) ($p < .30$)	-.846 (14) ($p < .01$)	-.091 (15) ($p < .45$)

and choose a new neighborhood—as our model points out, the chances of successful exit are greater for high-status people. But exit is at least a *possible* alternative for low-status whites as well, and the fact that they are less likely to find a new neighborhood entirely to their satisfaction only dampens their willingness to try and does not eliminate it. The response of blacks—whose mobility options are constrained by segregation—to this inefficiency of the voice option will be dealt with later.

The data presented so far speak simply to the association between each of the response strategies taken separately and the perception of problems in the neighborhood. They do not tell us anything about the relation between exit and voice among different kinds of individuals, or about the frequency of the two in different kinds of neighborhoods. Neither do they tell us anything about the incidence of *resignation*—the rejection of both voice and exit as viable problem-solving devices. Table 4 includes only those respondents who reported problems in their neighborhood and shows (for whites) the correlation between exit and voice in different types of neighborhoods and for the two status groups. The significant fact that emerges is the *positive* correlation between exit and voice among high-status residents of the urban and mixed areas and the *negative* correlation among the high-status residents of suburban areas. Evidently, in the urban and mixed areas, the high-status residents most prone to voice are also the ones most prone to exit, while in suburban areas the opposite is the case: the high-status voice-prone population is most likely to stay in the area.

The finding is elaborated in Table 5, in which the respondents who experienced neighborhood problems are classified into four possible response-types: those who choose exit only, those who choose voice only, those who choose both, and those who choose neither. Our con-

cern is with the dominant patterns in the different types of neighborhoods. Propositions 5a, 5b, 6a and 6b summarize the differences between central and suburban areas; the data make it plain that the mixed areas are transitional between these two extremes.

5. Urban Areas:

- a. *The dominant pattern among the high-status residents is voice and exit together, with voice by itself a poor second.*
- b. *The dominant pattern among low-status residents is exit by itself, with neither exit nor voice (apathy or resignation) a close second.*

6. Suburban Areas:

- a. *The dominant pattern among high-status residents is voice by itself, with voice and exit together a poor second.*
- b. *The dominant pattern among low-status residents is also voice by itself, with the other three far behind. Apathy or resignation is much less frequent than among low-status residents in urban areas.*

Our model raises the question: What are the system-level consequences of the multitude of individual adaptations to neighborhood conditions? We argued that voice—particularly on the part of people with ample political resources—can reasonably be expected to increase the realization of sought-after outcomes. In the suburbs, the dominant strategy of both

Table 5. The Relation Between Exit and Voice among Whites; Percentage of Each Education Group in the Four Response Categories

	Suburban Areas	Mixed Areas	Urban Areas
A. High Educated:			
Response Type			
Exit only	7.1	1.7	5.9
Voice only	62.4	58.6	20.6
Exit and Voice	24.7	28.6	67.6
Neither	5.8	12.1	5.9
Total	100.0% (85)	100.0% (58)	100.0% (34)
B. Low Educated:			
Response Type			
Exit only	11.1	21.4	40.0
Voice only	55.6	57.1	13.3
Exit and Voice	16.6	7.1	13.3
Neither	16.7	14.3	33.3
Total	100.0% (18)	99.9% (14)	99.9% (15)

status groups is voice; the minority of the low-status population living in the suburbs respond in virtually the same way as their high-status neighbors. Equally important, these "voice-prone" suburbanites apparently intend to stay and fight, since only a few of them fall into the "exit-and-voice" category. Hirschman's discussion of quality-conscious people fits these residents well: faced with problems in their high-quality neighborhood they must voice in order to maintain their enjoyment of it, since other neighborhoods offer only low-quality options.

On the other hand, the "voice-prone" population of the urban areas is very largely also "exit-prone." Most voice in these neighborhoods comes from high-status residents, but the overwhelming majority of these activists are also people who plan a move out of the neighborhood. Our model pointed out that migration from a neighborhood was likely to reduce the area's tax base (since people who have money are likely to use it in making a move to a better neighborhood) and therefore lower its capacity to solve its problems. It also pointed out, borrowing from Hirschman, that the people who make the move will very often be the people who—had they stayed—would have expended the energy needed for neighborhood improvement. This general description seems to fit the urban areas. When high-status people move they are more likely (since they have the money) to move to an entirely different neighborhood, certainly a "better" one and probably in the suburbs. When they leave, they take with them taxable income and their political energy as well. We have no way of knowing which is

Table 6. Exit-Fatigue among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Exit and Voice by Number of Previous Addresses

	Problems and Exit	Problems and Voice
Number of other addresses:		
One	.200 (78) ($p < .28$)	.434 (76) ($p < .09$)
Two	.305 (94) ($p < .15$)	.432 (94) ($p < .08$)
Three-four	.273 (102) ($p < .20$)	.461 (99) ($p < .06$)
Five+	.280 (89) ($p < .19$)	.238 (88) ($p < .22$)

Table 7. Exit-Fatigue among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Exit and Voice by Length of Residence and Urbanism

	Length of Residence		
	-2 yrs	-6 yrs	6 yrs +
<i>A. Suburbs:</i>			
Problems and exit	-.179 (51) ($p < .33$)	-.425 (43) ($p < .17$)	.599 (104) ($p < .008$)
Problems and voice	-.098 (50) ($p < .42$)	.696 (42) ($p < .05$)	.212 (105) ($p < .27$)
<i>B. Mixed areas:</i>			
Problems and exit	.035 (40) ($p < .47$)	-.172 (41) ($p < .37$)	.216 (79) ($p < .27$)
Problems and voice	.679 (40) ($p < .03$)	.388 (41) ($p < .21$)	.344 (76) ($p < .14$)
<i>C. Urban areas:</i>			
Problems and exit	.294 (23) ($p < .32$)	.724 (14) ($p < .06$)	.900 (48) ($p < .0001$)
Problems and voice	.647 (23) ($p < .10$)	.538 (14) ($p < .19$)	-.167 (46) ($p < .36$)

the worst consequence for urban areas, but it is clear that both contribute to perpetuating neighborhood problems there.

Clearly, living in a suburban neighborhood increases the chances of an individual voicing, and living in the urban areas increases the chances of his exiting. Within such neighborhoods, however, several other factors seem likely to influence his choice of response. One of these is what might be called "exit-fatigue." If a person has made many moves in search of a satisfactory neighborhood, it seems possible that as time goes on he will become increasingly willing to stay and try to make something out of the place where he happens to be; voice, in other words, might increase with number of previous moves, and exit might decrease. A related hypothesis is that exit becomes a more frequent response as length of residence in a particular neighborhood increases, and that voice becomes less frequent. After having paid the costs of a move, people might be unwilling to pay them again for a while and during that time might be more willing to pay the costs of voice. Data relevant to these two hypotheses are given in Tables 6 and 7.

The first hypothesis is not supported: Table

6 shows no relation between the number of other addresses in Columbus and the tendency to exit or to voice. Evidently, the amount of moving one has done *before* coming to an area has no bearing on how one responds to the problems one faces *in the present*. On the other hand, Table 7 shows that length of residence in a neighborhood does have an impact on the strategy one is likely to use, and that this impact varies among types of neighborhoods.

The findings are summarized by propositions 7, 8, and 9:

7. *In urban areas, voice decreases rapidly with length of residence, and exit increases rapidly.*

8. *In the mixed areas, voice decreases with length of residence, but not as rapidly as in the urban areas.*

9. *In the suburban areas, the period between the third and sixth years of residence is a time of lowest exit and highest voice, after which voice decreases and exit increases.*

The hypothesis that people who have just made a move will be unwilling to exit for a while must be modified by the comparative instability of residents of the suburbs and mixed areas during the first two years; in these areas the period of greatest stability is from the third through the sixth year of residence. After an initial testing period during which a new resident "checks out" a neighborhood, if he likes it he becomes more committed. But these first years in the suburbs and mixed areas are, nevertheless, much more stable than the first years in the urban areas. If a period of "checking out" happens in the urban areas, there is no evidence that acceptance is very widespread, since exit snowballs from its already high rate to far higher rates. In the suburbs and mixed areas, "exit-fatigue" seems to have vanished after the sixth year, but in the urban areas it is much more rapidly overcome.

The other part of the hypothesis—that when people are "fatigued" by a recent move, they will be more likely to voice—does gain support from the data. In the suburbs, for example, voice is highest during the stable middle period, and declines during the next period as exit once again becomes acceptable. In the urban areas the two sets of correlations move in opposite directions: during the first period when exit is lowest, voice is highest, and during the last period when exit is highest, voice is lowest. When people are most reluctant to use the exit option, it seems that they are willing to turn to voice to deal with their problems.

Several other hypotheses dealing with influ-

ences on choice were tested. The first dealt with sunk costs in the neighborhood. We hypothesized that people who owned a home in a neighborhood would be most reluctant to exit and most willing to voice in the event of problems. This seemed reasonable since a home represents a financial investment that is likely to deteriorate with deterioration in the neighborhood; people with such an investment might be expected to fight in order to save it. Conversely, it seemed possible that exit would be lower among home owners. That this was not the case is shown by Table 8.

The second dealt with what Hirschman calls "entry costs." As he points out, a person who has paid very high costs for entering an organization will be more likely than others to reject exit in the event of deterioration and to try voice in order to keep things as he wants them: "... it may be expected that once deterioration is averted to, members of an organization that requires severe initiation will fight hard to prove that they were right after all in paying that high entrance fee."⁴³

It seemed quite plausible to hypothesize that the high levels of voice among suburbanites and the low levels of exit were a consequence of such "entry costs." Much of the present suburban population has migrated from the central areas and, in doing so, has paid a high price. Whether or not they own a home, their willingness to stay and fight might be a reflection of their determination not to make another move. In order to test this hypothesis, we drew on the measure of urbanism for the past place of residence. This enabled us to distinguish three categories of respondents: those whose move was

⁴³ Hirschman, p. 93.

Table 8. Home Ownership and Response among Whites: Correlation (Yule's Q) between Seeing Problems and, Respectively, Exit and Voice by Education

	Home Owners	Renters
A. High Educated:		
Problems and exit	.201 (201) (p < .19)	.358 (104) (p < .08)
Problems and voice	.121 (207) (p < .32)	.649 (105) (p < .006)
B. Low Educated:		
Problems and exit	.538 (75) (p < .06)	.149 (38) (p < .38)
Problems and voice	.179 (72) (p < .30)	.101 (39) (p < .41)

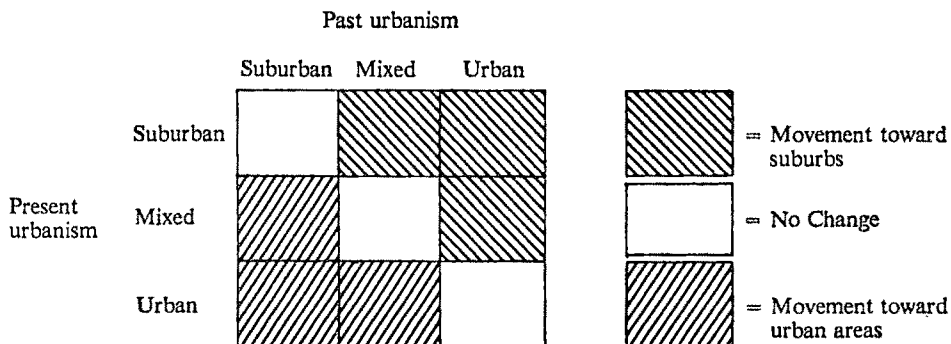
Table 9. Entry Costs; The Four Responses among Geographically Mobile People*

	Movement Toward Urban End	Movement Toward Suburban End
Rank order		
First	Voice only (58.8%)	Voice only (46.9%)
Second	Exit and Voice (22.1%)	Exit and Voice (37.5%)
Third	Exit only (10.3%)	Neither (12.5%)
Fourth	Neither (8.8%)	Exit only (3.1%)
Total	100.0% (68)	100.0% (32)

* Cases include only those seeing problems.

toward the more suburban end of the urbanism continuum, those whose move was toward the more urban end of the continuum, and those whose move was between essentially similar areas—whether they were urban, mixed or suburban areas.⁴⁴ We examined the dominant pattern of responses among those who reported problems in their neighborhoods. The data are given in Table 9. Some evidence in support of the hypothesis comes from the fact that the total incidence of voice is higher among those who made a move toward suburban areas, but the difference is not great and is based on a large number of respondents who both exited *and* voiced after the suburban move. Making finer distinctions among mobility types leaves us with too few cases for reliable estimates, but does not give any evidence in support of the hypothesis, either. Generally, the similar rank ordering of the two broad types suggests that the past residential history of a person has little impact on his mode of response to problems where he lives at present. This finding is consistent with the data showing that the number of moves made before arriving at the present

⁴⁴The categories were defined as follows



neighborhood has little impact on the choice of a strategy. Our overall conclusion is that the choice of a strategy for responding to neighborhood problems depends—in addition to the factors spelled out in the model—on the type of neighborhood a person lives in and how long he has lived there.

We also tested and rejected the hypothesis that social integration into a neighborhood would make exit less likely and voice more likely. We expected, very simply, that a person who had friends in the neighborhood would be more reluctant to leave than one who knew nobody there; we also expected that he or she would be more likely to be drawn into attempts at remedial action for the neighborhood. We asked the question: "Do most of your friends live around here, or do they live somewhere else?" A few respondents who said they had friends "both here and elsewhere" were coded as "friends here." The data do not support the hypothesis—in fact, they are the opposite from what we expected. They are given in Table 10.

Table 10. Location of Friends among Whites; Correlation (Yule's Q) Between Problems and, Respectively, Exit and Voice by Education and Location of Friends

	Friends Around Here	Friends Elsewhere
A. High Educated:		
Problems and exit	.523 (140) ($p < .006$)	-.014 (179) ($p < .48$)
Problems and voice	.255 (141) ($p < .22$)	.427 (173) ($p < .05$)
B. Low Educated:		
Problems and exit	.517 (51) ($p < .08$)	.042 (60) ($p < .47$)
Problems and voice	.048 (49) ($p < .45$)	.139 (60) ($p < .36$)

We have no ready explanation for the propensity of people with friends in the neighborhood to be *more* ready to leave and *less* likely to voice than people without friends there. This is not a function of different friendship patterns in different neighborhoods since holding urbanism constant makes no major difference to the pattern. We can, however, draw the limited conclusion that:

10. *Social integration into an area does not make one more likely to stay and fight when problems there arise.*

It seems that if a local community of "friends and neighbors" was ever the basis for political action in American cities, it is no longer so. We suspect that easy access to all parts of the city reduces the importance of living close to one's friends, so that community, while it has not ceased to exist, is no longer readily defined in geographic terms.⁴⁵

Our conclusion for whites can be summed up as follows: While status (defined by education) makes some difference in selecting a strategy independently of area characteristics, other individual-level differences (home ownership, integration in the neighborhood) make no difference. The most important factor seems to be the type of neighborhood one lives in and the length of time one has lived there.

B. Blacks:

As we pointed out earlier, blacks experiencing problems in their neighborhoods face a fundamentally different situation from that of whites experiencing problems. In many respects their situation is similar to that of a purchaser who is obliged to buy his goods from a monopolistic enterprise. Hirschman's hypothesis is that a monopoly situation is likely to breed voice, since voice is the only available way of resisting decline in the product:

The actual level of voice feeds on inelastic demand, or on the lack of opportunity for exit . . . [In this situation] the role of voice would increase as the opportunities for exit decline, up to the point where, with exit wholly unavailable, voice must carry the entire burden of alerting management to its failings.⁴⁶

Of course, individual blacks do have opportunities for movement out of the ghetto, but segregation effectively constrains the movement of most in the population. Blacks are obliged to

"purchase" their residential neighborhood in a severely restricted market.

Nevertheless, the potential of the voice mechanism for blacks is also notoriously limited. We have already cited Parenti's case studies showing the ineffectiveness of attempts to make modest changes in certain ghetto neighborhoods in Newark.⁴⁷ Many commentators make the same point. The important question is: Given these limits on both exit *and* voice, what do central city blacks *do* in the face of neighborhood problems?

Because of the small number of cases, our data on blacks must be taken with reservations;⁴⁸ nevertheless, they are highly suggestive. The first proposition that gains support is:

11. *Blacks are more likely to voice in response to problems than are whites of similar status who live in similar urban areas.*

Table 11 shows the distribution of response types among blacks who reported problems in their neighborhood; the corresponding data for whites are contained in Table 5. The striking difference is the comparative frequency of voice *by itself* among blacks: 40.0 per cent of the more highly educated blacks take this option compared to 20.6 per cent of highly educated whites living in urban areas, and 35.7 per cent of less educated blacks compared to 13.3 per cent of less educated whites living in urban areas. Our data are thus consistent with Olsen's earlier mentioned finding that when status is controlled, blacks are now participating in social action more than whites.

We also pointed out that the high frequency

⁴⁵ Parenti, "Power and Pluralism."

⁴⁶ Blacks are about 13.3 per cent of the population in the Columbus SMSA. Our sample of the area included 13.9 per cent blacks. Because we did not over-represent blacks, we have only 85 cases to deal with. Care was taken to ensure that black students did the interviewing in black neighborhoods, and white students in white neighborhoods.

Table 11. Relation Between Exit and Voice Among Blacks; Percentage of each Status Group in the Four Response Categories by Education*

	High Educated	Low Educated
Response Type		
Exit only	5.0	14.3
Voice only	40.0	35.7
Exit and Voice	45.0	28.6
Neither	10.0	21.4
Total	100.0	100.0
	(20)	(14)

* Cases include only those seeing problems.

⁴⁵ A similar point has been made by Stanley Milgram in his discussion of adaptations to overload in metropolitan areas. See his "The Experience of Living in Cities."

⁴⁶ Hirschman, p. 34.

of voice and exit together among high-status white residents of the urban areas was likely to drain those areas of political energy that might help toward recovery. Nathan Wright has argued that among blacks, a similar departure of middle-class residents from the urban areas represents an "incalculable loss politically."

. . . [this departure] has represented a hazardous drain upon a resource crucial for the new civic, political, and economic adjustments the cities have to face. . . . While other rising ethnic groups built power blocs that gave them a voice in the determination of civic, political, and economic affairs, black men and women possessed of the greatest leadership possibilities followed the path of flight to the suburbs.⁴⁹

Our data (admittedly scant) suggest that the problem is present among high-status blacks, but it is not as crucial as for whites: only about half of the voicing population is also exit-prone. Evidently, the financial resources that make exit the easiest and best strategy for many high-status whites in these areas do not function in quite the same way for high-status black residents of the ghetto. This is consistent with the hypothesis that constrained opportunities for exit—such as face blacks in most American cities—result in heightened voice. Although the data are weak, we can speculate that this fact makes the prospect for solving problems better in the ghetto than in the surrounding white areas, assuming the problems are of equal severity.

With low-status blacks as well as high-status blacks, the *total* voicing population is far larger than among whites of similar status: together the "voice only" and "voice and exit" populations constitute 64.3 per cent of the low-status black population compared to 26.6 per cent of the low status white population in the urban areas. Once again, though, about half of these "voice-prone" blacks are also "exit-prone." The *comparative* proneness to voice among ghetto blacks provides a firm basis for attempts to organize the black community into effective political action, but the main obstacle to such attempts appears to be the *temptation* of exit—despite the fact that residential moves by blacks are almost always within the limits imposed by the ghetto.⁵⁰

Table 12 shows, for blacks, the relation between seeing the neighborhood's problems and, respectively, exit and voice when length of resi-

Table 12. Correlation (Yule's Q) Among Blacks Between Seeing Problems and, Respectively, Exit and Voice by Length of Residence

	Length of Residence		
	-2 yrs	-6 yrs	6 yrs+
Problems and exit	.250 (17) ($p < .36$)	.909 (20) ($p < .0001$)	.366 (36) ($p < .23$)
Problems and voice	† (16)	.362 (20) ($p < .27$)	.546 (37) ($p < .07$)

† The distribution of cases in this cell made the Yule's Q meaningless.

dence is held constant. The trend it reports is almost the opposite of that for urban whites (see Table 7). Among whites in these areas, exit increases rapidly with length of residence and voice decreases rapidly, but:

12. *Among blacks, exit reaches a peak a few years after arrival but declines thereafter.*

13. *Among blacks, voice increases with length of residence.*

The earlier data showed that, among whites, exit during the first two years of residence was more frequent in the urban areas; evidently, the "fatigue" arising from a recent move is not sufficient to stop white residents of urban areas from planning another move soon after arrival. Ghetto blacks are similar to urban whites in this respect and similar also in the rapid increase of exit during the next few years. They are different, however, in the decline of exit beyond that and in the increase of voice. It is as if the realities of constrained exit opportunities are finally driven home by repeated disillusionment with "new" neighborhoods. When a decision is finally made to stop looking, it seems that a parallel decision is also made to start fighting.

For both blacks and whites we tested the hypothesis that age would make a difference in the selection of a strategy; we expected that older people would have "deeper roots" in their neighborhood and, because of that, would be less likely to exit and more likely to voice. Table 13 breaks respondents into two broad age groups—above 35 and below 35. It shows the correlation between seeing the problems and, respectively, exit and voice when race and age are taken into account.

In nonurban areas there was little difference attributable to age, and Table 13 includes only whites living in urban areas. Perhaps the slight

⁴⁹ Nathan Wright, *Ready to Riot* (New York: Holt, Rinehart and Winston, 1968), pp. 60–62.

⁵⁰ As expected, our data show that virtually all black residential mobility does take place between areas at the extreme "urban" end of the urbanism continuum.

Table 13. Effect of Age; Correlation Between Seeing Problems and, Respectively, Exit and Voice By Race and Age

	Below 35 Years	Above 35 Years
A. Whites		
Problems and exit	.149 (157) ($p < .26$)	.337 (290) ($p < .05$)
Problems and voice	.347 (153) ($p < .13$)	.260 (288) ($p < .08$)
B. Blacks:		
Problems and exit	.684 (20) ($p < .10$)	.307 (53) ($p < .22$)
Problems and voice	.905 (20) ($p < .0001$)	.421 (53) ($p < .11$)

differences among whites in these areas suggest that with increasing age the preference for exit over voice that we observed at all ages increases. The most interesting age comparisons, however, are among blacks:

14. *Younger blacks are substantially more prone to exit and to voice than older blacks.*

Young blacks are more prone to voice than they are to exit, but the most characteristic thing about them is that they are very likely to do *either*. Our data support the standard observation that the restlessness of the ghetto at present is largely a restlessness of youth. But they elaborate that observation by showing this restlessness to be geographic as well as broadly "political."

The problem for those organizing black political action is to overcome this residual tendency to exit in the face of bad neighborhood conditions. This tendency can be reduced by making exit still less attractive or by making voice *more* attractive or both. For collective action to be sustained or even to get started, it must have reasonably good prospects for succeeding, a condition that rarely holds for ghetto blacks. Lipsky argues that evidence of success is necessary to sustain a protest movement,⁵¹ but if there is no such evidence then reliance may have to be placed on charisma and other "nonpolitical" appeals. It seems likely, though, that the increasing black dominance of central city politics will change such assessments. Newark, where Parenti found such high levels of

cynicism about political effort, now has a black mayor, and so do several other cities. If exit remains a limited strategy for blacks—and there is evidence that it will since segregation in housing appears to be *increasing*—then these changes may cause voice to increase to unprecedented levels. In short, segregation *plus* viable voice opportunities seems one recipe for massive political action.

Implications

Figure 1 is a decision-tree showing the various paths to voice, exit and passivity. Notice that, for purposes of simplicity, we have made the exit decision come first in the sequence, but there is no necessary priority of this decision over the voice decision; we only suggest that both decisions are made at some time by people experiencing problems. Notice also that there are two paths to each of the three possible behaviors. One can get to passivity, for example, either because there are no problems experienced or because there are no solutions perceived.

Our main interest, however, is in the patterns that seem characteristic of different kinds of metropolitan neighborhoods. We have identified three such patterns, remembering that we are talking only about central tendencies. The first is the sequence for blacks in the ghetto: if problems are experienced, exit is judged impossible, and voice is judged possible. An earlier set of political realities, we suspect, meant that voice was also judged impossible, resulting in terminal passivity for most blacks. The second sequence is for urban whites: if problems are experienced,

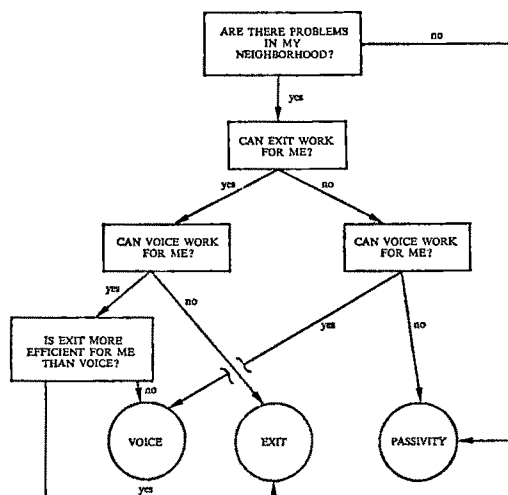


Figure 1. Decisions leading to exit, voice and passivity.

⁵¹ Lipsky, "Protest as a Political Resource."

exit is judged possible and voice either more problematic or impossible resulting in terminal exit. The third sequence is for suburban whites: if problems are experienced, exit is judged possible and voice is also judged possible. We have suggested that the decision between exit and voice is most likely to be decided by such considerations as quality consciousness, the absence of still more attractive neighborhoods to move to, and the viability of political processes. Like ghetto blacks, but for very different reasons, suburban whites decide to stay and fight.

The important difference is in feedback to neighborhood conditions themselves. Magoroh Maruyama has made a distinction between deviation-counteracting and deviation-amplifying social processes.⁵² Traditional cybernetics, he argues, has been mainly concerned with the former—processes whereby a system of interacting parts moves toward some stable equilibrium. "The second cybernetics," as he calls it, deals with processes that move a system of interacting parts away from such a stable equilibrium. In some situations, "whatever the change, either an increase or a decrease, amplifies itself."⁵³ In suburban neighborhoods, problems characteristically produce voice, which, according to our model, contributes to solving those problems. As the problems disappear, people are increasingly freed from the need to decide between exit and voice and can sink into happy passivity. Problems produce responses that eliminate the problems. In urban areas that are white, on the other hand, the appearance of problems triggers exit which, we have argued, only makes things worse by draining the area of its tax base and its political energy. This is clearly a deviation-amplifying process: problem solving in these areas makes problems there get worse and worse.⁵⁴

The interesting question about ghetto neighborhoods is this: will voice continue to be seen as a viable option? We have suggested that this depends, to some extent, on the success blacks have in gaining control of central city political institutions. But it also depends on the availability of resources sufficient to do something about ghetto problems. Without such resources, we can well project a future in which blacks once again lapse into political passivity. This

time it would be based on actual experience with the futility of voice, and we can expect that the attendant frustrations would be significantly greater than now. But this is to relax our assumption that voice is likely to improve things, and without projecting to such a depressing future, we seem to have identified another deviation-counteracting feedback process: problems in the ghetto produce adaptations that make problems there get better and better.

The question still remains to be answered: *What* problems are being dealt with by the residents of the different neighborhoods? Table 14 reports the percentage of all respondents mentioning a problem and the mean number of problems they mentioned in the three types of neighborhoods and in the black ghetto. Although, as we might expect, whites in urban areas mention somewhat more problems—and more of them mention problems—than whites elsewhere, there are not major differences by neighborhood. Problem-awareness is fairly constant in all areas, it seems.⁵⁵

On the other hand, the actual problems mentioned differ very sharply among types of neighborhood. Table 15 reports the problems that were mentioned first by those respondents who did mention something; it gives the percentage distribution of all problem mentions in the three types of neighborhood and the ghetto.

⁵² Because urban residents now tend to be less educated than suburban residents, we should expect that a greater proportion of suburbanites would mention more problems; education is usually associated with articulate and full responses to questions such as these. But in the suburbs the two education groups are just about the same in this respect: 54.0 per cent of the more highly educated mention problems and 50.0 per cent of the (minority) less educated population do so. The predominance of more educated people in the suburbs does not inflate the aggregate number of problem mentions.

On the other hand, educated people are more articulate when they have something to talk about: 69.2 per cent of the more educated white respondents living in urban areas mention one or more problems,—i.e., more than their status counterparts in the suburbs—while only 54.3 per cent of the less educated respondents do.

Table 14. Concern about Problems in Different Types of Metropolitan Neighborhoods

	Sub-urban Areas	Mixed Areas	Urban Areas	Blacks
Percentage mentioning any problem	50.2	42.9	60.2	44.2
Mean number of problems mentioned	.79	.70	.83	.72
Total population	213	177	93	86

⁵² Magoroh Maruyama, "The Second Cybernetics: Deviation-Amplifying Mutual Causal Processes," *The American Scientist*, 51 (1963), 164-179.

⁵³ Maruyama, 177.

⁵⁴ Hirschman makes a similar point (p. 122) by classifying "inner cities" as "organizations" that are primarily sensitive to voice but in which decline in performance arouses mainly exit.

Table 15. The Percentage Distribution of Problem Mentions, by Urbanism and Race

	Suburbs	Mixed Areas	Central Whites	Central Blacks
Problem mentioned:†				
Environmental quality	4.8	19.2	21.7	17.8
Crime	22.5	23.4	30.4	32.1
Schools	24.2	17.0	0.0	7.1
Transport	29.0	14.9	8.2	10.7
Slums/housing	6.5	12.8	39.2	28.5
Race related	12.9	12.8	0.0	3.6
Total mentions (1st item)	62	47	40	28

† Coding: Environmental quality included such things as air pollution, water pollution, beautification in general.

Crime included such things as police problems, youth violence, street lighting, lack of safety on the streets.

Schools included such things as their general inadequacy, the need for more and better schools, discipline problems.

Transport included such things as the inadequacy of public transport systems, their cost and poor coverage, difficult access to other parts of the city.

Slums/housing included such things as the need for slum clearance, the problems from slum clearance, high rents, the difficulty of finding a house.

Race included such things as blacks moving in, the dislike of proximity to blacks, racial difficulties in general.

These data are best interpreted as giving the *priority* of concerns in different areas. The greater frequency with which schools are mentioned in the suburbs supports the general thesis referred to earlier about the familistic concerns of suburbanites, but the fact that the schools are not mentioned by urban whites does not necessarily mean they are not seen as a problem by them. It does mean that other things—such as housing problems—have a greater urgency and are mentioned first. Only in matters related to crime do the residents of all areas have nearly equal concern; although police records show crime decreasing precipitously from the central areas outward, suburbanites nevertheless feel a threat from crime that is comparable to the threat felt by urbanites.⁵⁶

⁵⁶ Thus, in the urban areas, but not in the suburbs, there does appear to be a sound base for "law and order" as an issue. The Columbus City Police keep unusually good records of the incidence of crime by census tract. See 1965 *Statistical Supplement to the Annual Report of the Columbus City Police Department*.

The important matter is the correlation between the problems experienced by the residents of different neighborhoods. As expected, suburban whites and urban whites differ sharply: the rank-order (ρ) correlation between the kinds of problems mentioned in the two areas is $-.42$. On the other hand, the correlation between the problems reported by whites and blacks in the central areas is nearly perfect. Whites who live in the suburbs and the central areas have very different concerns, but the blacks and the whites who live in neighboring areas of the central city have virtually identical problems to deal with.

Earlier, we drew on Hirschman's suggestion that many problems, and complex ones, might make voice an inefficient mechanism. Banfield has pointed out that:

For something like two-thirds of all city dwellers, the urban problems that touch them directly have to do with comfort, convenience, amenity, and business advantage . . . these are "important" problems but not "serious" ones. In a great many cases, these problems cannot even be called important; a considerable part of the urban population—those who reside in the "nicer" suburbs—lives under material conditions that would be hard to improve upon.⁵⁷

The present data support our suspicion that suburban problems are "easier" to deal with, and therefore more likely to provoke voice; problems such as environmental quality, crime (the *reality* of crime), and poor housing are likely to be more "difficult" for the urbanites who experience them than are the transportation and school problems that plague suburbanites.

One intriguing prospect is that the neighborhood problems of central city whites might be reduced by the voice behavior of ghetto blacks. We have shown them to experience similar problems, and we have also shown that, while whites characteristically try to leave the area to make things better for themselves, blacks characteristically take some kind of action; we drew the inference that black voice was likely to increase greatly in the near future. Earlier analysis of the present data has shown that low-status white hostility toward blacks is greatest when they live close by;⁵⁸ one plausible projection to the near future is that the neighborhood problems of these central city whites might be ameliorated by spillover from the political ac-

⁵⁷ Banfield, *The Unheavenly City*, pp. 11–12.

⁵⁸ John M. Orbell with Kenneth S. Sherrill, "Racial Attitudes and the Metropolitan Context: A Structural Analysis," *Public Opinion Quarterly*, 33 (Spring, 1969), 46–54.

tivity of neighboring blacks—in short, by the Black Power movement.

Although rational grounds appear to exist for a political alliance between ghetto blacks and central city whites, the realities of racial hostility are likely to hinder its realization. Perhaps the political parties, particularly the Democratic party, might provide an organizational basis and an arena for conflict to the benefit of both races, but this possibility depends on the extent to which the parties are accepted by blacks as useful.⁵⁹ A hindrance of a different kind, however, might be the disinclination of central city whites to engage in political action *at all*. The question we must ask is: What changes would increase the willingness of whites in the central city to take corrective action in their *present* neighborhoods? Quite obviously, it is impossible to make major reductions in central city exiting behavior, for exit is just too easy and readily available a solution to what is bothering people. But some modest interventions might make equally modest changes that might have, in turn, significant consequences. One logical possibility is to increase the costs of entry into the suburbs, thereby making it more rational to stay in the present area and fight. Higher suburban taxes, for example, might have this effect. But in general, we might anticipate that such tactics would only limit the movement of those anyway least able to make the move; those with financial resources (whom we need in the city) would be able to overcome this barrier.

A second and more reasonable possibility is to increase the prospects for successful voice in the central areas. Richard Sennett, among others, has mourned the passing of the political machine as a device for linking individual to political decision making:

⁵⁹ For differing views on the propriety of blacks working through the parties, see: Joyce Gelb, "Blacks, Blocs & Ballots," *Polity*, 3 (Fall, 1970) 46-54; James Q. Wilson, "The Negro in Politics," in *American Ethnic Politics*, ed. Lawrence Fuchs (New York: Harper and Row, 1968) pp. 217-246.

For insofar as the reform crusades have succeeded in amending the past, the little guys, the white working-class and lower middle-class voters, have lost their sense of connectedness with the body politic; when the machines lost a personalistic character, the little guys were isolated from the only channels of political influence *they* believed effective. The "alienated voter" phenomenon is not a slogan; there is . . . a real and powerful sense of disconnection. There is now a large debate going on in political science circles over whether in functional fact the little-voter is so cut off from power. From the human point of view, this debate is academic—the people themselves feel cut off, perceive themselves as robbed of something they cannot put a name to.⁶⁰

While the machine in its traditional form is, no doubt, gone for good, it might be possible to restore some of this "connectedness" that it once supplied. Neighborhood control of schools (not a *primary* source of concern from our data, but an important one) has been attempted in various places with mixed results; one obvious consequence has been to heighten the level of conflict in the community where it is tried. But a high level of conflict is one indicator of active voice, and is not—in itself—necessarily undesirable.⁶¹

In order to increase voice in these white urban areas, it is necessary—from our model—to give people grounds for believing that their efforts have a *reasonable* chance of influencing for the better the things they are concerned about. One of the most urgent tasks for Political Science is creative theory building about institutional changes that might justify these beliefs.

⁶⁰ Sennett, *The Uses of Disorder*, pp. 79-80. A similar conclusion is supported by quantitative analysis of beliefs about the distribution of power in the United States: the poor and Negroes give most credence to elitist and economic models of power, while the pluralist interpretation is supported most by high-status people. See William H. Form and Joan Rytina, "Ideological Beliefs on the Distribution of Power in the United States," *American Sociological Review*, 34 (February, 1969), 19-31.

⁶¹ To the contrary, Sennett has argued that individuals whose environment exposes them to conflict and obliges them to deal with it in a realistic way are unlikely to resort to violence in time of major crisis.

Some Conceptual Problems in Nuclear Proliferation

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The Proliferation Problem

A world in which many more countries acquire nuclear weapons could be a much more grim one. Wars that today kill thousands might instead kill millions; wars that now are averted might instead be launched, simply because each side preemptively felt it had such powerful weapons that it dared not hesitate to use them.

To prevent a further spread of nuclear weapons, the United States and the Soviet Union have offered for signature a Nuclear Non-Proliferation Treaty (NPT) freezing the number of states possessing such weapons at the current five—the U.S., USSR, Great Britain, France, and Communist China.¹ Countries possessing nuclear weapons pledge themselves under the treaty not to give them away or to assist other nations to produce them. Countries not yet having such explosives pledge themselves in the same treaty never to accept them and never to manufacture them. The latter “nonweapons” states moreover would agree to accept inspection safeguards by the International Atomic Energy Agency (IAEA) on all their peaceful nuclear activities, to ensure that such facilities and materials are not diverted to producing nuclear explosives.

The United States and Soviet Union came to agreement on an NPT text between the fall of 1967 and spring of 1968, and the treaty was offered for signature on July 1, 1968; ratifications by 43 states, including the U.S., USSR, and Great Britain, were required for the treaty to go into effect. Acceptance of the NPT was delayed somewhat in the aftermath of the Russian intervention in Czechoslovakia, but on March 5, 1970, Soviet and American ratifications were deposited together with those of enough other nations to activate the treaty. Among nonweapons nations with significant nuclear industries, only Canada and Sweden thus far have signed and ratified the NPT; West Germany, Italy, India, Japan, Australia, Israel, Brazil, and Argentina are some of the other states potentially capable of producing nuclear weapons.²

At first glance, we might interpret any foreign resistance to the NPT as indicating a malicious craving for nuclear weapons. More honorable and acceptable arguments can be advanced for opposing the treaty, however—arguments which will induce real political debate in the countries involved. Even if a nation has no intention of ever producing nuclear weapons, it might relish the legal *option* of producing them, for the political concessions this can produce, or simply for the prestige of being on an equal legal footing with the “superpowers.” Surrendering the weapons option can also have economic costs. Scientists have argued that valuable spinoffs in the peaceful application of atomic energy may be lost if weapons research is rigorously and explicitly forbidden, since the technologies often overlap. More important, there are definite costs in the verification process which the IAEA would install to assure outside powers that materials were not being diverted to weapons. Nations being inspected will have to detail trained and expensive personnel to accompany IAEA inspectors on their rounds. In some cases existing equipment might have to be redesigned to make adequate monitoring possible. At other times more detailed and costly bookkeeping procedures will be required. At the extreme, it might even be necessary to shut down operations in some facility to confirm records on the fissionable materials it contains.³

States allowed to retain nuclear weapons under NPT may openly or secretly welcome the binds it imposes on the countries which never get into the “nuclear club.” States denied such weapons will resent such binds on themselves, but may welcome the bind on other “nonweapons” states. The debate on the NPT, or on alternative barriers to the spread of nuclear weapons, will thus take varying forms from country to country. National values will be crucial to the trends of such debates, as will the particular political and economic situations of the countries involved. Technical opinions will

¹ The text of the Nuclear Non-Proliferation Treaty as finally presented can be found in United States Arms Control and Disarmament Agency, *Documents on Disarmament: 1968* (Washington: U.S. Government Printing Office, 1969), pp. 461–465.

² A full discussion of the spreading potential for nuclear bomb production, and efforts to control the

spread, can be found in C. F. Barnaby, ed., *Preventing the Spread of Nuclear Weapons* (London: Souvenir Press, 1969).

³ A basic analysis of the costs and problems of inspection appears in Arnold Kramish, “The Watched and the Unwatched,” *Adelphi Papers* (Institute for Strategic Studies, London) 36 (June, 1967).

also be crucial, as each society consults its physicists on the priorities and opportunities involved, asking such questions as: "If we retained our legal option to make bombs, how long would it take us to acquire them?" "How far are our neighbors from the bomb?"

Yet authoritative answers here can easily be misunderstood within a conceptual confusion on "how close" nation X or nation Y is to making an atomic bomb. Depending on context and political biases, clearly different answers will be supplied in response to seemingly identical questions. The intent of this article is thus to explore the semantic ambiguity which appears in physicists' answers to politicians' questions on proliferation.

If we ask how far Japan is from the bomb, we might get an estimate of "three years." This answer does not tell us that Japan will have a bomb in three years; yet neither does it assure us that three years from now Japan would still have to wait three years for a bomb. Perhaps three years is the minimum required if a deliberate effort is expended; yet if no explicitly military effort is expended, will the time-lag remain the same?

What will concern most of us is not the earliest date at which six or eight countries can have bombs; there may be no evidence of crash programs in any of these states. Rather we must fear what the time-lag for any hypothetical deliberate crash program will asymptotically sink to, simply as the result of the more innocent civilian-purpose steps already being taken in each country. The following diagram is intended to clarify some of the distinctions involved here:

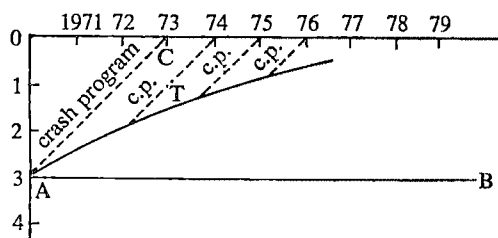


Figure 1. Innocent progress toward the bomb.

The vertical axis indicates the number of years it will take some nation to produce nuclear weapons of Nagasaki magnitude if it begins at any particular date. The horizontal axis specifies, on an identical scale, years of the future for which the above scale applies. AT illustrates what may be a typical curve. By the very definition of the axes, no curve can approach the zero time-lag line more rapidly than at a 45

degree angle (AC). If Sweden were defined as being four years from the bomb and started her crash program now, she in four years would be zero years from the bomb. Any 45 degree line, any line parallel to AC, is a crash program line. (For the moment, we are only discussing time-lags to production of *bombs*; if delivery systems are judged important, these can be added to the analysis as suggested later in this article.)

The concept of a "crash program" may be a little fuzzy. Political decision-making bodies will typically see certain "satisficing" points⁴ at which they are doing "all that can be done" to secure some weapon. From our disinterested perspective, we might see this as a rationalization; perhaps one can *always* achieve still more rapid results if one only will spend still more money; perhaps not. The lengths of time assumed for "crash programs" here thus reflect the probable willingness of states to devote resources to the race, once an explicit move toward nuclear weapons has been approved.

Why are the questions posed on such a graph relevant? Why indeed should the outside world care *how far* any particular state is from acquiring nuclear weapons? We of course prefer that this state not have such weapons, so we hope that it will be *some* distance from them. If this distance is a little greater, moreover, we can expect to detect any deliberate move to acquire bombs before it is successful, while we still will have time to initiate political, economic, or even military retaliatory moves. We hope that anticipation of such detection and retaliation will deter the move for the bomb in the first place. The longer the time-lag until a bomb could be produced, the more time for the outside world to notice and react to such a move, and the greater the deterrent to it. In the worst or tensest situation, the sanction for any perceptible grasping for bombs might be war and nuclear attack. Sanctions far short of this might suffice, however, as with general political condemnation by the outside world, the freezing of assets and remittances, or the imposition of some sort of embargo.

The Shrinking Time-Lag

One might now venture to contemplate the characteristics of the normal curve (AT). Why does it not stay horizontal and parallel to the zero line (AB); why does the time-lag not stay constant for a nation not actively seeking to acquire nuclear weapons? A number of factors

⁴ The "satisficing" concept is of course drawn from Herbert Simon, "A Behavioral Model of Rational Choice," *Quarterly Journal of Economics* 69 (February, 1955), 99-118.

can be predicted here to bring even the most innocent nations ever closer to the bomb.

Economic needs for electrical power may encourage or require the building of power reactors. Some power reactors are fueled with natural uranium, which is freely available in a number of countries, precluding any external inhibitions to supply. Others are fueled with enriched uranium, which, although it must typically be purchased from a source like the United States, may lend itself more easily than natural uranium to further enrichment to weapons-grade U-235. Either of such reactors produces plutonium as well as electrical power. Plutonium, when the economics of power production justify it, can be processed again in a chemical separation plant for use as fuel for various kinds of reactors. Over time larger quantities of plutonium or enriched uranium will thus come into circulation in various countries. National plants for the production of plutonium or enriched uranium will also become commercially advisable. Plutonium and enriched uranium both can be adapted for use in a bomb.⁵

Quite independently of this production of fissionable materials, basic research may be undertaken on how such material would be assembled into bombs. This may not be inspired by any direct desire to manufacture weapons, but merely by an interest in "pure research"; or it may follow incidentally from research in related practical civilian fields such as controlled fusion or breeder reactors. The need for possible civil defense measures against other nation's bombs may similarly stimulate such research; so will simple patriotic concern that other nations are undertaking similarly basic enquiries. (Thus, for instance, in Sweden basic research has been done on bomb designs although atomic bombs have never been assembled.) If the laboratories required for these investigations are small enough, their activities may be impossible for outside agencies to monitor, and to verify their findings may require nothing so momentous as an actual test detonation.

In general the discoveries of other nations in the nuclear field will steadily leak out, often in straightforward academic publications. Peripheral equipment similarly will become available in world markets at lower costs, perhaps even in cases in which the entire kit would pre-

viously have had to be constructed indigenously. Finally, the infrastructure of trained personnel and functional organization will tend to grow in most nations so that any program as sizable as weapons development will be more feasible.

At first, the AT curve may be almost parallel to an AC military crash-program curve, since all the bomb-relevant basic research that could be undertaken will emerge almost naturally in any event. Abstractly, the United States in 1900 was doing all it could do (was on a "crash program") to produce nuclear weapons. So is Upper Volta today. At such an early stage, almost no additional money could be profitably spent to accelerate a bomb capability. Governments and legislatures thus are able to speak of "no need yet to make a decision" on nuclear weapons. All that can be done is being done. This may merely evade a decision for the early years of a nation's nuclear development, but a need for choice definitely emerges later, when the nation must deliberately decide whether an optimal military program will be undertaken or avoided. Yet if such a program is not undertaken, the remaining effort required for bomb-production may still always be decreasing.

One can argue that this menacing overlap between civilian and military nuclear technology is nothing new, for responsible officials have been alert to it for years. No one expects the IAEA inspectorate, which will have primary responsibility for inspecting peaceful nuclear installations under NPT, to spend most of its time watching for autonomous and explicitly military programs; the focus of attention will instead be on the civilian activities which in their natural growth more ever nearer to the military line. Any reader of the evolving IAEA safeguards documents since 1961 will be impressed by their ingenuity in closing all major gaps through which materials might slip unnoticed into bomb manufacture.⁶

IAEA, based in Vienna, came into existence in 1959 and since then has engaged in a mixture of inspection and international technological assistance activities. Inspection has involved materials and equipment which might otherwise be utilized for the production of nuclear explosives; IAEA practices have required ac-

⁵ Good basic accounts of the overlap of military and peaceful nuclear technology can be found in Leonard Beaton, *Must the Bomb Spread?* (London: Penguin Books, 1966), and Alastair Buchan, *A World of Nuclear Powers* (Englewood Cliffs, N.J.: Prentice-Hall, 1966).

⁶ Comprehensive accounts of the evolution of IAEA practice can be found in Arnold Kramish, *The Peaceful Atom and Foreign Policy* (New York: Harper and Row, 1963), and Lawrence Scheinman, "Nuclear Safeguards, the Peaceful Atom, and the IAEA," *International Conciliation*, 572 (March, 1969).

cess to records on the utilization of such materials, as well as verification of records by inspection of the fuel or equipment itself. IAEA inspection experience has been accumulated under three circumstances to date: when the Agency has itself arranged for the transfer of equipment or fuel, when a technical-assistance agreement between two states has designated IAEA as the inspection agent in place of the donor state, and when a state has unilaterally asked the Agency to apply safeguards over some or all of its facilities.

If bomb capabilities are coming a little closer now, the addition of an international inspection or safeguards system can still serve to alert the outside world after a deliberate bomb program has been launched and before it reaches fruition. After completion, it may be more difficult for the outside world to impose any retaliatory sanctions. Indeed, after the bomb program has reached fruition, the nation involved may want to publicize rather than to hide it.

Yet this IAEA vigilance may not relieve our concern but only prove how well taken it was. In effect the ingenuity of IAEA safeguard documents illustrates the running of a possibly hopeless race. The inspectorate must watch what is legitimate, to be certain that nothing illegitimate (i.e., lacking civilian purpose, moving toward explosives) is undertaken. As more and more activities become legitimate—not only reactors, but reprocessing plants, separation plants, and perhaps soon uranium diffusion plants—the inspectors must follow along, watching always a new zone of legitimacy, to make certain that the next “illegitimate” zone is not entered.

As projects justifiable in civilian terms bring nations ever closer to nuclear weapons, the time-lag will continually be shrinking. And even with the presence of an IAEA inspectorate, the outside world may find it impossible to detect a bomb program early enough for meaningful reprisals. Perhaps the IAEA could not even disclose a bomb program as early as the country itself wanted it disclosed; past a certain point, a nation will want to advertise and proclaim the fact that it has decided to violate its NPT commitments.

Questions also can always arise about whether we really know with certainty how far Israel or Canada or Sweden is from a bomb. Without inspection, the extent of militarily useful nuclear progress in a country may be difficult to determine; even with such inspection, a certain percentage of the fissionable material in

an advanced commercial system might be undetectably divertable to military uses.

Zero Time-Lags

One might then question whether the AT curve will inevitably reach the zero time-lag line, or whether it will level off at some relatively even distance from it. Perhaps by the very definition of the curve, some gap would always have to remain, for the AT curve describes progress where there is no intention to manufacture nuclear explosives. One can theoretically conjure up a reactor design that would be exactly identical to a bomb, but this happily is unlikely, so that an absence of intent to assemble bombs would presumably keep them from coming into existence. There will thus always be some final work to be done, some final questions to be answered, before bombs would be available. Perhaps three months is a reasonable estimate of the residual time-lag at which we would asymptotically level off; this may be too pessimistic or too optimistic.

A nation may thus wait “just short” of nuclear weapons, as the result of its full development of peaceful nuclear industry. Yet much will depend on how it waits. Some states might elect to allow plutonium to accumulate first, before designing and building any models or prototypes of bombs; this would allow honest disclaimers that no “work on bombs” was under way. Others might move right ahead with prototypes, so as to speed the final production of bombs whenever the international situation seemed to require it. Perhaps India is in the first category today, and Israel may be in the second. Once work on bomb design has been done, however, it cannot easily be undone, as scientists do not quickly forget what they have learned. If a computer simulation has once been run on the design of a bomb, the country which has commissioned it has been moved that much closer to possession of bombs; for an indefinite time into the future, the time-lag until actual weapons production may have been reduced from months to a matter of weeks.

With the necessary margin of explicitly military activity so slight, the temptation to begin and complete a crash program might become very great, especially for nations which have not signed a treaty renouncing nuclear weapons. The mere desire for scientific tidiness may stimulate the manufacture of the bomb, to prove out all the speculation as to whether it could now be easily done. Fears of some preemptive intervention by a hostile state (perhaps

one also immeasurably close to bombs) might similarly stampede a nation into crossing the line (see ATN, Figure II); "ten bombs in a cave" may be a better deterrent to war than "ten bombs anytime, at twelve weeks' notice."

If such reasoning on preemptive war is at all correct,⁷ one could even question whether peace is best served by trying to hold back nations at such a short distance from bomb stockpiles. Nations with no imminent conflicts with their neighbors may indeed be better off never having touched the zero-line. Those under the shadow of attack might behave less nervously, however, if the line had already been crossed. If assured second-strike forces are preferable to preemptible forces among the great powers, they might also be preferable for some small-scale confrontations.

Legal Obligations

One should thus consider the impact of the obligations imposed by the Nuclear Non-Proliferation Treaty, as well as by other international agreements. NPT commits a nation not to make nuclear weapons, and to accept international safeguards to ensure that it is complying. A nation may withdraw from NPT upon giving three months' notice.

Perhaps more binding still are the various bilateral agreements whereby nations accept equipment or nuclear fuels from abroad, under the agreement that such resources will not be used for military purposes, and again that their use will be subject to international safeguard. Even after three months' notice, such resources cannot legally be used for military purposes, but must either be returned or kept safeguarded for civilian use. Development of domestic fuels or reactor designs may have been postponed because such international offers were commercially more attractive. It can thus be argued that such bilateral arrangements, as with the American AEC, are more effective in delaying nuclear weapons than is NPT.⁸

Yet the combination of NPT plus nuclear assistance may be the most effective restraint of all. Nations having formally renounced nuclear weapons will be more willing to let their domestic independent nuclear options atrophy, to

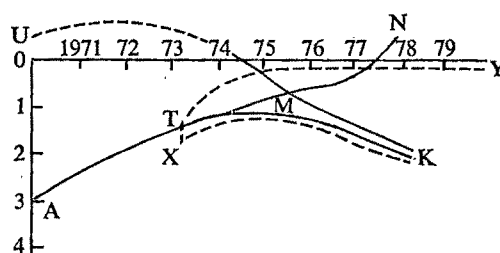


Figure 2. The impact of legal restraints.

be replaced by external assistance. Nations which have been seduced into becoming dependent on outside assistance will be more willing to go ahead to sign NPT. The combination of the two restraining influences can be greater than the sum of their independent influences.

The presence of a treaty obligation thus forces us to discuss "legal" and "illegal" crash programs, distinctly defined by varying degrees of treaty violation. How long will it take an NPT-signatory nation in 1975 to manufacture bombs, if it seizes safeguarded stocks of fissionable materials (i.e., arresting and deporting, or simply ignoring, the IAEA inspectorate)? How long, if it instead openly and honestly revokes the treaty, giving the proper notices, surrendering or returning all safeguarded equipment and materials, beginning afresh to produce and accumulate plutonium free of any international legal commitment? Our "illegal" curve (ATY) will show the physical capability of an NPT-signatory for cases in which it is prepared to ignore its treaty obligations completely. The "legal" (ATXK) curve conversely shows time-lags to the bomb for cases in which such treaty obligations are to be respected, that is, if no price in "international reputation for law-abiding behavior" is to be paid.

The distinction between "legal" and "illegal" crash programs is important, at least because it is another source of the semantic confusions under examination here. When asked how long it will take his country to make a bomb in 1975, some scientists will answer with reference to strict adherence to treaty obligations, others with total indifference to such obligations.

The ATN curve shows how capabilities might have developed if there had been no NPT. When compared to this "no-treaty" curve, signing NPT may thus have brought a nation closer to bombs for an interim period, at least via potential "illegal" bomb programs. Signing up probably has given a nation access to greater outside assistance in nuclear technology, and induced a greater foreign tolerance of

⁷ The reasoning on preemptive and surprise attack is spelled out quite fully in Thomas Schelling, *Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960), pp. 205-254.

⁸ See Jan Prawitz, "A Nuclear Doctrine for Sweden," *Cooperation and Conflict*, 1968:3, pp. 184-193, for an account of the diverting of Swedish nuclear activities from military to purely civilian purposes, in part through the acceptance of foreign (American) assistance.

big reactor projects or of plutonium-enrichment plants. Looking at the "legal" curve (ATXK) however, one sees that the bomb program time-lag has been increased. First, there is a flat addition of three months notification, at the date of signature. Second, the signatory state thereafter may be more likely to accept fuels and facilities under IAEA safeguards from other countries, rather than maintaining indigenously produced nuclear fuels and facilities, probably because these will be much more efficient and economical; hence the establishments legally free of bans on military use will actually begin to atrophy. The time-lag for "legal programs" will thus gradually expand again to level off at some number of years instead of months.

One must note that this latter "legal" ATXK curve will at best be a benchmark. It indicates what a state can do with roughly as much legal dishonor as would have been incurred in a bomb program before the treaty had been adhered to. In actuality, the benchmark may become unrepresentative in one direction or another. International morality might come to disapprove strongly of any state's exercising even its legal prerogative of withdrawing from the treaty; alternatively, international morality might come to tolerate states violating the treaty, when they were severely threatened, even without ever formally withdrawing from it.

Increasing Time-Lags

It will be interesting to examine the ways in which the curve could move away from zero, as the time-lag grew. Theoretically it is possible for a nation to lose its proficiency at physics, if scientists were to be driven out of the country or if research programs were allowed to wither for lack of funds (ATMK). Something like this happened in Germany between 1930 and 1935; if the Weimar Republic had survived, it could have had the atomic bomb earlier than Nazi Germany; as it happened, the German "crash program" curve moved away from zero during the years in which Jewish and other physicists were driven to leave the country.⁹

For another example, a nation that already has a nuclear stockpile and destroys it will have increased its "time lag" (UK). If all facilities capable of producing fissionable material were to be dismantled, this would widen such a gap even more.

Other possibilities exist, for there are some

irreversible processes in the exploitation of economic opportunity. A nation might at some point have to choose between investing in a large power reactor project, or in a similarly extensive hydroelectric facility. Either of these would supply electricity, and would lower the market price of such electricity enough to make the other redundant. A military crash program that also produces needed electricity can be assigned a lower explicitly "military" price tag, and can be funded more rapidly and generously. Suppose that the hydroelectric project were built. If the electrical needs of a country thus had been satisfied by alternative means, the probable speed of any crash program would be reduced and the time-lag to weapons increased. All such pricing and costing thus depends on the environmental assumptions. Many nuclear electrical projects can conversely be rated as profitable if, and only if, the plutonium produced is explicitly assigned the higher market value of military purposes.

Even within the nuclear field, newer and more profitable technology may over time require less handling of fissionable materials, although the current trend is clearly in the opposite direction. As absolute weapons-capable facilities are thus closed down, to be replaced by newer non-weapons-capable plants, the time-lag to weapons would naturally grow again in countries not consciously pursuing weapons.

One might still vaguely cherish the hope that weapons-grade uranium or plutonium will never be required for any commercial purpose, that the fuels for power reactors can always be less pure. If this were so, the innocent processes of commercial development could indeed leave states still far from bomb capabilities. And if that were so, the Nuclear Non-Proliferation Treaty could indeed have been written still more tightly, to ban explodables as well as explosives, to preclude the possession of fissionable materials by any state outside the current five weapons states. Yet the exact definition of what is fissionable would require disclosure of relatively classified military information, and the facts here may never be settled for all time. For the U. S. or USSR to inform the world of the exact purity of uranium or plutonium required for a bomb might only accelerate the proliferation process. Research reactors may inevitably require weapons-grade materials in any event and so may certain power reactors some day.

A nation may similarly be induced by commercial consideration to depend on a foreign supply of enriched uranium or on foreign reprocessing of plutonium, thus again slowing

⁹ See Robert Jungk, *Brighter than a Thousand Suns* (New York: Harcourt, Brace and World, 1958) for a fascinating account of the distribution of nuclear expertise in the 1920s and 1930s.

and making commercially redundant any indigenous national program legally free to produce weapons. But here we are returning to the distinction between legal and illegal crash programs introduced above. In many circumstances, such a legal barrier would slow moves toward bombs just as much as physical obstacles would. In other cases, however, legal scruple will count for less. Even so "law-abiding" a country as Sweden might consider illegally seizing safeguarded plutonium or enriched uranium to make some bombs if a foreign army seemed about to cross its borders and advance on Stockholm. For the most dire circumstances, every advanced nation might thus become a short-time-lag nuclear weapons state, unless the physical technology of nuclear energy were to move countries away from this capability.

It was earlier argued that the international inspector's explicit function will soon be made obsolete by the shrinking of the "time-lag to bombs." Perhaps the Non-Proliferation Treaty could thus have been better written without any provision for inspection, and so could the bilateral agreements by which nuclear-weapons states give technological and material assistance to nonweapons states. Inspection promises to pose many problems, in terms of the sheer cost of accommodating such international visitors, and the threats of commercial espionage or simple misunderstanding. Yet it would be an overstatement to say that international inspectors would serve no purpose any more once the crucial time-lag had fallen below six months. By their very presence, they may lengthen the physical time-lag.¹⁰

To be in with a relatively trivial point, no one can be certain that a bomb program would not still be detected somehow by the inspectors wandering around the premises. Since estimates of secrecy will have to be probabilistic, the addition of a safeguards system may yet deter a bomb program, out of fear that the outside world would be roused before the program could be completed. Even if they could no longer detect a bomb program any earlier than the cheating government wanted them to, the inspectors in position will be a continual and visible reminder of the state's treaty obligations, a reminder that some international retaliation will come after bombs are acquired, no matter how late it comes. Violating NPT will be a delicate decision for any state; if forgetting about

international feelings were to make it easier for some state to decide to go ahead, the presence of inspectors will make these harder to forget.

The presence of inspectors generally upsets the legitimacy of purely national decisions on nuclear weaponry, in effect undermining the sovereignty of the nation in the nuclear field. It will be less easy for the crucial physicists to see a clear pattern of duty for themselves; some may become confused enough in their loyalties to betray a national bomb program to the IAEA representative passing through. Inspectors moreover make it more difficult for outside states to forget or back out of their threats of reprisal; the insult is compounded when one's nationals on duty as inspectors are ignored and defied.

Finally the IAEA safeguards procedures, however inadequate, force the government in question to institute national auditing and control systems which otherwise might not be established. With international inspection, any move to a bomb will at least require making an explicit national decision, rather than allowing the development to go forward by default. In states which otherwise might have let small cliques of scientists or military men launch military weapons programs, the inspectorate may thus forestall a bomb acquisition.

Exceeding the Crash Program

One must also reexamine the assumption that the curve cannot approach the zero-line at a more severe angle than 45 degrees (AZ). The time-lag at point A is defined only by conditions within the country in question. Naturally, every nation has a zero time-lag when another state already possessing nuclear weapons is prepared to give some away. The definition also excludes drastic redefinitions of the country in question; e.g., if the Common Market on short notice were to become sovereign, Luxembourg in that short time would have become (part of) a nuclear weapons state. The time-lag we will normally cite for Luxembourg assumes that there will be no merger with France and no gifts of nuclear warheads from the United States. But what if a group of French physicists tomorrow flee to exile in Luxembourg (perhaps bringing stolen fissionable material with them)? This may give the country a bomb by 1972, yet our curve for 1970, did not show a two-year time-lag. Indeed, to take a real-life example, could one have predicted in 1925 that the United States was only 20 years from the atomic bomb? With even J. Robert Oppenheimer studying in Germany, the balance of scientific talent might have suggested 1955 as

¹⁰ Some of the following argument was made earlier in a broader arms control context by Lawrence S. Finkelstein, "New Trends in International Affairs," *World Politics*, 18 (October, 1965), 117-126.

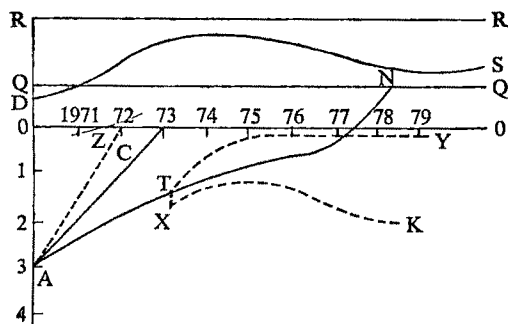


Figure 3. Redefinition of the baseline.

the bomb-acquisition date for the United States (and 1942 for Weimar Germany).¹¹

The vertical axis on our graphing thus shows only the probable duration of a crash program utilizing the resources and technical manpower likely to exist in the country at any given time. It represents a prediction of how long military crash programs would take, and of how other programs will function in lieu of crash programs, given the current scientific potential available to particular national governments.

Redefinition of the Baseline

To add a semi-trivial category of conclusions to those already spelled out, we may differ as to what our zero time-lag line should represent. When we ask, "How far is Australia from the bomb?" do we mean only one Nagasaki-type plutonium bomb (probably not), or twenty such bombs, or one hundred; or do we mean hydrogen-bombs (if we do not find ordinary A-bombs particularly significant); or do we mean bombs plus "first-rate" delivery-systems? Scientists frequently give reassuringly long time estimates for some country's bomb program, and then afterwards explain that these estimates were meant to refer to a "sizable" stockpile, or to include extensive development of military bomber or missile systems.

Such longer estimates often will not be so reassuring; "two plutonium-bombs, delivered by an Israeli airliner" may not be insignificant. The variations here might be shown additively by using alternative baselines drawn parallel to

each other (O-O, Q-Q, R-R) to distinguish various levels of accomplishment. By definition it takes longer to produce 50 bombs (R-R) than to produce 10 (Q-Q). Delivery systems, if they are relevant, may be more variable in their expansions of the time-lag. One could try to predict how available such systems will become "naturally" (i.e., with no explicit military intent) in the future. If a Boeing 707 were a ready-made bomber in 1961, perhaps SAM missiles reduce its effectiveness today. Conversely, if many nations decide to invest in space research, perhaps intermediate-range missiles will come to be close at hand for many of the countries which are about to acquire plutonium bombs. The zero baseline for "bombs-plus-delivery systems" might thus be a curved line (D-S), so that the time-lag for full military capability would be accurately shown.

Summary

The graphic formulation used here is intended to show the range of answers possible for the set of questions, "How far is India from the bomb?" The confusion and multiplicity of answers is of more than academic interest, for it shows that reassuring the outside world on one question in this range may not always be consistent with reassurance on another. Stemming the spread of nuclear weapons may not be hopeless, but it also may not ever be definitively accomplishable. There may be an important distinction between legal and illegal capabilities, once a treaty has been introduced, such that we will have to measure *political* capacities for violating treaties as carefully as *physical* capabilities. We will also have to consider the ranges of time-lags that bring a nation "too close" to a bomb, so close that opposing states would be tempted to try preemptive attacks, so that actual possession of bombs might have been preferable, so close that serious steps should be considered somehow to widen the time-lag.

Above all, the anticipation of nuclear proliferation may exhibit many of the same reiterative strategic calculation problems as does deterrence after the weapons are acquired. "How far is Japan from a bomb" may not be as important a question as "How far is Japan from being three months from a bomb"; the answer to the latter question may set some preemptive wheels whirring, in ways we wish to avoid.

¹¹ This is of course a somewhat speculative extrapolation from the evidence offered by Jungk, *Brighter than a Thousand Suns*.

On the Concept of Ideology in Political Science*

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"Ideology" has been, and remains, one of the most persistent and ubiquitous concepts in modern political thought. No doubt this popularity is due to the absence in our political lexicon of alternative terms that link political thought and behavior in a satisfactory way. Yet, as one writer on the concept of ideology observes, "the growing popularity of the term has been matched, if anything, by its growing obscurity."¹ This ambiguity in the concept is reflected from author to author in a wide variety of definitions, explicit or implicit, and a lack of agreement regarding even the basic properties of ideology.²

If the concept of ideology is worth saving for political analysis—and the attempt seems warranted by both the concept's widespread use and the lack of an acceptable substitute—then the conceptual and terminological confusion surrounding it must be dispelled. The problem, I maintain, is not merely a formal one of defining ideology and using the definition uniformly, as if we had "the right to retreat into our own worlds of meaning, and demand only that each of us remain consistent within his own private terminology."³ It seems necessary rather to inquire into the basis of the concept's strength and meaning. What are the theoretical and empirical grounds for using ideology in one way rather than another? How does this usage allow one to discriminate among phenomena and to call attention to salient features of what is be-

ing explained? Can the concept be formulated and defined in such a way that it is relevant to empirical inquiry? Only in light of the answers to such fundamental questions will a definition of ideology be comprehensible or persuasive. The aim of this paper, therefore, is to explore the theoretical bases for conceptualizing ideology in a particular way, and, in terms of this conceptualization, to offer a definition which, I believe, is relevant empirically. The discussion will proceed in three parts. Section one will examine the basic problem in the conceptualization of ideology, that of establishing boundaries of ideology vis-à-vis other cultural phenomena. This will lead, in the second section, to a presentation of criteria by which ideology may be distinguished from related cultural forms—especially myth and utopia—with which ideology is often confused. Section three will be devoted to an elaboration of the concept of ideology through an analysis of components suggested in section two, and will conclude with a definition of ideology based on these components.

Ideology and the Problem of Boundaries

The main impediment to developing an adequate conceptualization of ideology, I believe, has been the general failure of those using the concept to recognize the necessity for distinguishing ideology from the great backdrop of other cultural phenomena. In many conceptualizations, ideology is so all-inclusive that other symbolic forms are encompassed by it; because ideology is too comprehensive, it lacks "discriminating power"; and, lacking the power to discriminate among various, often similar, phenomena, the concept fails to achieve empirical relevance.⁴ In other conceptualizations, this same problem of boundaries arises because ideology is subsumed under another and more comprehensive, symbolic form such as myth. In neither of these situations does the concept of ideology have much value for political analysis,

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¹ Giovanni Sartori, "Politics, Ideology, and Belief Systems," *American Political Science Review*, 63 (June, 1969), 398.

² The best review of the many meanings of ideology is provided by Arne Naess and Associates, *Democracy, Ideology and Objectivity: Studies in the Semantics and Cognitive Analysis of Ideological Controversy* (Oxford; Basil Blackwell, 1956), especially, pp. 141-176. An appreciation for the range of meanings, however, can be gained from the much shorter list collected by Robert E. Lane, "The Meanings of Ideology," in *Power, Participation and Ideology*, ed. Calvin J. Larson and Philo C. Wasburn (New York: David McKay, 1969), pp. 321-323.

³ Hannah Arendt, *Between Past and Future: Six Exercises in Political Thought* (Cleveland: Meridian, 1963), p. 96.

⁴ This difficulty is recognized clearly by Giovanni Sartori (pp. 398-400). Although I disagree with Sartori's concept of ideology, his work is important because it raises, in a self-conscious and systematic way, the problem of distinguishing ideology from other mental and cultural phenomena.

I have analyzed Sartori's approach in an article titled, "Sartori's Concept of Ideology: A Dissent and An Alternative," in *Public Opinion and Political Attitudes: A Reader*, ed. Allen R. Wilcox (New York: Wiley, forthcoming).

because its limits and differentia remain obscure. A brief exposition of some representative examples from the literature on ideology will illustrate the nature of the problem of boundaries and suggest how one might solve the problem satisfactorily.

Karl Marx has had perhaps the greatest continuing influence on the concept of ideology. Nevertheless, Marx was not the first to study ideology, nor did he invent the term. Francis Bacon may be said to have originated the study of ideology in his investigation of the preconceptions and prejudices, or "idols," which impede the exercise of scientific method.⁵ As a distinctive term, however, ideology is the product of certain French post-Enlightenment theorists called the *Idéologues* who developed a theory of ideas based on sensationist philosophical principles similar to those of Locke.⁶ The *Idéol-*

⁵ An excellent account of Bacon's contribution to the theory of ideology is provided by Hans Barth, *Wahrheit und Ideologie*, 2nd ed., enlarged (Zurich: Eugen Rentsch Verlag, 1961), pp. 32-60.

⁶ The most prominent of the *Idéologues* is generally considered to be Antoine Destutt de Tracy whose basic work on ideology was his *Elements d'Ideologie* (1801-1815). An excerpt from the *Elements d'Ideologie* in English translation, together with some comments on the work of the *Idéologues*, is contained in Richard H. Cox, ed., "The Original Concept of Ideology," in *Ideology, Politics, and Political Theory* (Belmont, California: Wadsworth, 1969), pp. 10-27. On the *Idéologues* see also H. B. Acton, "The Philosophy of Language in Revolutionary France," in *Studies in Philosophy*, ed. J. N. Findlay (London: Oxford University Press, 1966), pp. 143-167; Barth, pp. 13-31; George Lichtheim, "The Concept of Ideology," *History and Theory*, 4 (1965), 164-170; or, Jay W. Stein, "Beginnings of Ideology," *South Atlantic Quarterly*, 55 (April, 1956), 163-170.

After the Revolution of 1789, when France was still a democratic republic, the *Idéologues*, most of them members of the National Institute, sought to develop doctrines that would be a solvent for the *a priori* philosophical and theological speculations which had bolstered the old regime. The method the *Idéologues* developed to meet this double exigency was the "science of ideas" which they called "ideology," and through it, they aspired to provide a firm ground for reason by "reducing" all ideas to their genesis in sensation. Using this technique, the transcendent notions of religion and philosophy were subjected to criticism with the aim of hindering the perpetuation of false abstract principles which, they believed, impeded "not only the understanding of men amongst themselves, but also the building of the state and society." (Barth, pp. 16-17. Translation by Ilse M. Mullins.)

Their iconoclastic views naturally brought the *Idéologues* into conflict with Napoleon Bonaparte after his rise to the position of Emperor. He set out to discredit the science of ideas, which, it seemed, undermined political authority and promoted the dissolution of the body politic. Through his powers as head of state, Napoleon reorganized the Institute, abolishing the Second Class of Moral and Political Sciences, until then a sanctuary for the *Idéologues*, and he

ogues regarded ideology quite simply as a "science of ideas." Ideology did not refer to a quality or type of thought, but to a technique for discovering truth and dissolving illusions. Marx, however, changed the meaning of ideology significantly, and, in doing so, cast it in its modern form.⁷ Instead of being a method for discovering truth, in Marx's theory ideology designates a quality of thought—especially social thought—that is illusory or distorted. By using the concept of ideology in this way Marx raises for the first time the problem of limits. If ideology refers to a type or quality of thought, what are the boundaries of ideology and what are the relations of ideology to associated phenomena? Unfortunately, Marx's theory of ideology does not meet this problem satisfactorily, for he uses the term so comprehensively that it seems to include almost all cultural phenomena. As he views ideology, it encompasses all the mental misconceptions that constitute the failure of individuals to understand their alienated relationship to their surroundings and the significance of that relationship within the whole development of history.⁸ Moreover, it is

turned the terms "ideology" and "ideologue" into pejoratives, indicating that the *Idéologues* were visionary dreamers who had no understanding of practical political affairs.

⁷ Of course, Marx's concept of ideology was influenced by antecedent developments in German philosophy, especially as found in the works of Hegel and Feuerbach. See, for example: Barth, pp. 61-96; A. James Gregor, *A Survey of Marxism: Problems in Philosophy and the Theory of History* (New York: Random House, 1965), pp. 3-37; Lichtheim, pp. 174-177; or John Plamenatz, *Ideology* (New York: Praeger, 1970), pp. 32-45.

⁸ In relating Marx's concept of ideology, it is tempting to stop with his portrayal of ideology as a separation or "inversion" of thought and being, where ideas become reified and lose their relevance to one's existential sociological condition. The cure for these mental chimeras, therefore, would be "positive science." See Marx and Engels, *The German Ideology*, ed. R. Pascal (New York: International, 1947), pp. 13-15.

This, however, would be an incomplete account of ideology as Marx conceived it. That is because, for Marx, a true understanding of one's existential situation in a particular epoch necessitates not only "positive science," but requires, in addition, the capacity to understand the significance of that situation as measured from the perspective of "history as a whole." In the words of George Lichtheim: "To Marx, as to any Hegelian, the actual world of empirical perception was only an imperfect realization—at times indeed a caricature—of the real or rational world, in which man's essential nature (his rationality) will have overcome the reified existence he leads while the surrounding object-world is not perceived as the product of his own creativity. The attainment of this liberated state is the work of history, whose dialectic is not disclosed by empirical perception, but by critical (philosophical) reflection on the totality of the process" (p. 194).

not clear in Marx's theory, to what degree these misconceptions are equal to consciousness itself.⁹ Certainly, Marx maintains that in the stage of industrial capitalism and bourgeois society, the entire "cultural superstructure" is ideological, and within the superstructure he includes "the legal, political, religious, aesthetic [and] philosophic"¹⁰ forms of ideology. In Marx's view, only Marxist social theory, based on a true understanding of history, appears free from ideological distortion.

The problem of boundaries in the conceptualization of ideology also arises in the work of Karl Mannheim, where the dominant meaning of ideology is "the whole outlook of a social group,"¹¹ its "total *Weltanschauung*,"¹² as conditioned sociologically by the group's political orientation, and temporally by its location in the ongoing historical process. Moreover, Mannheim universalizes ideology, insisting that even Marxism is an ideology.¹³ To the extent that Mannheim does attempt to distinguish between ideology and utopia, the criteria of differentiation are of little value analytically. Ideologies, he says, are of two types. Those incorporating norms from the past and tending to preserve the status quo are called "ideologies." On the other hand, thought is called "utopian" if based on transcendent elements "which, when they pass over into conduct, tend to shatter, either partially or wholly, the order of things prevailing at the time."¹⁴ A crucial problem with the distinction is that one cannot tell when thought is utopian except in the successful case where norms serve to shatter the pres-

ent order, and only after they do.¹⁵ Analysis must always be *post facto*.¹⁶

The inclination to use ideology as a comprehensive concept incorporating most of what we ordinarily view as the theoretical or systematic side of culture, and the corollary failure to distinguish ideology from other cultural forms, may also be seen in the works of more recent writers. In a book by L. H. Garstin, for example, the terms "ideology," "myth," "creed," "conceptual scheme," and "*Weltanschauung*" are used pretty much interchangeably.¹⁷ Garstin says that "all societies are characterized by *weltanschauungs* [sic] of one kind or another."¹⁸ As basic presuppositions, these *Weltanschauungen* or ideologies function to integrate beliefs and ideals and to give life meaning. In this sense ideologies exist equally in the primitive cultures of Malinowski's Trobriand Islanders or Ruth Benedict's Kwakiutls, in the premodern civilization of medieval Christianity, and in modern secular society.¹⁹ Another author, Harold Walsby, even more categorically states that ideology refers to the inchoate underlying presuppositions of a society's world view.²⁰ Ideology, he argues, is not limited to

basic ideas underlying some system of economic or political theories. [Ideology includes] the basic ideas (or rather, the assumptions) underlying any system of ideas. And since we shall find that all ideas (or more definitely, all propositions) are based on, or take for granted, certain cognitive assumptions, and that, moreover, on the basis of these assumptions, propositions can be classified or divided into different groups or systems, then it can be seen that our extended definition will cover the whole field of propositions—political, economic, religious, philosophical, scientific or otherwise.²¹

The literature of the so-called "end-of-ideology" school which flourished in the 1950s and early 1960s²² also disregards the problem of

⁹ Compare, for example, Marx and Engels, pp. 13-15, with Marx, "Excerpt from *A Contribution to the Critique of Political Economy*," in *Marx and Engels: Basic Writings in Politics and Philosophy*, ed. Lewis S. Feuer (Garden City, New York: Doubleday, 1959), p. 44.

¹⁰ Marx, "Excerpt from *A Contribution . . .*," p. 44. In Marx's view, myth and utopia, as types of distorted theoretical thinking, are also forms of ideology. On myth, see Marx, "Excerpts from *The Eighteenth Brumaire of Louis Bonaparte*," in *Marx and Engels: Basic Writings . . .*, ed. Feuer, especially pp. 320-323. See also Harold Rosenberg's excellent analysis of the *Brumaire* in his "The Resurrected Romans," *The Kenyon Review*, 10 (Autumn, 1948), 602-620. On utopia, see Marx and Engels, "*Manifesto of the Communist Party*" and Engels, "*Socialism: Utopian and Scientific*," in *Marx and Engels: Basic Writings . . .*, ed. Feuer, pp. 37-39, 68-111, respectively.

¹¹ *Ideology and Utopia*, trans. Louis Wirth and Edward Shils (New York: Harvest, 1936), p. 59.

¹² Mannheim, p. 57.

¹³ Mannheim, pp. 125, 277.

¹⁴ Mannheim, pp. 192-193.

¹⁵ Mannheim argues that "wish-images" are not utopian until they pass over into "actual conduct" (p. 193).

¹⁶ For a discussion of this difficulty in Mannheim's thought, see Helmut R. Wagner, "Mannheim's Historicism," *Social Research*, 19 (Spring, 1952), 320-321.

¹⁷ *Each Age is a Dream: A Study in Ideologies* (New York: Bouregy and Carl, 1954). Compare pp. 6, 7, 14, 27, 32, 34, 35, 37, 38.

¹⁸ Garstin, p. 27.

¹⁹ Garstin, pp. 10-47.

²⁰ *The Domain of Ideologies: A Study of the Origin, Development and Structure of Ideologies* (Glasgow: William MacLellan, 1947).

²¹ Walsby, p. 142 (author's emphasis).

²² See Raymond Aron, *The Opium of the Intellectuals* (New York: W. W. Norton, 1962); Daniel Bell, *The End of Ideology: On the Exhaustion of Political*

distinguishing between ideology and other cultural forms. After a perusal of their writings, one critic finds that the members of this movement tend to share a view of ideologies as "absolute utopias."²³ One may also perceive in this literature a propensity to use the term "ideology" coextensively with the phrase "secular religion."²⁴ In either case, the contours of ideology are nebulous and of little analytical merit.

Finally, it will be instructive to examine an approach which offers a relatively more sophisticated—but, it will be argued, an unsuccessful—attempt to distinguish among ideology, myth and utopia. In the schema of Harold D. Lasswell and Abraham Kaplan, "the *political myth* is the pattern of the basic political symbols current in a society."²⁵ Myths are described as "certain fundamental assumptions which at the time, whether they be actually true or false, are believed by the mass of the world to be true with such confidence that they hardly appear to bear the character of assumptions."²⁶ In Lasswell and Kaplan's view, myth comprehends ideology and utopia because "*ideology* is the political myth functioning to preserve the social structure; the *utopia*, to supplant it."²⁷

Ideas in the Fifties (New York: The Free Press, 1965); Albert Camus, *The Rebel: An Essay on Man in Revolt* (New York: Vintage, 1956); Seymour Martin Lipset, *Political Man: The Social Bases of Politics* (Garden City, New York: Doubleday, 1960); Edward Shils, "Ideology and Civility: On the Politics of the Intellectual," *Sewanee Review*, 66 (Summer, 1958), 450–480, and Shils, "Letter From Milan: The End of Ideology?" *Encounter*, No. 26 (1955), 52–58.

Much of the literature of this movement and its critics is found in Chaim I. Waxman, ed. *The End of Ideology Debate* (New York: Funk and Wagnalls, 1968).

²³ William Delany, "The Role of Ideology: A Summation," in *The End of Ideology Debate*, p. 295.

²⁴ See, for example, Aron, pp. 265–294; Bell, pp. 400–401; Camus, pp. 110–132; and Shils, "Ideology and Civility . . .," pp. 457–466.

²⁵ *Power and Society: A Framework for Political Inquiry* (New Haven: Yale University Press, 1950), p. 116 (author's emphasis). For an analysis of political symbols similar to that presented in *Power and Society*, see also, Lasswell, "The Language of Power," in *Language of Politics: Studies in Quantitative Semantics* by Lasswell, Nathan Leites and Associates (New York: George W. Stewart, 1949), pp. 3–19.

²⁶ Lasswell and Kaplan, p. 117, quoting A. V. Dicey. Lasswell and Kaplan note the similarity between their "myth" and, among others, "Marx's 'ideology,'" and "Mannheim's 'ideology' and 'utopia'" (p. 117).

²⁷ Lasswell and Kaplan, p. 123 (author's emphasis). In a footnote on this page, it is suggested that the term "utopia" is retained because of its "familiarity in technical contexts," but that the term "countermyth" might be "less misleading" (author's emphasis).

In this section Lasswell and Kaplan also conceptualize other aspects of the political myth, each of

The approach of Lasswell and Kaplan makes no attempt to analyze the *structure* or form of ideology or to note the ways in which it differs structurally from other symbolic forms. The various types of political myth, including ideology and utopia, are distinguished on a *functional* basis. Following Mannheim's example, therefore, Lasswell and Kaplan argue that:

[A] utopia as here defined need not be "utopian" in the sense of either impracticability or perfection. We call a pattern of political symbols a utopia if their function in the political process is to induce fundamental changes in power relationships or practices; an ideology, if they serve to maintain the given power patterns. . . .

The definitions are based on the way the symbols function, not on characteristics of the symbols themselves. Symbols functioning at one time as utopias may at another serve as ideology, as indeed usually happens in the case of successful revolution—utopian symbols are retained regardless of their increasing divergence from the power facts.²⁸

This functional approach to differentiating between ideology and utopia serves to remind us that the same symbolic structure can have differential functions vis-à-vis the political system. This was the case with liberalism and Bolshevism, for example, each of which played a revolutionary role at one historical juncture and a preservationist role at another. For a number of reasons, however, Lasswell and Kaplan's approach does not solve the problem of boundaries in the conceptualization of myth, ideology, and utopia.

First, their conception of utopia is subject to the criticism made earlier of Mannheim; there is no way to know whether thought is utopian except in the positive instance where it facilitates the breakdown of existing power relationships or practices, and only after the fact. In other than a *post facto* way, therefore, the conception is analytically worthless.

In a second sense, moreover, this approach

which has a distinct political function. These are: the "*political doctrine*" which "formulates basic expectations and demands;" the "*miranda*" which consists "of basic symbols of sentiment and identification;" and, the "*political formula*" which "is the part of the political myth describing and prescribing in detail the social structure" (pp. 117–123; 125; 126–133, author's emphasis).

I shall concentrate on the relationships between political myth, ideology and utopia, because they relate more directly to the general theme of this paper. Some of the remarks that follow, however, would apply, *ceteris paribus*, to the logical relationships between political myth, doctrine, miranda and formula, as well.

²⁸ Lasswell and Kaplan, p. 123.

has only limited value, considered taxonomically. Using Lasswell and Kaplan's distinctions, we are put in the logically odd position of saying, for instance, that a particular symbol system was utopian, is now ideological, and may be utopian again, all without any significant change in the symbol system itself. And, at the same time that the system is either ideological or utopian, it remains mythical.

Third, to confuse the problem of classification even further, the same criteria of differentiation are not applied to all the symbolic structures under consideration in a way that would provide common units of comparison. On the one hand, the criteria for myth are more or less structural—involving fundamental assumptions about reality so basic that they do not appear to be assumptions at all. On the other hand, the criteria for ideology and utopia are functional—involving their propensity either to maintain or to alter existing political practices.

Fourth, the differentia proposed by Lasswell and Kaplan fail to recognize established usages or to honor distinctions which scholars normally utilize. For example, are the detailed schemes of social perfection portrayed in Plato's *Republic* and More's *Utopia* to be considered nonutopian because they never served "to induce fundamental changes in power relationships or practices"? On the other hand, does it make sense, at least without considerable qualification, to call Marxism utopian? Marx and Engels not only criticized "utopian" socialists for painting "fantastic pictures of future society" and for rejecting "all political, and especially all revolutionary, action"²⁹ but they also refused to spell out the details of communist society for fear of being considered utopian.³⁰

Finally, Lasswell and Kaplan's approach is weak because it relies on an *a priori* definition

which limits the empirical relevance of their conceptions. Instead of identifying ideology and utopia as cultural forms and then seeking to ascertain their functions in the political system, Lasswell and Kaplan define ideology and utopia in terms of their functions. The result is to make it impossible through empirical inquiry to falsify the assumption that ideology is functional, or that utopia is dysfunctional, to the maintenance of the political system. If, for example, Marxist "ideology" could be shown to be dysfunctional to the system, the notion that it is nevertheless functional could be saved by noting that, in this case, Marxism is functioning as utopia. But, surely, as an empirical matter, we want to know just what consequences certain standardized cultural forms have for the system. The consequences may be functional, dysfunctional or neutral. And this assessment can be made only by identifying the patterns, delineating the system, and studying their relationship empirically, not by settling the outcome through definitional fiat.³¹

A Comparison of Ideology, Myth, and Utopia

It was argued, in the preceding section, that the problem of boundaries in the conceptualization of ideology arises from the failure to maintain distinctions between ideology and other cultural phenomena (especially myth and utopia) with which ideology is often confused. Implicit in this argument was the additional notion that only by discerning differences in the cultural systems or symbolic structures themselves will one be likely to arrive at differentia which solve the problem of boundaries and afford empirically relevant concepts. The possibilities of this approach have been largely ignored by writers on ideology. There are, however, a few significant exceptions to this general rule, and to these writers I am considerably indebted. Lee C. McDonald recognizes that systematic distinctions between myth and ideology are rarely maintained,³² and he offers some valuable suggestions on how this might be done.³³ Another writer, Ben Halpern, has made

²⁹ Marx and Engels, "*Manifesto . . .*," p. 38.

³⁰ Marx and Engels' well-known reference to communist society in *The German Ideology*, hardly counts as an example of detailed utopian planning: "[I]n communist society, where nobody has one exclusive sphere of activity but each can become accomplished in any branch he wishes, society regulates the general production and thus makes it possible for me to do one thing to-day and another to-morrow, to hunt in the morning, fish in the afternoon, rear cattle in the evening, criticize after dinner, just as I have a mind, without ever becoming hunter, fisherman, shepherd or critic" (p. 22).

The most "utopian" aspect of Marxism is perhaps the expectation that in communist society something resembling utopian planning would be possible because, there, production is carried out "by freely associated men, and is consciously regulated by them in accordance with a settled plan." Karl Marx, *Capital*, Vol. I (Moscow: Progress, 1965), p. 80.

³¹ This argument is similar to one made by Robert Merton against the practice of assuming that every structure has a function. Whether this is so can only be decided on the basis of empirical inquiry. See, *Social Theory and Social Structure*, 2nd ed., rev. and enlarged (Glencoe, Illinois: The Free Press, 1957), pp. 32-36.

³² "Myth, Politics and Political Science," *Western Political Quarterly*, 22 (March, 1969), 150.

³³ McDonald, p. 149 and, especially, "Politics and Myth," in John A. Hutchison and Lee C. McDonald, *Myth, Religion and Politics* (The Methodist Church: Division of Higher Education, General Board of Education, 1965), pp. 26-27.

a most sophisticated and helpful analysis of ideology and myth as cultural forms in conjunction with a consideration of their sociologically dynamic properties.³⁴ Some additional insights into differences between ideology and myth as cultural forms are found in a short essay by Kenneth Burke.³⁵ Clifford Geertz has written what is, in my view, the best account of ideology as a cultural system, especially as it contrasts with the system of science.³⁶ Finally, Carl J. Friedrich has some useful ideas on the differences between myth, utopia, and ideology as cultural phenomena, although his analysis is dominated by a concern with the functional status of these forms.³⁷

I will attempt to incorporate and extend the insights of these writers by developing a typology that distinguishes among all three of the forms—ideology, myth, and utopia—and which does so with primary attention to their

³⁴ "The Dynamic Elements of Culture," *Ethics*, 65 (July, 1955), 235-249. Although Halpern is concerned with the socially dynamic functions of ideology and myth, he first establishes criteria for these phenomena, the functions of which are studied. He is attempting, in fact, to correct a deficiency in Talcott Parsons's theory of action which, while it studies the functions of cultural phenomena vis à vis the social system, "says very little about the systematic character of culture itself," or about how cultural forms guide actors in the social system (p. 236).

³⁵ "Ideology and Myth," *Accent*, 7 (Summer, 1947), 195-205, especially 198-200.

³⁶ "Ideology as a Cultural System" in *Ideology and Discontent*, ed. David E. Apter (New York: The Free Press, 1964), pp. 47-76.

³⁷ *Man and His Government: An Empirical Theory of Politics* (New York: McGraw-Hill, 1963), especially pp. 83-93 and pp. 94-105.

My view is that the question of an ideology's functions for a political system may be extremely important to the scientific understanding of political phenomena, and it is always, therefore, a matter with which students of ideology might be legitimately concerned. Two caveats, however, are in order. First, as I indicated in the previous section, questions about functions are empirically useful only after the identification of both what is functioning, and the system in relation to which there is a function. Second, the search for functions, with its emphasis on systemic patterns, should not obscure the fact that the patterns are abstractions from the concrete activities of individual actors who have feelings, motives, and purposes which are expressed and structured by the symbolic forms utilized by the actors. Friedrich is not susceptible to criticism regarding this second factor, although, regarding the first, he has a propensity to define ideology in terms of its functions.

The analysis of ideology in this paper, while relevant to problems of system-maintenance, is more directly concerned with how individuals and groups of individuals utilize symbol constructs to comprehend reality and direct their activities. For statements regarding the importance of not neglecting this relationship between individuals and culture in social explanation, see: Geertz, pp. 52-65; and, Halpern, pp. 235-236.

intrinsic features. Several prefatory remarks, however, are in order. First, there is no intention of presenting more than a general schema outlining the main factors which differentiate myth, utopia, and ideology. Second, therefore, the analysis concentrates on the salient differences among these cultural phenomena, rather than on the complexity of interrelationships among them. It should be remembered that, empirically, ideologies often include elements characteristic of myth, such as the notion of the social contract in liberalism, or the importance of the founders in Marxism-Leninism. And in most ideologies there is also an ideal of perfection, often unarticulated, which one might describe as "utopian." In an attempt to portray the general features of myth, utopia, and ideology, however, it will simplify matters to concentrate on prototypes. Moreover, inasmuch as these types are comprehensible it will be easier to discern the mythical and utopian elements incorporated by particular ideologies.

The discussion will begin with an analysis of the historical perspective implicit in ideological thought, and that, in turn, may be facilitated by placing ideology in historical perspective. Ideology, I maintain, is largely a modern phenomenon. It has little in common with a traditional social outlook, and although it can be discerned in earlier political movements,³⁸ it is not ubiquitous in Western thought before the French Revolution. The rise of ideology, in fact, appears to correspond to the breakdown in the traditional perspectives which characterized the *Ancien Régime*.

In times of historical crisis, when fundamental, time-honored, "self-evident" truths are questioned, ideologies bring the issue of a society's worth to the level of consciousness and guide men's actions over historical terrain that is, looked at in comparison to traditional society, terrifyingly unfamiliar and uncharted. This, I believe, helps to explain why ideologies are rapidly spreading from the West to the societies of the so-called "developing" world, where

³⁸ Some find the beginnings of ideology in earlier millennial Christian movements. See, for example, Melvin J. Lasky, "The Metaphysics of Doomsday," *Encounter*, 32 (January, 1969), 36-47; or, Michael Walzer, *The Revolution of the Saints: A Study in the Origins of Radical Politics* (Cambridge, Mass.: Harvard University Press, 1965). A more compact version of Walzer's view may be found in his article, "Puritanism as a Revolutionary Ideology," *History and Theory*, 3 (1963), 59-90.

To Michael Oakeshott, the watershed of ideological politics appears to be the American, not the French, Revolution. See his "Rationalism in Politics," in *Rationalism in Politics and Other Essays*, by Oakeshott (London: Methuen, 1962), p. 28.

traditional order is being disrupted and transformed. Recognition of the intimate relationship between rapid historical change and the appearance of ideologies, however, does not mean that ideology is a dependent variable—a mere mental reflex generated by the personal, structural and cultural dislocations in developing societies. Inasmuch as ideology conceptualizes the historical process and orients human beings for shaping it, ideology is itself an active agent of historical change.

By viewing ideology as it relates to the emergence of modern society, it is possible to discern what I consider to be a crucial aspect of ideological thought, and a basic criterion for differentiating between ideology, myth, and utopia. Ideology incorporates a type of "historical consciousness" characteristic of the modern age, in which a high degree of social change is recognized, where "'change' [means] the coming into being of new and specifically *different* constellations and not merely the replacement of one set of conditions by a new set of the same specific type, or simply the biological features associated with ageing in any natural process."³⁹

Since it perceives discontinuities in human society and culture, historical consciousness is concerned about the things to come. Hence, there is a need to invent conceptions of the future. How the future is conceived will be greatly influenced, of course, by how the present and the past are understood. But while there may be some sense of tradition in historical consciousness, tradition is no longer accepted as a sufficient guide to human action in a historical context. In historical consciousness—and in ideology—the shape of the future, the nature of historical change, and the limits and possibilities of human control over these changes, become questions of overwhelming importance.⁴⁰

³⁹ Frederick M. Barnard and Jene M. Porter, *Discontent and Political Development: A Discussion of Some Contemporary Methodological and Substantive Problems* (mimeographed), paper delivered to the Canadian Political Science Association meeting, Calgary (June 5, 1968), p. 12.

⁴⁰ Although there are enormous differences in their theoretical content and conclusions, most variants of liberalism, socialism and fascism view politics as working in accord with historical developments toward more preferable social arrangements. As an ideology, conservatism also considers the relationship between historical developments and political action, but it argues that given the nature of history and the limits of politics, men should avoid any but the most circumscribed political and social innovations. A basic difference between ideological conservatism and a traditional view, incidentally, is that, in the former, historical change is a practical and intellectual problem, whereas, in the latter, no such problem arises.

Historical consciousness, therefore, implies the possibility of imagining qualitatively new social arrangements, of empathetically placing oneself in them, and of conceptualizing the extent to which individuals, including oneself, might be causally effective in bringing new arrangements about. This, it seems, is not possible in the "traditional consciousness" of those who inhabit traditional societies. For example, David Riesman observes that in a series of interviews conducted in the Middle East, a Turkish peasant, when asked what he would do if he were president, responded:

'My God! How can you ask such a thing? How can I . . . I cannot . . . president of Turkey . . . master of the whole world?'

Correspondingly, [Riesman continues] many of the tradition-minded in these interviews, asked where they would like to live if they could not live in their native villages, said they would rather die; they could not conceive of living anywhere else, any more than of being somebody else.⁴¹

Clearly, the peasant views himself as being politically impotent, the member of a rigidly defined social structure which has always been, and will continue to be, essentially as it is. For him, there can be no program of political action; unable to conceive of a new social order, he would find it quite unthinkable to make any effort on behalf of its emergence. He is simply not historically aware.

The modern historical time which is an essential part of ideology differs fundamentally from the nonhistorical time of myth or utopia, both of which find their classic expression in the pre-modern era. In historical time, events are conceived of as occurring once at some determinate point on a linear continuum. Historical events, therefore, are viewed as unique, and social activities or structures, understood as historical events, are *sui generis*. In mythical time, on the other hand, the concern is not with what is unique in human experience, but with what is universal; "not with what happened but with what happens . . . in other words [with] the typical or recurring element in action."⁴² In the

⁴¹ "Introduction," in *The Passing of Traditional Society: Modernizing the Middle East*, by Daniel Lerner (New York: The Free Press, 1964), p. 3.

⁴² Northrop Frye, "New Directions From Old," in *Myth and Mythmaking*, ed. Henry A. Murray (Boston: Beacon, 1968), p. 116. Frye uses these phrases to describe the subject matter of the poet, but as he recognizes, the description applies equally well to the content of myth.

Recognizing the intimate relationship between poetry and myth, McDonald stresses the public nature of the latter: "Myths are poetry, but a special kind of poetry—the poetry men live by" ("Myth, Politics and Political Science," p. 141).

mythical time of primitive man, Eliade reports, "events were not thought of as happening once for all (as, we would say, true events must do) but were 'happening' eternally. The impression of an inexorable forward movement in time is an illusion; there is only 'the eternal return,' the constantly repeated cycle, the endless round."⁴³ Noting this emphasis in myth on the recurring aspects of human action, Halpern says:

In fact, the process involved in a myth vision is essentially a poetic one: it requires discovering in superficially diverse act-complexes underlying morphological identities—so that Proserpine descending to Hades exemplifies all experiences of temporal mortality and rebirth, the revolving seasons, the succession of day and night, and the cycles of sowing and reaping.⁴⁴

In politics, some of the most important myths concern the founding of the polity.⁴⁵ The founding myth often refers to heroic figures, real or imaginary, who created the political order. When persons invoke the name or recount the exploits of the founders, past and present are merged; the past is renewed in the present, even as the present draws significance from the past. By such practices, political communities celebrate the essential identity between the original act of founding and the continuing actions that extend and preserve the polity.⁴⁶ Hence in a society characterized by mythical consciousness, the social structure is not viewed as an arrangement that might be altered by each generation according to its perceptions of justice or convenience, but as a sacred and timeless order which is sanctified by the myths that explain its importance and its origin.⁴⁷

Utopia, like myth, conceptualizes time as recurrent and nonhistorical. First, because utopia

presents an intellectually reconstructed world without sin or human frailty,⁴⁸ and because of its emphasis on a perfected and closed social structure, it "does not, and perhaps cannot, recognize the unending flow of the historical process."⁴⁹ Although there is change in utopia, it is not historical change: "all processes going on in utopian societies follow recurrent patterns and occur within, and as part of, the design of the whole."⁵⁰ In utopian society there is no conception of a historical process within which social transformation is possible. "Considered as a final or definitive social ideal, the utopia is a static society; and most utopias have built-in safeguards against radical alteration of the structure."⁵¹ There is also a second sense in which classical utopia poses a disjunction with history. It is ahistorical because it neither incorporates an expectation that the ideal will ever be realized nor presents any historical program for its actualization. In the words of Judith Shklar, utopia portrays "a timeless 'ought' that never 'is.'"⁵² In his utopia, therefore, Thomas More echoes Plato's pessimism regarding the possibility of bringing utopia into existence through political means.⁵³ The function of clas-

⁴³ Myth has a religious quality of high seriousness; it is believed to be true and it deals with the intense, even tragic, character of real human experience. See Susanne K. Langer, *Philosophy in a New Key: A Study in the Symbolism of Reason, Rite and Art* (New York: Mentor, 1951), pp. 151-153.

Ideology is also characterized by serious intent; it is oriented to the problems of human beings living in society, and it is concerned with whether and how the human social condition may be improved. Utopia, on the other hand, is frankly imaginary and lacks the drama or tension of myth and ideology.

⁴⁴ Ralf Dahrendorf, "Out of Utopia: Toward a Reorientation of Sociological Analysis," *The American Journal of Sociology*, 64 (September, 1958), 115.

⁴⁵ Dahrendorf, p. 117.

⁴⁶ Northrop Frye, "Varieties of Literary Utopias," *Daedalus*, (Spring, 1965), p. 329. See also, Judith Shklar "The Political Theory of Utopia: From Melancholy to Nostalgia," in this same issue. Utopia, she says, "is an expression of the craftsman's desire for perfection and permanence. That is why utopia . . . is of necessity a changeless harmonious whole, in which a shared recognition of truth unites all the citizens. Truth is single and only error is multiple. In utopia there cannot, by definition, be any room for eccentricity" (p. 371).

⁴⁷ Shklar, p. 372.

⁴⁸ See Thomas More, *Utopia*, trans. H. V. S. Ogden (New York: Appleton Century-Crofts, 1949), pp. 18 and 24. In his closing sentence More admits "that there are many things in the Utopian Commonwealth that I wish rather than expect to see followed among our citizens."

In *The Republic*, see especially Book VI. For the argument that, according to *The Republic*, it is impossible to bring the good city into being, see the discussion by Leo Strauss, *The City and Man* (Chicago: Rand McNally, 1964), pp. 118-138.

⁴³ Quoted by John Knox, *Myth and Truth* (Charlottesville: The University of Virginia Press, 1964), p. 56.

⁴⁴ Halpern, p. 238.

⁴⁵ On the myth of the founding, see Friedrich, p. 96 and pp. 393-396.

⁴⁶ Regarding the mythical renewal of the past in the present, Thomas Mann says: "[L]ife in the myth, life, so to speak, in quotation, is a kind of celebration, in that it is a making present of the past, it becomes a religious act, the performance by a celebrant of a prescribed procedure; it becomes a feast. For a feast is an anniversary, a renewal of the past in the present. Every Christmas the world-saving Babe is born again on earth, to suffer, to die, and to arise. The feast is the abrogation of time, an event, a solemn narrative being played out conformably to an immemorial pattern; the events in it take place not for the first time, but ceremonially according to the prototype." See "Freud and the Future," in *Myth and Mythmaking*, ed. Murray, p. 374.

⁴⁷ See Bronislaw Malinowski, *Magic, Science and Religion and Other Essays* (Garden City, New York: Doubleday Anchor, 1954), pp. 100-101.

sical utopia is that of criticism; by postulating a condition of perfection, it reveals the limits of actual society. According to Leo Strauss, this is the role performed by Plato's utopia: "Socrates makes clear in the *Republic* of what character the city would have to be in order to satisfy the highest need of man. By letting us see that the city constructed in accordance with this requirement is not possible, he lets us see the essential limits, the nature, of the city."⁵⁴ But although the perfection of utopia stands as a condemnation of the real political system, the utopian model is not a guide for the existing system's transformation. As an object of contemplation it is "'prolonged and enjoyed for its own sake,'"⁵⁵ but it is not, in fact, a guide to any actual doing.

Judith Shklar has argued that after the French Revolution, utopias (in the classical sense) decline in importance. To the extent that utopias continue as a genre, they tend to retain the outward form, but lack the essence of, the classical archetype. Influenced by the "future-directed activism" of the postrevolutionary age, these utopias enter the stream of history as possible future societies, although their proponents argue only for their "historical non-impossibility."⁵⁶ In the words of Bertrand De Jouvenal, the typically utopian "journey to a distant place has been gradually superseded by the projection into a time distant in the future. . . ."⁵⁷

In this sense, these utopias resemble ideologies more than did their classical predecessors. But there is still a significant difference between ideologies on the one hand, and modern "historical" utopias on the other. The latter enter the realm of history in the sense that they might happen, but they do not confront the historical problems associated with transforming existing society. There is no preoccupation as in ideology with how, given the sociological context, the limits and possibilities of human action, and the political resources available, the good society might be more nearly approximated—or, to put it negatively, how the bad society might be avoided. And, while ideologies often contain "utopian" ideals of justice or the good society, they are not expressed as elaborate and aesthetically balanced models of perfection suspended in a "no place" or a "some time." In ideology such ideals are linked to concrete programs and strategies having imme-

diate social relevance. As such they become motives for action rather than mere intellectual constructs. No doubt this helps to explain why ideologies, not modern utopias, are associated with political parties and their programs.

In addition to conceptualizing change differently, myth, utopia and ideology may also be distinguished according to the symbols⁵⁸ by which they are constituted. Since it utilizes the highly figurative and metaphorical language of poetry, of course myth is not logical in form. Indeed, myth may be viewed as a mode of expressing basic principles or fundamental truths without recourse to philosophical language.

[S]uppose that you had no such words as "substance," "essence," "principle," "logical," as used in our traditional philosophical idiom. Yet at the same time you did feel the need to justify the social practices and relations of your society. . . . Is there any spontaneous vocabulary that might substitute for the missing terms?

There is indeed, and in primitive "myth" we see it everywhere. "Principles" means "firsts." The corresponding word in Greek is the word from which we derive "archetype," "archeology," "archaic," and the like. Hence, if you want to deal with logical principles, or "firsts," and don't have such a language, you get the equivalent by talking of *mythical* firsts. To derive a culture from a certain mythic ancestry, or ideal mythic type, is a way of stating that culture's essence in narrative terms.⁵⁹

The language of ideology is replete with metaphor; nevertheless, ideology, unlike myth, is cast in the form of logic. Whereas myth finds expression in the apposite image, ideology relies also on the correct category. Whereas myth personalizes events, encouraging "personal identification" with its symbols,⁶⁰ ideology is convincing, in part, because it facilitates a logical grasp of events and situations. Whereas myth is essentially plastic and spontaneous, something that grows with the telling, ideology can be written down and its truths "worked out" logically.⁶¹ Whereas the images of myth compress meaning, ideology tends to explicate the significance of events, situations and possible courses of human action.

⁵⁸ I use the term "symbol" in the broad and untechnical sense that "[a]ll linguistic expression is symbolic representation." (Friedrich, p. 100).

⁵⁹ Burke, pp. 199–200 (author's emphasis). Burke suggests that ideology, with its philosophical vocabulary, is a way of translating mythic truths into the forms of discursive thought (p. 201). See also, Halpern, pp. 240–241.

⁶⁰ Halpern, p. 240.

⁶¹ Hence ideology, unlike myth, can be identified as the mental product of a specified political or cultural elite.

⁵⁴ Strauss, p. 138.

⁵⁵ Shklar, p. 371.

⁵⁶ Shklar, pp. 374–378.

⁵⁷ "Utopia For Practical Purposes," *Daedalus* (Spring, 1965), p. 438.

In comparison with myth, ideology expands meaning, but in relationship to the language of utopia, the language of ideology is decidedly more sparse and economical. Utopia, being an imagined condition of social perfection, tends toward elaborate description, often down to the most minute detail. Ideology, on the other hand, informs political action. Hence, far more than utopia, it is inclined to simplify alternatives, to reduce complex situations to understandable proportions so that action is possible. Whereas utopia expands the details, ideology appraises them, selecting some as especially relevant, and relegating the rest to the background.

The Components of Ideology: An Elaboration

The foregoing comparative treatment of ideology, myth, and utopia focuses on a fundamental factor in ideology—historical consciousness. This factor, in turn, suggests other distinguishing features of ideology, although in the preceding section these latter differentia were either developed only briefly or merely left implicit in the analysis. In this section, therefore, the remaining criteria of ideology will be explicated and discussed in detail. These are, in the order of their consideration: (1) cognitive power; (2) evaluative power; (3) action-orientation; and, (4) logical coherence.⁶²

The cognitive power of ideology is attested to by Philip Converse in his analysis of belief systems in mass publics.⁶³ In the group surveyed, he finds a high positive correlation between the level of their knowledge about politics, the degree of their political activism, and the amount of "constraint" in their political beliefs. The notion of constraint is extremely important regarding the cognitive power of ideology, and it is also central to the notion of logical coherence, a feature of ideology to be discussed later. In Converse's usage, constraint refers to the logical coherence or consistency between various components of a respondent's beliefs, and he defines it as "the success we would have in predicting, given initial knowledge that an individual holds a specified atti-

tude, that he holds certain further ideas and attitudes."⁶⁴ Constraint, therefore, refers not to randomly related specific attitudes but to attitudes which are organized into "wide ranging belief systems."⁶⁵ By emphasizing the variable of constraint in his measurement of political ideologies, Converse accentuates the important cognitive role they play in arranging political information so that it is meaningful to "the truly involved citizen" who seeks to understand, and to participate in, political life.⁶⁶ Converse asserts that for these people, highly constrained ideologies are able to organize "economically" extensive information which contains many elements into systems whereby the elements "may be described and understood."⁶⁷ He maintains that

the use of such basic dimensions of judgment as the liberal-conservative continuum betokens a contextual grasp of politics that permits a wide range of more specific idea-elements to be organized into more tightly constrained wholes. . . . With it, for example, new political events have more meaning, retention of political information from the past is far more adequate, and political behavior increasingly approximates that of sophisticated "rational" models, which assume relatively full information.⁶⁸

Hence, Converse says of the ideologue and near-ideologue: "Empirically they differ sharply from the less sophisticated in their attention to new political events and in the size of their store of information about past events."⁶⁹

At first glance, it may seem odd that the cognition and retention of information about politics should be promoted through the use of abstract categories like "liberal" or "conservative," or that Converse would suggest an affinity between ideologies and "rational" models. Rationalistic abstractions, however, are vital to various forms of human understanding. Abraham Kaplan observes that even in the primordial, essentially spontaneous, cognitive activity of "identifying" and symbolizing recurrent aspects of experience, abstraction and generalization occur. "There is," he says, "no cognition without recognition, that is, without a constancy of some kind by which what is being known is recognized for what it is." And when we engage in identification, he adds, we recognize "a sameness beneath differences in space, time, or appearance—a One in the Many."⁷⁰

⁶⁴ Converse, p. 207.

⁶⁵ Converse, p. 229.

⁶⁶ Converse, p. 246.

⁶⁷ Converse, p. 214.

⁶⁸ Converse, p. 227.

⁶⁹ Converse, p. 232.

⁷⁰ *The Conduct of Inquiry: Methodology for Behavioral Science* (San Francisco: Chandler, 1964), p. 85.

⁶² Although in the pages to follow the components of ideology, as cultural phenomena, are analyzed separately, one should keep in mind that at the level of individual psychology these components appear to be subtly interconnected and interdependent. This is shown by Richard M. Merelman in his article, "The development of Political Ideology: A Framework for the Analysis of Political Socialization," *American Political Science Review*, 63 (September, 1969), 750-767.

⁶³ "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. Apter, pp. 206-261.

At another, more self-conscious, level of understanding, explanation in natural and social science generally involves the practice of intentionally abstracting from the welter of events certain uniformities which are considered worthy of special attention. In political science, for example, Anthony Downs applies rationalistic constructs to the study of politics in a manner resembling the economists' hypothetical treatment of the "rational" consumer.⁷¹ His method is first to abstract from empirical political behavior such key variables as political man's propensity to gratify his wishes through the utilization of appropriate means by gaining control over public policy. Downs then constructs a deductive model based on the assumption that political participants act according to this kind of "rationality." The model allows him to concentrate on certain prominent features of political action by abstracting these features and ignoring others. Hence, he argues:

[W]e do not take into consideration the whole personality of each individual when we discuss what behavior is rational for him. We do not allow for the rich diversity of ends served by each of his acts, the complexity of his motives, the way in which every part of his life is intimately related to his emotional needs.⁷²

This highly formalistic method may or may not be successful, but it is offered as an example of what any systematic theory attempts to achieve—an explanation that abstracts from complex phenomena by emphasizing their more important aspects, not their details.

The process of simplifying and ordering for purposes of explanation and understanding, however, extends beyond the practice of science to other areas of human thought. In the world of the citizen and politician, as in the world of the scientist, rationalistic constructs are necessary in order to comprehend the myriad events and activities which one seeks to understand. This process of abstraction is crucial in the political arena, where actors are continually forced to make policy choices based on their information about the situation. Either they must choose consciously to preserve, alter, or eradicate their social condition, or, without a political tradition, they drift willy nilly with circumstances over which they exercise no control. Henry David Aiken has argued, therefore, that

simplification is an essential feature of any rational initiation of action. To refuse to simplify when

one confronts a problem is in effect to reject the obligation to reach a solution; it is to make a game of possibilities and hence to move automatically outside the context of agency and choice.⁷³

There are, of course, significant differences between science and ideology. In science one aspires to a self-critical, analytical, "objective" understanding of phenomena without reference to normative categories which assign value to alternative courses of action. Seeking to maximize intellectual clarity, science is not concerned with dilemmas of policy, choice or commitment. In politics, however, it is necessary to understand situations in terms of their moral significance for human beings, and the normative language of ideology provides reasons for supporting one social arrangement rather than another, reasons why this or that political program should be instituted, defended or abandoned. Science may be the final arbiter of an ideology's "factuality." Ideology, however, comprehends situations in a way which science cannot for ideology, unlike science, is able to portray the "facts" in terms of their relevance for human wants and aspirations.

This observation identifies another distinguishing feature of ideology: its role in the evaluation of policies and programs. Political ideology has particular relevance to the area of political agency because, in addition to providing a relatively structured and consistent conception of the causal forces operating in the social world, it also incorporates evaluations of what is conceived. Moreover, ideology conceptualizes and evaluates the contours of reality, not merely as it exists, but also as it might be shaped depending on the intervention of politically organized human beings in the historical process. And the power of these "experiments in the imagination" derives not only from their capacity to anticipate symbolically the physical features of a situation; their deeper significance lies in their ability to convey the human meaning of possible events or conditions. Aiken writes:

I can assent to the proposition that on the first day of an atomic war every major city in the United States would be destroyed, without in the least realizing, in human terms, what the statement really means. In order that I may even remotely grasp such an idea, in absence of the event, I must somehow try symbolically to live through the horror and the agony of such a calamity. But this is precisely what the cold, literal, objective statement of fact does not require me to do.⁷⁴

⁷¹ Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957).

⁷² Downs, p. 7.

⁷³ "The Revolt Against Ideology," in *Ideology, Politics and Political Theory*, ed. Cox, p. 154.

⁷⁴ Aiken, p. 153.

The language of science is too meager to capture the moral dimensions of such situations.

The evaluative component of ideology should not be equated with an "emotivist" view wherein ideologies mobilize mass publics through their power as "secular religion[s]" that both trigger and express "deep moral feelings,"⁷⁵ or by providing "ideological passion" that has the effect of "arousing and unleashing energies."⁷⁶ In philosophy, the "emotivist" view of normative propositions elaborated by A. J. Ayer and others,⁷⁷ argues that the only adequate test of a proposition's meaning is empirical verification; that normative terms, having no objects, lack any cognitive status; and that, lacking a cognitive role, such terms operate only to express and stimulate moral sentiment. Viewed as effects, the argument continues, normative terms are mere ejaculations of approbation and disapproval; conceived of as "causes," they are "triggering mechanisms" which elicit negative or positive responses from the recipient of value-laden communications.⁷⁸

The contention that normative symbols operate solely to express feelings and arouse emotions, it may be admitted, has a certain *prima facie* appeal, because we are all familiar with instances in which certain symbols are met with a passionate expression of like or dislike, or cases in which strong feelings of attraction or revulsion for an object, event, or institution result in the expression of symbols that appeal to similar sentiments in others. I will not attempt here to consider whether such cases are adequately explained in terms of "causal" analysis, although I am inclined to believe that they are not.⁷⁹ I do contend, however, that these cases, and their explanation according to emotivist theory, do not take into account the extremely important role normative terms play in evaluation, especially the evaluations related to political action. For evaluation—the actor's ap-

praisal of his environment and its potential for change in terms of his needs and aspirations⁸⁰—is not the equivalent of expressing or arousing emotions, even though both activities involve the employment of normative terms.

An elaboration of this point will serve also to clarify another important feature of ideology—its action-component. Evaluation is "causally" related to purposive action in a much different way than symbols are said to "cause" political mobilization in the "emotivist" conception. In purposive explanation we do not, in fact, refer to antecedent events which are said to "cause" any kind of doing; rather one refers to conditions or events which the actor is attempting to bring about. Hence, "to explain by purpose is to explain by the goal or result aimed at, 'for the sake of' which the event is said to occur."⁸¹ If we speak of common purposes, as we usually do in the discussion of ideologies, we mean that each member of a group "accepts a description either of a fairly specific state of affairs or of a more generalized objective (such as 'social security for all'), to be effected if possible through deliberating and acting together."⁸² The importance of ideology for politics, it seems to me, resides in its power to communicate cognitions, evaluations, ideals, and purposes among members of groups. It enables them to appraise their political condition and its prospects for the future and thereby facilitates the mobilization and direction of energies and resources for common political undertakings. The significance of ideology in mobilization is not that it "causes one to do" but that it "gives one cause for doing." It provides grounds or warrants for the political activity engaged in. Reference to purposes and evaluations gives meaning to political actions, making them comprehensible to oneself and others. To explain the mobilizing aspects of ideology in terms of triggering mechanisms or the release of "ideological passion," therefore, ignores a vast and important aspect of how political actors understand and orient their political activi-

⁷⁵ Bell, pp. 400–401.

⁷⁶ Sartori, pp. 403–405.

⁷⁷ For expressions of the "emotivist" position in ethics see A. J. Ayer, *Language, Truth and Logic*, 2nd ed. (New York: Dover, 1946), and Charles L. Stevenson, *Ethics and Language* (New Haven: Yale University Press, 1944). For the systematic use of this approach in the analysis of ideology, see Gustav Bergmann, "Ideology," *Ethics*, 61 (April, 1951), 205–218.

⁷⁸ See Bergmann, pp. 206, 211.

⁷⁹ See, for example, A. I. Melden, "Reasons for Action and Matters of Fact," *Proceedings and Addresses of the American Philosophical Association*, 35 (October, 1962), 45–60, and A. R. Louch, *Explanation and Human Action* (Berkeley and Los Angeles: University of California Press, 1966), especially pp. 134–158.

⁸⁰ My notion of evaluation is consistent with the more explicit but cumbersome definition of values proposed by Clyde Kluckhohn: "A value is a conception, explicit or implicit, distinctive of an individual or characteristic of a group, of the desirable which influences the selection from available modes, means, and ends of action." See "Values and Value-Orientations in the Theory of Action," in *Toward a General Theory of Action*, ed. Talcott Parsons and Edward A. Shils (New York: Harper Torchbooks, 1962), p. 395.

⁸¹ Charles Taylor, *The Explanation of Behaviour* (London: Routledge and Kegan Paul, 1964), pp. 5–6.

⁸² Dorothy Emmet, *Function, Purpose and Powers: Some Concepts in the Study of Individuals and Societies* (London: Macmillan, 1958), p. 113.

ties, and how observers, attempting to understand these activities, might explain their significance.

In the preceding pages some of the main factors in my conception of ideology—historical consciousness, cognitive power, evaluative power, and action-orientation—have been explored. The remaining prominent feature of ideology, its logical coherence, has been treated implicitly during much of the discussion, but several more explicit remarks regarding this criterion of ideology will be useful. By stipulating that ideology takes a logically coherent form, I mean to distinguish “constrained” ideologies from any random or inchoate ideas, attitudes, or feelings about politics. This is not to claim, as one might assume, that ideology observes the strictures of formal logic; ideology is not normally an exercise in syllogistic reasoning, and its arguments rarely proceed with “logical necessity,” even if, occasionally, it may give that appearance. The logic of ideology is usually as broad as the conceptions, reasons, and justifications that typically “count” in the sociocultural structure (or, to use Wittgenstein’s phrase, “form of life”) within which the ideology operates.⁸³ Nevertheless, given these variegated logical resources (which include, but are always much broader than, those allowed by formal logic), ideology must not repeatedly violate their canons of sensibility. Within their confines the ideology must “make sense” and not result in logical absurdities. At one extreme, this conception of ideology will allow us to distinguish between it and the *ad hoc*, piecemeal appeals of propaganda. At another extreme, ideology becomes distinguishable from ambiguous and unarticulated conceptions such as ethos, spirit of the times, *Weltanschauung*, or public opinion.

In the foregoing analysis it is argued that ideology may be distinguished from other cultural forms by the combined presence of several elements—historical consciousness, action-

orientation, cognitive power, evaluative capacity and logical coherence. Hence I would define ideology as a logically coherent system of symbols which, within a more or less sophisticated conception of history, links the cognitive and evaluative perception of one’s social condition—especially its prospects for the future—to a program of collective action for the maintenance, alteration or transformation of society.⁸⁴

It is hoped that this definition and the analysis on which it rests will help to clarify the concept of ideology, making it more relevant to the already substantial—and continuing—research concerning the nature and role of ideology in politics. Of course, before this concept of ideology can be made operational for empirical research, further conceptual refinement will be necessary. This paper proceeds on the conviction, however, that if the basic theoretical issues underlying a concept’s usage are treated systematically and self-consciously, the empirical techniques and results which are shaped by that concept will be of greater merit. Certainly, in the case of ideology, an ordered attempt to give the concept empirical discriminating power by clarifying its relations to other phenomena of political culture is long overdue, and I hope that this paper has made some progress in that direction.

⁸⁴ This definition is, to varying degrees, in accord with the usage of some other contemporary writers on ideology in several academic disciplines. See: Aiken; Zbigniew B. Brzezinski, *Ideology and Power in Soviet Politics* (New York: Praeger, 1962); Converse; Friedrich; Geertz; Merelman; Joseph LaPalombara, “Decline of Ideology: A Dissent and an Interpretation,” *American Political Science Review*, 60 (March, 1966), 5–16; Walzer, “Puritanism as a Revolutionary Ideology.”

The observation of another writer on ideology would seem to indicate that the factors included in my definition have a wider acceptance: “Agreement on the meaning of the term [ideology] is far from universal, but a tendency can be discerned among contemporary writers to regard ideologies as *systems* of belief that are elaborate, integrated, and coherent, that justify the exercise of power, explain and judge historical events, identify political right and wrong, set forth the interconnections (causal and moral) between politics and other spheres of activity, and furnish guides for action.” Herbert McClosky, “Consensus and Ideology in American Politics,” *American Political Science Review*, 58 (June, 1964), 362 (author’s emphasis).

⁸³ See Dallas M. High, *Language, Persons and Belief: Studies in Wittgenstein’s Philosophical Investigations and Religious Uses of Language* (New York: Oxford University Press, 1967), pp. 211–212.

Preconditions of Mayoral Leadership*

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Introduction: The Search for Mayoral Leadership

After reviewing the problems which constitute the contemporary "urban crisis," most observers come to the conclusion that urban political leadership—usually mayoral leadership—is a crucial ingredient in a city's ability to deal with those problems. The President's Riot Commission was outspoken on this subject:

Now, as never before, the American city has need for the personal qualities of strong democratic leadership. Given the difficulties and delays involved in administrative reorganization or institutional change, the best hope for the city in the short-run lies in this powerful instrument. In most cities the mayors will have the prime responsibility.¹

For the Commission, the role of the mayor is an awesome one:

As leader and mediator, he must involve all those groups—employers, news media, unions, financial institutions and others—which only together can bridge the chasm now separating the racial ghetto from the community. His goal, in effect, must be to develop a new working concept of democracy within the city.²

Proponents of mayoral leadership readily admit that few mayors have formal authority and resources which are adequate to deal with the enormous tasks facing them. But this is not considered an insuperable obstacle. Alexander George has written that

The pluralistic structure of dispersed, decentralized power requires a political leader who can accumulate personal influence to supplement his limited formal authority. Studies of successful leaders in urban systems have led to the formulation of a simple, but incisive model of the "political entrepreneur"—a political activist adept at accumulating a variety of political resources and using them to gain

influence and additional resources.³

An alert mayor can view political fragmentation as providing numerous opportunities for the "brokerage" function—mediating disputes, serving as a line of communication, and suggesting integrative solutions in which diverse groups can all gain. And in performing this function, the mayor finds his own power increased. "To the political entrepreneur, who possesses skill and drive, the pluralistic dispersion and fragmentation of power in democratic systems offer unusual opportunities for pyramiding limited initial resources into a substantial political holding."⁴ Thus, effective political leadership in a fragmented system is proclaimed to be a necessity in American cities. And political scientists have provided a model of the broker-entrepreneur who can fill that need.⁵

It is easy to advocate "mayoral leadership" and "political entrepreneurship." But what does successful "leadership" mean in a modern American city? Andrew McFarland has defined leadership in the following way:

A leader may be defined as one who has unusual influence. Influence may be viewed as one's capacity to make people behave differently than they would have otherwise. A leader may also be defined as one who has unusual power. Here we view "power" as a person's capacity to make others do something that they would not do otherwise and that the person specifically wants or intends.⁶

According to this social causation definition, a mayor may be said to exercise leadership when he alters human behavior in ways that he himself desires. Thus, he must set goals and influence others to aid in the pursuit of those goals.

* Alexander L. George, "Political Leadership and Social Change in American Cities," *Daedalus* (Fall, 1968), p. 1197.

¹ George, p. 1197.

² See Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), pp. 225–227, 308; Edward C. Banfield, *Political Influence* (New York: Free Press, 1961), chap. 8 and pp. 17, 309, 312–313, 320–321; Aaron Wildavsky, *Leadership in a Small Town* (Totowa, N.J.: Bedminster Press, 1964), pp. 244–245, 248; H. H. Gerth and C. Wright Mills, *From Max Weber* (New York: Oxford University Press, 1946), p. 109; and Andrew S. McFarland, *Power and Leadership in Pluralist Systems* (Stanford, Calif.: Stanford University Press, 1969), pp. 153–219.

³ McFarland, p. 154.

* This paper is based upon four years of participation in, and observation of, the mayor's office in Oakland. The research was conducted under the auspices of the Oakland Project at the University of California at Berkeley, headed by Aaron Wildavsky and funded by the Urban Institute. I would like to thank Professor Wildavsky, Robert Biller, William K. Muir, Jr., Bill Cavala, and Robert Nakamura for their comments.

¹ *Report of the National Advisory Commission on Civil Disorders* (New York: Bantam, 1968), p. 298.

² *Report of the National Advisory Commission on Civil Disorders*, p. 299.

Model of Mayoral Leadership. For those who call for strong and progressive mayors, a model of effective mayoral leadership should include a specification of: (1) the goals toward which a mayor might strive; (2) those groups in his environment which he must influence in support of those goals; (3) the ways in which he may influence them; and (4) the resources required for an effective exercise of power.

The goals toward which a model mayor might devote his energies and lead his constituency are numerous, varied, and difficult to accomplish. Writers on urban affairs have listed numerous possible goals: effective law enforcement; redevelopment; relocation of those living in redeveloped areas; expansion of the tax base; construction of low-cost public housing; improvement of the schools; creation of jobs and job training programs. Besides these substantive goals, the ideal mayor might strive to maintain within the political system a process of constructive dialogue between diverse groups which would contribute to harmony in the city.

In pursuing his goals, a mayor would have to exercise control and direction over the city council and relevant city departments: schools, redevelopment, housing, police, etc. The successful mayor would attempt to stretch his "legal" jurisdiction as far as possible. Furthermore, he would have to activate nongovernmental groups in the community to support his efforts by promising rewards and threatening sanctions. He would have to persuade businessmen to locate factories in his city and to initiate job training programs, and he would have to persuade labor unions to open places in apprenticeship programs to minority groups.

An ideal mayor would use his control of the mechanism of a political party to further his policy preferences, by supporting the nomination and election of candidates who were willing to support him. Furthermore, the model mayor would be willing to use publicity in order to appeal to the public to support him against opponents by voting in elections or by trying to influence recalcitrant groups.

In order to exercise leadership over these various groups—in order to make them do what they would otherwise *not* do—the ideal mayor would require various resources. They would include:

- (a) Sufficient financial and staff resources on the part of the city government;
- (b) City jurisdiction in social program areas—such as education, housing, redevelopment, job training, etc;
- (c) Mayoral jurisdiction within the city gov-

ernment in these policy fields;

- (d) A salary for the mayor which would enable him to spend full time on the job;
- (e) Sufficient staff support for the mayor—for policy planning, speech-writing, intergovernmental relations, and political work;
- (f) Ready vehicles for publicity, such as friendly newspapers or television stations;
- (g) Politically-oriented groups, including a political party, which the mayor could mobilize to help him achieve particular goals.

As this list suggests, the opportunities for mayoral leadership in a particular case may be limited by the political structure of a city (electoral system, parties, groups), by governmental structure, and by the lack of staff, jurisdictional and financial resources. Furthermore the quality of mayoral leadership may be strongly influenced by the personality of the mayor himself. Although social forces and governmental structures set limits to what *any* man might be able to do as mayor in a particular city, personal variability should not be discounted.⁷ Thus in examining the quality of mayoral leadership, we must ask to what extent a given mayor's performance is influenced by social forces and governmental structure, and to what extent that performance is influenced by the mayor's personality.

Perhaps because of most mayors' lack of extensive governmental authority, observers of city politics have not concerned themselves with the problem of unrestrained mayoral leadership. But an enumeration of forces for restraint might include the mayor's dependence on independent groups for the attainment of his

⁷ Fred I. Greenstein has stated that the likelihood of personal impact on politics (1) increases to the degree that the environment admits of restructuring; (2) varies with the actor's location in the environment; and (3) varies with the personal strengths and weaknesses of the actor. See Greenstein, "The Impact of Personality on Politics: An Attempt to Clear Away Underbrush," *American Political Science Review*, 61 (September, 1967), 634. For Greenstein, personal variability is most likely to manifest itself in those fluid situations in which "political actors lack mental sets which might lead them to structure their perceptions and resolve ambiguities" (p. 638). In positions which are typically free from expectations of fixed content, personal variations will be more evident (p. 639). McFarland also notes the importance of an ambiguous environment; he sees leadership opportunities in "conflicting expectations among others about the person's behavior in a position." McFarland, p. 203.

goals and the existence of competitive elections in the city.

This paper will explore the politics of mayoral leadership—or nonleadership—in a particular city: Oakland, California, under the administration of Mayor John H. Reading. The second part examines the often hostile environment in which the mayor works—the city of Oakland and its political structure, the city government, and the mayor's own office. The third part focuses on the personality of the mayor himself, using material drawn from speeches, interviews, newspaper articles, and personal observation. The last part analyzes the mayor's performance in light of the resources available (or potentially available) to him.

By examining mayoral leadership in a particular city, and by comparing that leadership to the model described above, this essay will suggest some of the difficulties surrounding urban leadership in an era of racial tension, confrontation, and insufficiency of urban resources. If we understand these difficulties, we will be better prepared to chart the preconditions for successful mayoral leadership.

Although some of the vital preconditions—financial resources, governmental authority, groups, etc.—may be missing, a mayor can still attempt to be a leader. But the style and consequences of his leadership depend to a large extent on the type of governmental and political system in which he operates; the concluding section of this paper will discuss some of the connections between political environment and leadership.

Environment

The City. Oakland's population in 1970 was 361,561, of which 34.5 per cent was black.⁸ Studies have repeatedly shown that minority groups (blacks and Mexican-Americans) are heavily over-represented in both the poor and unemployed population of the city.⁹ And Oakland's overall unemployment rate has reached a level more than twice the national average.¹⁰ This is the kind of city which is discussed at length in the news media and in the growing "urban crisis" literature. It is characterized by an increasingly large minority population, by geographical separation and emotional tension

between races, and by continuing problems of poverty and unemployment.

Political Environment: The "Non-Politics" of Oakland. One of the requirements postulated for successful mayoral leadership is the existence of "politically-oriented groups, including a political party, which the mayor can mobilize to help him achieve particular goals." Such groups and parties would give the mayor an opportunity to identify interests which had become aggregated by and articulated through organizations. Furthermore, groups and parties could perform the function of recruiting political leaders with whom the mayor could bargain. Finally, the mayor could use friendly groups as vehicles for political education in the goals toward which he was working and as organizing devices to bring citizens together in support of those goals.

In Oakland, a fundamental obstacle to mayoral leadership is the nature of the political environment itself. For this environment is characterized by a lack of interest in the electoral process and an absence of political parties and groups.

It often appears as though politics in Oakland does not exist. In 1969, when elections were held for mayor, city council, and the board of education, the candidate response was tepid at best. Two little-known men emerged to challenge Mayor Reading's attempt for reelection; two new candidates filed against two of the three incumbents on the school board; and all three city councilmen who were up for reelection were unopposed.

In a city marked by serious divisions, what causes the avoidance of electoral politics? Many answers have been suggested: the lack of resources and power in city government; the middle- and upper-class white bias of the at-large election system, which makes candidacies expensive and victories for nonwhites difficult to achieve; and, finally, the paltry salaries of elected officials.

Another reason for the anemia of Oakland's electoral process has been the noticeable absence of political party activity in municipal elections. This absence has, no doubt, been encouraged by the nonpartisan nature of the municipal ballot, which gives voters no indication of the party to which a candidate belongs. But formal nonpartisanship is not a sufficient explanation for party inaction; it has not prevented extensive activity by Democratic and Republican organizations in neighboring Berkeley and San Francisco.

A basic cause of party inactivity in Oakland has been the lack of organized politically interested groups in the city upon which a party or

⁸ Census report cited in the *Oakland Tribune*, March 14, 1971.

⁹ See, for example, Survey Research Center, University of California, *Poverty and Poverty Programs in Oakland* (Berkeley, 1967) and Stanford Research Institute, *Human Resources Development for Oakland: Problems and Policies* (Menlo Park, Calif., 1968).

¹⁰ Stanford Research Institute, p. 10.

mayor could build. When an organization of high-school students tried to form a "coalition" in 1968 and 1969 to work for changes in school procedure and curriculum, they found, as one leader put it, "that there was no one to coalesce with." Labor has not played a significant role in city politics since the 1940s. And business groups, though formally endorsing certain nonpartisan causes like charter reform, have not heavily involved themselves in city politics.

The absence of active parties and groups, and of citywide politics itself, constitutes a grave limitation on mayoral leadership. Additional limitations are provided by the structure and process of the city government.

The Limits of City Hall Control. It is important at the outset to understand the jurisdictional limits of city government in Oakland. For only a few of the problem areas that we usually associate with the "urban crisis" are directly under the control of the city council and city manager. The school system, for example, is completely independent of city hall, except for the provision that the city council may pass a 5 per cent "emergency tax" for the schools. Another powerful independent body is the Oakland Board of Port Commissioners, which has exclusive control over management of the massive Port of Oakland.

Although redevelopment and housing have been two of the most controversial policy areas in Oakland, the commissions that have jurisdiction in these areas are the autonomous Redevelopment Agency and Housing Authority. These two organizations have had strained relations with each other, as well as with the city government.

Finally, the local poverty agency, the Oakland Economic Development Council, Inc., has been militantly independent of city hall since 1967. This agency operates a host of OEO programs, as well as the Department of Labor's Concentrated Employment Program.

The implications of fragmentation for mayoral leadership are fairly serious. Without jurisdiction, a mayor may be powerless to control outcomes in policy areas which are important to him. In Oakland, the mayor has been disturbed both by the slow pace of renewal and by the lack of relocation opportunities caused by the slowness of progress in public housing. But he has encountered difficulties in communicating with the two autonomous agencies involved, let alone in forging a bond of cooperation between them. Although a charter revision in 1968 brought the parks, recreation, library, and museum commissions more closely under city

hall's control, the critical agencies discussed above remained autonomous.

Now that we have considered some of those governmental bodies which are outside city hall, let us look more closely at the inside of city government itself.

The Separation of Politics from Administration.

In a seminal article which first appeared in 1887, Woodrow Wilson declared that "administration lies outside the proper sphere of *politics*. Administrative questions are not political questions. Although politics sets the tasks for administration, it should not be suffered to manipulate its offices."¹¹ Politicians should set policy, and scientific administrators should carry it out. This separation of politics from administration, a hallmark of the Progressive era in American politics, lies at the root of the council-manager form of government. In Oakland, according to the city charter: "The Council shall be vested with all powers of legislation." The charter goes on to state, however, that the council "shall have no administrative powers."¹² These are reserved for the city manager, who has "administrative" power over city government departments—police, fire, street and engineering, parks, recreation, and so on.

As for the mayor, who is a member of the council, the charter makes his duties sound impressive:

The Mayor shall be the chief elective officer of the City, responsible for providing leadership and taking issues to the people and marshalling public interest in and support for municipal activity. He shall recommend to the Council such legislation as he deems necessary and shall encourage programs for the physical, economic, social and cultural development of the City. He shall preside over meetings of the Council, shall be the ceremonial head of the City, and shall represent the City in inter-governmental relations as directed by the Council.¹³

In theory, the mayor leads public opinion, the council formulates policy, and the manager carries it out. But in practice, the relationship between politics and administration in Oakland is strongly affected by the personalities of the men holding city offices and by the wide disparity in the levels of resources available to the politicians on the one hand and the administrators on the other.

An examination of comparative resources helps to explain why there is a strong tendency

¹¹ Woodrow Wilson, "The Study of Administration," reprinted in *Political Science Quarterly*, 56 (December, 1941), 494.

¹² *Oakland City Charter*, Sec. 207.

¹³ *Oakland City Charter*, Sec. 219.

in Oakland for administration to devour politics—for the mayor and council to follow the manager's lead in the policy-making process. Councilmen, who earn \$3600 annually from the city, and the mayor, who is paid \$7500, find it financially difficult to spend much time at their jobs; the manager's annual salary of \$38,940 permits him to do so. The entire city council is served by one secretary, and the mayor is not much better off with three secretaries and an administrative assistant. But the city manager, besides having three full-time staff assistants, is able to utilize the manpower and informational resources of all city departments under his control. As a result, the city manager tends to know more than anyone else about city government structure, processes, and substantive policy.

A potential monopoly on staff and informational resources is not, however, the only reason for city manager domination of the Oakland governmental process at the present time: Jerome Keithley, the current city manager, is a man who is unusually adept at dealing with the internal environment of city government. The city council's power to fire the city manager gives councilmen an ultimate trump card to play against the manager, but Keithley's performance in his job has gained him the solid and enthusiastic support of the council. There are a number of reasons for this support.

First of all, Keithley shares the views of most councilmen about what the goals of city administration should be: efficiency, cost cutting, and thus lower taxes. The city manager defines his role as an inside administrator, avoiding ambitious social programs. Secondly, City Manager Keithley uses his access to departmental records to satisfy as quickly as possible councilmen's requests for information. And he permits councilmen to make decisions in areas in which they are particularly interested. For example, the city manager allows the council to have jurisdiction over the budget for "civic organizations" such as the Chamber of Commerce, the Elks Club, the Italian-American Federation, and the California Spring Garden and Home Show Committee. Finally, Keithley builds support on the council by being "one of the guys." He is a regular attendee at city council dinners and at ball games at the Coliseum—events that the mayor often avoids.

Armed with his considerable advantages, the city manager defines "policy" and "administration" in such a way that "administration" turns out to bulk very large and "policy" very small. When Mayor Reading complained in July 1968 that the police chief's stringent restrictions on

policemen's use of guns (a decision in which the city manager had concurred but about which the mayor had never been consulted) constituted a dangerous policy decision which should be overturned by the council, the city manager disagreed. The manager's reasoning was clear: "A policy decision would be that policemen in Oakland should carry guns. Administrative decisions would be when they should carry guns, where they should carry guns, and how they should use those guns."

The city manager's definition of policy and administration is usually supported by the city attorney, Edward Goggin, who is appointed by the council but who takes his lead from the city manager.¹⁴ Goggin has a penchant for defining the area of permissible action by the city government as extremely narrow; within the government, Goggin tends to rule against city council jurisdiction. When the mayor sought to investigate his supposed new powers under the Green Amendment to the poverty program, the city attorney advised him that the amendment—which allows city governments to take control of poverty programs—could not apply to Oakland. For the legislation provided that, in taking over the new jurisdiction, a city council would administer the program. And Goggin pointed out that the Oakland City Charter specifically prohibits the council from exercising administrative power.

The Typicality of Oakland's Governmental System and Electoral Rules. Table 1 shows the form of city government for cities with more than 5000 population. For cities in Oakland's population group (250,000–500,000), the council-manager system is the most popular form of government. This plan is particularly prevalent in states along the Pacific coast; 84.3 per cent of the cities (above 5000) in those states have adopted the council-manager plan.¹⁵

As to electoral rules, Oakland's nonpartisan form of local elections is widely used by cities in its population group; 76 per cent of cities in that range do not list parties on the ballot.¹⁶ But in another respect, Oakland's electoral system is unique. Seven of the city's nine councilmen must reside in and run for office from particular geographical districts; one councilman

¹⁴ For a description of the crucial role in policy making that can be played by a city attorney, see "The Cambridge City Manager" in *Public Administration and Policy Development: A Case Book*, ed. Harold Stein (New York: Harcourt Brace and Company, 1952).

¹⁵ *The Municipal Year Book*, 1968, p. 54.

¹⁶ *Municipal Year Book*, 1968, p. 58.

Table 1. Form of Government in Cities Over 5,000 Population

Population Group	No. of Cities Reporting	Mayor-Council		Council-Manager		Commission	
		No.	%	No.	%	No.	%
More than 500,000	27	22	81.5	5	18.5	—	—
250,000 to 500,000	27	11	40.7	13	48.2	3	11.1
100,000 to 250,000	93	33	35.5	50	53.8	10	10.7
50,000 to 100,000	215	83	38.6	116	54.0	16	7.4
25,000 to 50,000	439	166	38.2	233	53.6	40	9.2
10,000 to 25,000	1072	511	47.7	488	45.5	73	6.8
5,000 to 10,000	1112	686	61.7	378	34.0	48	4.3

Source: *The Municipal Year Book, 1968* (Washington, D.C.: International City Managers' Association), p. 54.

may reside anywhere in the city; and the mayor, who may live anywhere in the city, is elected separately. But each council seat is voted on by the city at large. Oakland is the only city in its (250,000–500,000) population group to “nominate by wards and at large, elect at large.”¹⁷

The City Council. The segment of the political environment closest to the mayor is the city council, of which he is a member. And the council is a critical element affecting the mayor's ability to exercise leadership. For if the mayor could persuade four councilmen to go along with him, he could direct the city manager and attorney to follow his policy leadership—or he could fire the city manager and hire a new man who would work toward his goals.

City council recruitment has not been closely linked to elections. “Appointment politics” has become a tradition in Oakland; of the present members of the council, 6 were originally appointed, and only 3 were elected. Table 2 shows that this pattern of council recruitment has been a popular one in the city.

Once on the council, incumbents tend to be able to survive subsequent elections with ease. In the past 16 years, no “appointed incumbent” has lost the ensuing election. Overall, in the period between the spring elections of 1953 through the spring elections of 1969, only a handful of incumbents have lost. Table 3 shows the enviable electoral record of Oakland's incumbents.

Official city council meetings take place on Tuesday evenings and Thursday mornings, but most decisions are made at special “work sessions” before the council meetings—in the mayor's office on Thursdays and over dinner on Tuesdays. Although citizens' groups and the press have raised pointed questions about the secrecy of such meetings, both the mayor and council have felt that they can be more honest

with each other at such sessions and also be shielded from pressure. The model of mayoral leadership specifies that the mayor must direct both the council and public groups, but the conflicts over secrecy have shown that it may be difficult to do both at the same time. For the mayor is well aware that closed meetings facilitate informal discussion and the forging of agreement among council members, but the nonpublic nature of those meetings makes it difficult to generate either interest in or support for city policies among Oakland citizens.

Responses to Conflict: The “Shrinking Violet” or “Retrenchment” Syndrome. On the whole, the social and political attitudes of the councilmen tend to be conservative; these men are distrustful of governmental action in social areas and disturbed by the growing militancy of black people. At times, the council seems to pretend that Oakland's problems do not exist. In July, 1968, the Coordinating Community Council (made up of 16 school-community councils) asked the city council to levy a 5 cent emergency tax to lease portable classrooms, so that 19 elementary schools could eliminate double sessions. The chairman of the council's Ways and Means Committee admitted that the city council had the right to levy such a tax, but he noted that “what constitutes an emergency is a matter of opinion.” He pointed out that, historically, an emergency in the schools had meant a “fire or catastrophe.” Overcrowding and double sessions were not in that category, and the council did not accede to the request.¹⁸

If there is one thing that Oakland city councilmen do not like, it is conflict. And they have become adept at conflict avoidance, a process which is discussed in the literature of organization theory. Anthony Downs describes the “shrinking violet syndrome,” in which a bureau avoids conflict by narrowing its actions and af-

¹⁷ *Municipal Year Book, 1968*, p. 59.

¹⁸ *The Montclairion* (Oakland neighborhood newspaper), July 24, 1968.

fecting fewer external agents.¹⁹ And Matthew Holden, Jr., in an article on "Imperialism' in Bureaucracy," states:

Perhaps the most neglected cases are those in which there is a clear disposition toward retrenchment or self-limitation. It is not merely that the agency adopts kid-glove tactics in order to maintain a cooperative relationship with a constituency, but that it actually denies its powers.²⁰

In order to avoid the conflict, uncertainty, and turbulence of a hostile urban environment, the Oakland City Council goes beyond merely narrowing its actions and denying its powers. Indeed, the council follows a consistent policy of actively giving up jurisdiction in areas which it considers troublesome. After turning down the mayor's proposal to appoint an ombudsman for the city in 1967, the council hit upon a painless way to solve the problem of citizens' grievances. For the low price of \$10,000, the city was able to enter into an agreement with Alameda County whereby the county's (powerless) Human Relations Commission would add another member to its staff—with that member concentrating his efforts in Oakland. The city government was able to avoid responsibility for setting up grievance procedures; restrictions on the county commission's activity made it unlikely that any administrator would be embarrassed or threatened; and the cost to the city was only \$10,000.

Avoidance of conflict should not be confused with passivity or openness to mayoral initiative. For some mayoral initiatives—for example, a move for greater control of the poverty program—would mean more conflict for council members. Furthermore, the council tends to resent any special privileges available to the mayor; council criticism of the mayor's staff

Table 2. Prevalence of Appointment Politics in Oakland, 1953-1968

Office	Number of Changes	Originally Appointed	Originally Elected
Mayor	2	1	1
Councilman	17	9	8

Source: Oakland Citizens for Responsive Government, "Effect of Appointments Upon the Oakland Governmental Structure" (pamphlet published in June, 1969).

¹⁹ Anthony Downs, *Inside Bureaucracy* (Boston: Little, Brown and Company, 1967), p. 217.

²⁰ Matthew Holden, Jr., "Imperialism' in Bureaucracy," *American Political Science Review*, 60 (December, 1966), 945.

Table 3. Success of Incumbents in Oakland Politics, 1953-1969

Office	Number of Incumbents' Campaigns	Number of Incumbents' Victories	Number of Incumbents' Defeats
Mayor	6	5	1
Councilman	34	29*	5

* 12 of these were unopposed.

Source: Oakland Citizens for Responsive Government.

has been common.

Even the mayor's own office in City Hall is not necessarily friendly terrain. When Mayor Reading discovered in the fall of 1967 that his receptionist was openly critical of him and his policies, the mayor decided to transfer her to another department. But the city manager, fearing the wrath of the employees' association, advised caution, and civil service moved slowly in arranging the transfer. It was not until the spring of 1969—a year and a half later—that the change was made.

Such an environment would test the mettle of any mayor. But what manner of man is John H. Reading? The next sections will examine Reading's personality and his use of resources. By comparing Reading's actions with those of his predecessor, John C. Houlihan, we can suggest how much difference personality makes in the Oakland mayor's office.

The Mayor Himself

John Reading's life has been marked by a number of significant successes. As an air force pilot in World War II, he rose to the rank of Lt. Colonel. As a businessman, he has greatly expanded his family's food processing business and has become one of the leading manufacturers of tamales in the country.

In 1961, Reading was appointed to the city council. Mayor Houlihan had noticed Reading's attendance at, and participation in, meetings concerned with various neighborhood and civic issues. Feeling that Reading's comments were sensible and that his enthusiasm for "civic involvement" was strong, Houlihan asked Reading if he would like to join the city council. Reading was reluctant at first, but he felt that his advocacy of civic involvement meant that he should take the responsibility. (The appointment of a man who was not active in party politics or political groups, but just an "interested citizen," is indicative of the nonpolitical, nonactive-group nature of the Oakland system.) In the spring of 1966, when Mayor

Houlihan resigned in the wake of an embezzlement scandal arising out of his law practice, Reading was chosen by the council to be the new mayor. He was overwhelmingly reelected in 1967 and 1969.

A Private Man in a Public Office. Discussing the "purist" style in American politics, Nelson Polsby and Aaron Wildavsky state that such a style "represents a virtually complete privatization of politics" in which

the private conscience of the leader—rather than his public responsibilities—becomes the focal point of politics. Internal criteria—possession of, devotion to, and standing up for private principles—become the standards of political judgment. Constituents disappear, and we are left with a political leader determining policy on the basis of compatibility with his private principles.²¹

In John Reading's view standards of conduct which are applicable to man's behavior in private life, embodying values of truthfulness, etiquette, and respect, should be applicable to the public sphere. If absolute openness is desirable in private life, then it is also desirable in politics. When a black community leader agreed with Mayor Reading on certain issues in private and then attacked the mayor in a public meeting before a black constituency, the mayor felt constrained to remind the community leader—publicly—of all he had said in private meetings. After all, reasoned the mayor, there should be no dividing line between private and public.

Mayor Reading finds himself facing a fundamental dilemma: He is a political leader who does not like politics. Talking about his experience at the University of California and afterward, the mayor once remarked: "I was never interested in political science, or any aspect of the political arena at all. But I am interested in business and modern management techniques—you know, span of control and that kind of thing. I wanted to put business management in practice in government." But the going has not been easy; Reading confessed that "as mayor, I've found that there isn't enough information or control to run government like a business. All we can do is just muddle through."²²

The Consequences of a Businessman's Approach to Politics. Mayor Reading's attempt to bring

private and business standards into politics has had a number of consequences for his behavior as mayor. First of all, this approach has led him to try to simplify politics, to make governmental action clearer and more rational. He has consistently argued for the formation of "super-agencies" within the city government, which could "coordinate" the tasks of existing departments. Reading talks about coordination as a technical exercise; a more public mayor might recognize that, when different organizations have different preferences, "coordination" between them might mean that one wins and the other loses.

A second consequence of the mayor's private-business approach is his tendency to define public problems in economic terms:

I am convinced that employment is a factor of economics much more than of civil rights. Granted there may be areas of discrimination directly or inadvertently and we should continue breaking barriers, urging realistic employment standards and emphasizing full opportunity. But in my experience many of the unemployed are just not adequately prepared to take advantage of those opportunities.²³

In the mayor's view, education and employment, not political organizing, are the keys to ending poverty.

Another outgrowth of the mayor's private attitude to politics is his tendency to deal with political problems in a moralistic way. People who take nonmoral social actions should be punished for them. When City Manager Keithley discussed the possibility in 1969 of levying a bar drinker's tax to help pay the costs of additional policemen, the mayor responded: "That's only fair. This links the issues nicely! People who drink at bars should bear the costs for protection from crime."

On law and order the mayor is uncompromising. Responding harshly to demands for decentralization and community control of the Oakland Police Department, Mayor Reading declared:

Some persons . . . talk of harassment and brutality by policemen who are sworn to defend and protect this community with their lives. They call for creation of locally elected police control commissions which would be empowered to patrol and control the police. What they really are asking is the elimination of the police, for the proponents of this plan obviously advocate anarchy.²⁴

Although the mayor often defends the status quo, it should not be thought that he is insensi-

²¹ Nelson W. Polsby and Aaron Wildavsky, *Presidential Elections* (New York: Charles Scribner's Sons, 1968), p. 180.

²² Informal remarks at meeting of San Francisco Bay Area Chapter of the American Society for Public Administration, Oakland, October 5, 1967.

²³ Remarks before U.S. Civil Rights Commission, May 6, 1967.

²⁴ Remarks at Oakland City Council Meeting, April 23, 1968.

tive to individuals' grievances against the government. In fact, his private-business orientation has led him to be suspicious of unrestrained governmental power. In an effort to protect individual rights, he has fought—unsuccessfully—for an ombudsman plan in Oakland.

Responses to Conflict. Like the city council, Mayor Reading dislikes conflict. "I'm thankful that there's no dissent on our council. When you look at Berkeley, there are enormous divisions." The question that naturally arises is why a man who operates so successfully in the competitive world of business finds it difficult to deal with conflict in politics. A possible explanation is that Reading's success in his food business has been caused in great part by his discovery of a method of manufacturing tamales that was significantly cheaper than his competitors'. Thus, he won a technological competition, not a conflictful interpersonal one.

Recognizing that differences of opinion do exist in public life, Mayor Reading tries to deal with them by using what might be called the "big round table" method. He believes that if people could just get together on a personal level, around a table, and talk over their differences, then everything could be settled. But a "public" mayor, one who wishes to exercise leadership in accordance with the model outlined in the first section, might have much to gain from a different view: a recognition of the fact that conflicts exist and may be taken advantage of by leaders. McFarland²⁵ views leadership in a pluralist system as an individual's response to multilateral conflict. When conflict increases, set expectations break down and the environment may be manipulated by the leader. A mayor may serve as mediator between conflicting groups; he may seek to "upgrade the common interest" (as Mayor Lee did in New Haven by creating an urban renewal program that unified otherwise conflicting interests); or he may take advantage of a conflict situation to support the party with whom he agrees. All of these strategies are impossible if the mayor does not recognize, face, and deal with conflict situations.

The problem of current city politics, according to Mayor Reading, is that certain groups—most notably black militants—will not agree to set aside their own selfish interests and work for the community interest. And the losers are inevitably the "large, inactive majority . . . who would like to see real, lasting, and reasonable solutions to our problems." These people "go about their business, raise their family, take

their tax lumps" and are currently "taking a back seat" to the militants in public dialogue.²⁶ The mayor has particular sympathy for Oakland's old people, who are living on fixed incomes and worried by crime.

Confrontation politics, with threats, demands, and insults—which Reading felt as personal attacks—took a tremendous toll on the mayor during disputes about the poverty program, police, and model cities in 1968. Depressed, he tentatively decided not to seek reelection in 1969. But the passage of a new charter, the election of fellow-Republican Richard Nixon to the Presidency, and, perhaps most important, "a look at the alternative candidates" convinced the mayor to run again. Civic responsibility had once again overcome personal predilection.²⁷

Now we must ask what Reading can do as mayor, and what a different kind of man might do in this difficult governmental and political structure.

Resources

As we have seen, students of mayoral leadership have focused on the wise use and "pyramiding" of resources as ways in which an urban political leader can gain power. Dahl notes that

Although the kinds and amounts of resources available to political man are always limited and at any given moment fixed, they are not . . . permanently fixed as to either kind or amount. Political man can use his resources to gain influence, and he can then use his influence to gain more resources. Political resources can be pyramided in much the same way that a man who starts out in business sometimes pyramids a small investment into a large corporate empire.²⁸

Oakland seems to offer few opportunities for a mayor to pyramid political holdings. But a mayor of Oakland does have some resources, or at least potential resources. They are:

²⁵ Mayor's Inaugural Address, July 1, 1969.

²⁷ In an essay entitled "Political Incentives," James L. Payne and Oliver Woshinsky discuss, among other types, politicians who are motivated by an "obligation" incentive. Such people enter politics because they feel a duty to do so, and they are preoccupied with upright conduct in accord with fixed principles. Like Mayor Reading, they do not enjoy politics and have a great distaste for compromise. (Unpublished essay, April 1970). James David Barber describes a similar type—he calls them "Reluctants"—in his study of Connecticut legislators. The author found that many Reluctants entered politics because of a sense of duty, though they had a distaste for conflict and for politics. See Barber, *The Lawmakers* (New Haven: Yale University Press, 1965), pp. 130–154.

²⁸ Dahl, *Who Governs?* p. 227.

²⁶ McFarland, *Power and Leadership* . . . , pp. 177 ff.

(1) *The mayor's position as president of the city council:* As mayor, Reading presides over public council meetings and private "work sessions." He also appoints council committees. Thus, he has an opportunity to exert a strong influence on the subjects that come up before the council and the ways in which they are discussed and decided.

Former Mayor Houlihan—an outspoken, gregarious lawyer who loves politics—enjoyed this power and exploited it. A careful counter of council votes, he set about to secure outcomes favorable to himself. Houlihan often dispensed favors to friendly councilmen; for example, he gave certain members of the council the opportunity to borrow the mayor's limousine. Once, in order to please a councilman whose chief interest lay in an airport golf course, the mayor created a Golf Course Committee and made the councilman its chairman and only member.

Unlike City Manager Keithley, who uses informal council gatherings to strengthen his position with that body, Mayor Reading avoids council dinners and trips with councilmen to ball games. He appoints council committees on the basis of interest and expertise, and he makes little attempt to influence their deliberations. The mayor is generous in his regulation of the council agenda. At a 1969 work session, Reading told the council that "committees should speak up when they have a report. I've heard complaints that I haven't notified committees that they may give reports, but it's your initiative, and any time you have a report, just speak up." In sharp contrast, Mayor Houlihan's practice was to give councilmen specific assignments and call upon them for reports.

Further complicating the mayor's task in leading the council is his unwillingness to count votes and to persuade councilmen to see things his way. That would be "pressuring," an activity the mayor does not regard as legitimate. Though Mayor Reading enjoys considerable respect on the part of the council, his failure to influence committee work, control the agenda, and employ persuasion have led him to an underutilization of the political resource of his city council leadership.

(2) *Power of appointment:* The mayor appoints (with the approval of a majority of the council) members of city boards and commissions—including the Housing Authority, Redevelopment Agency, and Port. But Reading has often appointed people he likes and admires personally—only to find out after their appointment that they do not share his policy preferences. And the mayor's unwillingness to "pres-

sure" the city council has also restricted his influence in the appointment process. In July of 1968, the council refused to approve the mayor's reappointment of two members of the Planning Commission—one of them the chairman. At the same time, the mayor's move to replace an anti-Reading councilman's business associate as head of the Housing Authority failed when the man simply refused to leave his post and was backed up by some councilmen. In neither of these cases was Mayor Reading willing to fight on the council or in the newspapers for his appointments.

(3) *Prestige:* Describing the success of Mayor Lee of New Haven, Dahl shows how the mayor was able to use his personal popularity and prestige to extract concessions from political and business leaders.²⁹ Richard Neustadt says that "popular prestige" in the form of "public standing" is a source of influence for a President in his dealings with people in Washington. This is because political actors depend on the public for votes and support.³⁰

Mayor Reading has had an enviable electoral record—amassing more than 80 per cent of the vote in 1967 and more than 63 per cent in 1969. The prestige that results from electoral success in the city's highest elective office might be turned into an important resource; Mayor Lee was certainly able to capitalize on his success at the polls. But in Oakland, the lack of interest in electoral politics renders this resource much less useful. Mayor Reading has been able to use the prestige of his office in certain instances: in organizing business leaders into a campaign for a school tax, in promoting support for a downtown hotel and convention center, and in leading the campaign for a new charter. But on the whole, the mayor has tended to underutilize this potential resource. For the existence of prestige without the willingness to use the next named resource, publicity, may not be sufficient to accomplish mayoral goals.

(4) *Publicity:* In a discussion of protest groups, Michael Lipsky has demonstrated how important publicity is to "low resource" political actors:

The communications media are extremely powerful in city politics. In granting or withholding publicity, in determining what information most people will have on most issues, and what alternatives they will consider in response to issues, the media truly, as Norton Long has put it, "set . . . the civic agenda." Like the tree falling unheard in the forest,

²⁹ *Who Governs?* p. 309.

³⁰ Richard Neustadt, *Presidential Power* (New York: John Wiley and Sons, Inc., 1960), pp. 86 ff.

there is no protest unless protest is perceived and projected.³¹

Like Lipsky's protest groups, Mayor Reading lacks adequate money, staff, and organizational foundation. But he has certain important advantages: widespread respect, the forum of city hall, and a friendly newspaper in the *Oakland Tribune*.

These advantages are not useful unless capitalized upon, and the mayor's essentially private nature and preference for quiet, rational discussion make him less than eager to take his case on public matters to the people through the news media. In May 1969, when the school board disregarded its own biracial citizens' screening committee in selecting a new superintendent (who turned out to be involved in a conflict-of-interest case elsewhere), the community was in an uproar. Sensing public outrage, the mayor sought to persuade the board to cancel an upcoming school tax election and to initiate immediate negotiations with community groups to try to rectify the situation. Although he was just one vote short, in a secret meeting, of influencing the school board to take his advice, the mayor adamantly refused to put the board on the spot by making any sort of public statement. The tax election ended in overwhelming defeat, and the mayor lost a golden opportunity to appear as a successful mediator.

Publicity is a resource which is not severely limited by considerations of formal structure and jurisdiction, and an enterprising mayor can make good use of the media. Mayor Houlihan had a 10-minute weekly radio program, through which he told listeners about his activities and current governmental issues in Oakland. When the city council refused to endorse a bond issue for Bay Area Rapid Transit, Houlihan came out publicly for the issue anyway. The bond campaign was successful in a close election; a switch of 2,000 votes in Oakland would have defeated the issue. In this resource area, a "public" personality can make a difference.

Underutilization and Dispersion of Resources. Operating in a low resource environment, Mayor Reading has tended to underutilize the political resources he has. For example, the mayor's failure to use persuasion has caused him to gain less than full advantage from his role as president of the council. Instead of pyramiding re-

sources, he often appears to be engaged in *resource dispersion*, failing to combine his resources with each other. Thus, his power of appointment has been blocked by his failure to lead the council and to use publicity; and the mayor's personal prestige has been underutilized because of his unwillingness to take issues to the public. Except for occasional pronouncements on law and order, the mayor's private style has led him to avoid the public spotlight. For a low resource political actor who enjoys considerable personal respect, such avoidance seems unwise.

Personal Opportunities in Spite of Structure.

Although governmental and political structures set limits on what any mayor can do, personality can make a difference. On the basis of Mayor Houlihan's experience and studies of successful leadership elsewhere, we can suggest some of the ways in which a more politically oriented mayor might use his leadership resources in Oakland. In *The Washington Community, 1800-1828*,³² James S. Young describes a situation in the nation's capital at that time which appears remarkably similar to that in Oakland today: a lack of active groups; fragmented social subgroups formed around separate branches of government, with few integrating forces; weak congressional parties; an anti-political ideology, even among politicians; and an absence of presidential staff. Here, according to Young, the personality of the President made a difference. For "the President had to improvise out of wit and ingenuity, as his political talents, circumstances, statutes and good fortune permitted."³³ During the period studied, only Jefferson was successful enough at "statecraft" to be able to provide leadership. He utilized a combination of "social lobbying" (mainly intimate White House dinners), personal persuasion, and the newspaper forum provided by a publisher who was a friend of the President's.

A mayor of Oakland can also use "social lobbying" and personal persuasion as techniques to influence councilmen to follow his leadership. Mayor Houlihan, as we have seen, delighted in providing special favors for friendly councilmen, and City Manager Keithley uses dinners and sports events to good advantage. Social lobbying is not an activity Mayor Reading enjoys, and his distaste for "pressuring" councilmen leads him to avoid attempts at persuasion. But if the mayor did control a majority on the council, he could then direct the work of

³¹ Michael Lipsky, "Protest as a Political Resource," *American Political Science Review*, 62 (December, 1968), 1151.

³² New York: Columbia University Press, 1966.

³³ Young, p. 159.

the city manager and attorney. When Mayor Houlihan was involved in drawing up a contract for Oakland participation in building the Coliseum Complex, he had the city attorney deliver legal opinions which supported the mayor's preferences.

Just as Jefferson used a friendly newspaper to present his program, so Houlihan used a radio program to publicize his point of view. When he could not get the council to go along with him—as in supporting rapid transit bonds—he took his case to the people. And when city resources are inadequate, a mayor can try to persuade outside bodies to help. Thus, Mayor Houlihan was able to convince the Alameda County Supervisors to grant \$350,000 from county highway funds to build a tunnel under the new Oakland museum.

Faced with a lack of groups in the political environment, a mayor can act to stimulate the creation of groups. When Mills College trustees informed Mayor Houlihan that construction of the potentially noisy MacArthur Freeway would force the college out of Oakland, the mayor decided to forestall the college's move by seeking a ban on truck traffic on that freeway. He formed a citizens' committee to support such a ban and joined them in pressing the case in the state capital. The state government decided to ban trucks; Mills stayed in Oakland.

Although mayors in Oakland are limited both by governmental structure and political disorganization, there are still opportunities for an inventive political man to make the most of the situation—by social lobbying, persuasion, publicity, and the creation of groups which will support him. But, as James Young has noted, all this requires adroitness, ingenuity, and political expertise. Even given Oakland's constraining structure, personality can make a difference.

American cities are unlikely to find their revenue constraints greatly eased in the near future, and the jurisdictional fragmentation about which so many mayors complain is not likely to disappear. Thus, the techniques of low resource leadership described here appear to have wide applicability.

Conclusion: Mayoral Leadership and the Political System

Although calls for "mayoral leadership" fill the literature on city politics, an understanding of such leadership in any given city requires an examination of the political environment, the personalities involved, and the ways in which the mayor uses the resources available to him.

It is clear that the leadership exercised by

Mayor John Reading of Oakland does not closely approximate the model of leadership described in Section I—the model of a mayor who sets goals and then uses a wide variety of means to influence others to act according to the mayor's own preferences. This is due both to environmental and personal factors.

The introductory section listed seven environmental requirements for effective mayoral leadership, and it is obvious that the Oakland governmental and political structure provides little comfort for any mayor. Like most cities, Oakland faces a revenue strain and rising personnel costs—which means that it does not have (a) *sufficient financial resources* with which a mayor can launch innovative social programs. Nor does the city have (b) *jurisdiction* in the vital program areas of education, housing, redevelopment, and job training. Under the council-manager form of government, there are severe limits to (c) *mayoral jurisdiction within the city government*. And the mayor had neither (d) *sufficient salary* (\$7500 per year) nor (e) *adequate staff* (one administrative assistant and three secretaries) to perform as a full time and well-informed official.

Although Oakland provides the mayor with (f) *a ready vehicle of publicity* in the *Oakland Tribune*, the rest of the political environment offers him little comfort. There are few (g) *politically-oriented groups* which can provide support for a mayoral program.

Without governmental jurisdiction, staff, and financial resources, it is hard for any mayor to direct, or even influence, the actions of others. But it is not true that mayoral leadership is impossible in Oakland. For the mayor does have certain resources (position as presiding officer; power of appointment; prestige; legitimacy), and an adept political man can pyramid those resources. John Houlihan, a man who enjoyed the turbulence of the public arena, was able to build on his meager mayoral resources. But John Reading, a private man with a distaste for conflict and a preference for private discussion, has consistently underutilized his political resources. In Oakland, a mayor's lot is not a happy one, but it is not a hopeless one, either.

As in other cities, the mayor of Oakland is in large measure the product of the political system. The nature of that system has a significant influence on the kinds of people who become mayor and the forms of leadership they can exercise. In the nongroup, nonpartisan, and non-electoral political system of Oakland, it is entirely possible for a "nonpolitical" civic reformer like John Reading to attain the position of mayor without really trying for it. No

groups, parties, or politicians could make an effective claim on the office. Even John Houlihan, who stands out among recent Oakland mayors as an ambitious politician, was promoted for the office by a loose coalition of citizens' groups representing many political views. Although Houlihan sought to be a full time mayor, the low salary forced him into deep financial trouble and hence led to his eventual downfall. Thus, the Oakland governmental system may be said to have penalized him for trying to exert sustained leadership.

In a governmental system in which a mayor lacks administrative authority, the leadership most often possible is hortatory in nature (encouraging voters to pass a bond issue, charter reform, etc.). And in a political system which is relatively amorphous, such leadership can also run the risk of being unconnected to public opinion. The lack of organized groups and parties deprives the mayor not only of potential support; it deprives him of information as well. As a result, mayoral leadership can be sharply divorced from the policy preferences of relevant constituencies. In the spring of 1970, when Mayor Reading suggested that National Guard troops be called to operate buses in the midst of a tension-filled transit strike, his proposal was sharply criticized by *both* labor and management.

It is important to recognize that hortatory leadership need not necessarily be an isolated public speaking exercise. If a political system contains active groups to whom a mayor can appeal and from whom he can gain information, then the mayor can urge those groups to help him in influencing others. Thus, the extent to which public exhortation can be used to supplement meager financial and jurisdictional resources will depend in part on the structuring of the mayor's audience.

Election campaigns can provide political actors an opportunity to appeal to the public. But if electoral competition is nonexistent, there is no incentive for groups of councilmen to join together in framing a program. If no boats are rocked, then no competition may surface. Contested elections, which can have the effect of defining alternatives and building support for those alternatives, might provide both a start and a direction for the victors.

Of course, a lack of leadership in the mayor's office does not mean that no leadership is being exerted in the urban political system. In Oakland, the city manager has certainly been a leader within the city bureaucracy. And the weakness of elected officials has doubtless helped Oakland's ghetto residents to increase

their control over federal poverty programs and model cities programs.³⁴ But leadership exerted by elected officials differs from that of city managers and poverty program administrators in being potentially subject to popular review at regular intervals. Although the absence of groups, parties, and electoral competition can severely limit the effectiveness of that review and dilute the influence of citizens on the government, the potential for popular control is not trivial. For elections can provide a vehicle for mobilizing the resource of numbers against other resources such as money or social standing or control of communications media. The lack of power on the part of elected officials may lead to greater power for those who presently hold these other resources.

A city manager or program administrator may be responsive to citizens' preferences because he deems it to be a wise policy to act in such a manner. But under conditions of electoral competition, an elected official has a more direct incentive to pay attention to public concerns. Furthermore, there is evidence that Americans *expect* their local elected leaders to be more responsive than nonelected administrators are. As Almond and Verba have shown, Americans—unlike Germans, Italians, or Mexicans—are much more likely to contact elected political leaders than administrators if they wish to influence their local government.³⁵

Because of their visibility, and the expectation that they might stimulate and respond to the concerns of urban constituencies, mayors have been a natural focus of those who study political leadership in cities. Mayors who can exert leadership by successfully pyramiding political resources are to be admired, but the ability to pyramid resources is by no means universal. Furthermore, it is often the case that people—even those in public office—try to avoid the exercise of power. Oakland officials are not unique in their avoidance behavior. Young's study of the early Washington community shows a group of men at the pinnacle of national politics who had a strong distaste for power.³⁶ And William K. Muir has pointed out that policemen, reacting to their hostile environment and the potential costs of decisive action, often devise ways of escaping the exercise

³⁴ See Judith V. May, "Two Model Cities: Political Development on the Local Level" (Paper delivered at annual meeting of the American Political Science Association, New York, 1969).

³⁵ Gabriel Almond and Sidney Verba. *The Civic Culture* (Boston: Little, Brown and Company, 1965), p. 148.

³⁶ Young, *The Washington Community*.

Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the Imbalance Thesis

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A number of analysts have pointed to imbalance between political and administrative institutions as a basic feature of politics in underdeveloped systems. Central to this idea of political-administrative imbalance are the observations that:

First, in a large number of new states, representative institutional structures . . . are characterized by extreme weakness and fragility. Second, the bureaucratic sector, possessing high functional capability and high relevance to economic development, has gained an overwhelming degree of influence over government policy.¹

According to the institutional imbalance literature, lack of correspondence between political and administrative development is anything but coincidental—for a developed bureaucratic system is itself thought to inhibit political modernization; accordingly, the imbalance argument tends to focus on the politically pathological consequences of modern bureaucracy. The purpose of this research note is to advance understanding of the relationship between administrative and political development by more closely examining a key premise of the institutional imbalance argument.

The Hypothesis

According to the imbalance thesis, the very presence of modern bureaucracy in "Third World" nations presents a formidable obstacle to political development. The tendency of administrative modernity to outstrip political development is considered to be so critical that Lucian Pye terms it "the great problem today in nation-building."² What makes this situation so crucial, it is held, is modern bureaucracy's tendency to become overpoliticized—a process that is reflected in its capacity to attract the most able political leaders, undermine party systems, overawe legislatures, curtail popular participation, exploit court systems, and capture interest groups.³ It is in this sense that Fer-

rel Heady terms the political systems of these nations "askew," so weak are their representative structures in the face of modern administrative systems.⁴ In short, as Fred Riggs argues, the vitality of bureaucracy relative to other political and governmental structures makes for a situation in which ". . . the political function tends to be appropriated, in considerable measure, by bureaucrats."⁵

In general, then, the chief components of the argument that bureaucratic development hinders the prospects of political development are the propositions that: (1) the relatively high level of bureaucratic development in new nations fosters overparticipation by the bureaucracy in the performance of governmental and political functions; and (2) such bureaucratic overparticipation stunts the growth of viable representative institutions.

In the absence of data by which the second proposition might be tested convincingly, the politically deleterious effects of bureaucratic overparticipation will not be questioned here. The intent of this note is to examine the contention—central in the imbalance argument—that bureaucratic overparticipation is the product of modern bureaucracy. More precisely, we shall investigate the hypothesis that: *In politically underdeveloped nations, there is a positive correlation between (1) level of bureaucratic development and (2) extent of bureaucratic overparticipation in governmental and political functions.* Confirmation of this hypothesis would imply that the institutional imbalance argument is essentially correct; rejection would suggest that the argument should be modified or abandoned.

Procedures

As students of administration are all too keenly aware, even the most basic cross-national data on bureaucracy are generally unavailable. Given this difficulty, systematic hypothesis testing remains a relative rarity in

¹Michael F. Lofchie, "Representative Government, Bureaucracy, and Political Development: The African Case," *Journal of Developing Areas*, 2 (October, 1967), pp. 39-40.

²Lucian W. Pye, *Aspects of Political Development* (Boston: Little, Brown, 1966), p. 19.

³Fred W. Riggs, "Bureaucrats and Political Development: A Paradoxical View," in *Bureaucracy and Political Development*, ed. Joseph LaPalombara

(Princeton: Princeton University Press, 1963), pp. 120-167.

⁴Ferrel Heady, *Public Administration: A Comparative Perspective* (Englewood Cliffs: Prentice-Hall, 1966), pp. 64-65.

⁵Riggs, "Bureaucrats and Political Development," p. 120.

Table 1. Bureaucratic Development and Political Overparticipation

		BUREAUCRATIC DEVELOPMENT	
		+	-
PARTICIPATION IN POLITICAL FUNCTIONS	Not Over- participant	<i>24 Nations</i> Argentina Honduras Bolivia India Brazil Lebanon Ceylon Liberia Chile Libya Colombia Malaya Cuba Morocco Dominican Republic Nicaragua Ecuador Panama El Salvador Paraguay Guatemala Peru Haiti Venezuela	<i>8 Nations</i> Afghanistan Algeria Burma Cambodia Jordan Laos Saudi Arabia Thailand
	Over- participant	<i>6 Nations</i> Ghana Guinea Mexico Turkey Union of South Africa United Arab Republic	<i>19 Nations</i> Cameroun Niger C.A.R. Nigeria Congo Sierra Leone Dahomey Somalia Ethiopia Sudan Gabon Tanganyika Iran Togo Ivory Coast Uganda Mali Upper Volta Mauritania

$$\chi^2 = 14.8 \text{ d.f.} = 1$$

$$p < .001$$

comparative administration; significant propositions more often seem to be accepted on faith than on convincing evidence. Fortunately, the key concepts of our hypothesis can be operationalized in terms of presently available data.

Bureaucratic Overparticipation. In order to measure bureaucratic overparticipation, one must answer both empirical (How much?) and evaluative (Too much?) questions. In the concluding chapter of *The Politics of the Developing Areas*, James Coleman designed a model system in which political and governmental functions are performed by specific structures:

... rule-making primarily by parliaments and secondarily by executives; rule application by bureaucracies functioning under political executives; rule adjudication by an independent judiciary; interest articulation by associational interest groups and/or competing parties; interest aggregation by competing parties and/or parliaments; political socialization and recruitment by system-wide secondary structures that penetrate primary structures; and political communication by autonomous and differentiated media of communication.⁶

⁶James S. Coleman, "Conclusion: The Political Systems of the Developing Areas," in *The Politics of the Developing Areas*, ed. Gabriel Almond and

The volume's five area experts (Coleman, Lucian Pye, Myron Weiner, Dankwart Rustow, and George Blanksten) evaluated political systems in light of Coleman's model, noting instances in which specific structures deviated from the model by overparticipating in the performance of governmental or political functions. In all, seventy-six nations in Latin America, Asia, and Africa were evaluated. The two bureaucratic overparticipation variables employed here are taken directly from the judgments of these area specialists.

Bureaucratic Development. There is little agreement among experts as to what the concept of "bureaucratic development" entails. Equally pressing is the lack of hard data needed to operationalize the concept, however it is defined. But although formidable, these difficulties are not insurmountable. The Banks and Textor variable "Character of Bureaucracy," while less than an ideal measure, is an often-employed index of bureaucratic development.⁷ This variable sep-

James Coleman (Princeton: Princeton University Press, 1960), pp. 559-560.

⁷For such a use of this variable, see, e.g., Gary L. Buck and Alvin L. Jacobson, "Social Evolution

Table 2. Bureaucratic Development and Governmental Overparticipation

		BUREAUCRATIC DEVELOPMENT	
		+	-
PARTICIPATION IN GOVERNMENTAL FUNCTIONS	Not Over-participant	20 Nations Bolivia Ceylon Chile Colombia Cuba Dominican Republic Ecuador Guatemala Haiti Honduras India Lebanon Liberia Libya Malaya Nicaragua Panama Paraguay Turkey Venezuela	5 Nations Afghanistan Algeria Cambodia Ethiopia Laos
	Over-participant	10 Nations Argentina Brazil El Salvador Ghana Guinea Mexico Morocco Peru Union of South Africa United Arab Republic	22 Nations Burma Cameroun C.A.R. Congo Dahomey Gabon Iran Ivory Coast Jordan Mali Mauritania Niger Nigeria Saudi Arabia Sierra Leone Somalia Sudan Tanganyika Thailand Togo Uganda Upper Volta

$\chi^2 = 13.2$ d.f. = 1
 $p < .001$

arates national bureaucracies into four categories; since none of the systems in the present analysis falls into the "modern" class, the relevant categories are:

- (1) *semi-modern*—"largely 'rationalized' bureaucratic structure of limited efficiency because of shortage of skilled personnel, inadequacy of recruitment or performance criteria, excessive intrusion by non-administrative organs, or partially non-congruent social institutions."
- (2) *post-colonial transitional*—"largely rationalized ex-colonial bureaucratic structure in process of personnel 'nationalization' and adaptation to servic-

ing or restructuring of autochthonous social institutions."

- (3) *traditional*—"largely non-rationalized bureaucratic structure performing in the context of an ascriptive or deferential stratification system."⁸

For purposes of analysis, Banks and Textor's procedure of dichotomizing this variable between the "semi-modern" and "post-colonial transitional" categories will be retained.

Politically Underdeveloped Nations. A procedure for gauging the degree of political development is necessitated by the fact that our hypothesis applies only to politically underdeveloped nations. On the basis of indices of stability, legitimacy, and participation, the author elsewhere devised an instrument for dichotomizing nations into categories of politically "developed" or "underdeveloped."⁹ Operation-

and Structural-Functional Analysis: An Empirical Test," *American Sociological Review*, 33 (June, 1968), 343-355; D. Stanley Eitzen, "The Use of Banks and Textor's 'A Cross-Polity Survey' for the Ranking of Nations: A Methodological Note," *Social and Economic Studies*, 16 (September, 1967), 326-329; John Forward, "Toward an Empirical Framework for Ecological Studies in Comparative Public Administration," in *Readings in Comparative Public Administration*, ed. Nimrod Raphaeli (Boston: Allyn and Bacon, 1967), pp. 450-472; and Andreas C. Tsantis, "Political Factors in Economic Development," *Comparative Politics*, 2 (October, 1969), 63-78.

⁸ Arthur S. Banks and Robert B. Textor, *A Cross-Polity Survey* (Cambridge: M.I.T. Press, 1963), pp. 112-113.

⁹ Lee Sigelman, "Modernization and the Political System: A Critique and Preliminary Empirical Analysis," *Sage Professional Papers in Comparative Politics*, Vol. 2, Series No. 01-016 (1971).

ally, then, formation of a sample of nations proceeded as follows: A list of nations common to the Coleman and Banks-Textor analyses was drawn up. From this list, five nations (Israel, Costa Rica, Philippines, Tunisia, and Uruguay) were eliminated because they were considered politically "developed." This left fifty-seven Latin American, Asian, and African nations for a pool of politically underdeveloped nations.

Findings and Implications

Tables 1 and 2 strongly confirm the hypothesis that bureaucratic development and overparticipation are interrelated in politically underdeveloped nations; the chi-square values for each matrix are significant beyond the .001 level.

If the imbalance argument is substantially correct, a disproportionate number of cases should be clustered in the lower-left and upper-right cells of each matrix. Inspection reveals, however, that *the cases are arranged in precisely the opposite pattern from that expected on the basis of the imbalance thesis*. The bulk of the relatively developed bureaucracies do *not* overparticipate: only six of the thirty developed bureaucracies overparticipate in political functions, only ten of thirty overparticipate in governmental functions. On the other hand, the vast majority of the underdeveloped bureaucracies *do* overparticipate: nineteen of the twenty-seven underdeveloped bureaucracies overparticipate in political functions, and fully twenty-two of twenty-seven in governmental functions. More generally, comparison of expected and obtained frequencies for each table reveals that the significant chi-square values are products of an *inverse* relationship between bureaucratic development and overparticipation.

In interpreting these findings, it should be

borne in mind that the data upon which they are based are judgmental in character. But if this analysis has any merit, it suggests that part of the accepted wisdom of comparative administration is badly in need of rethinking: no longer can it be argued on overparticipation grounds that bureaucratic development is politically dysfunctional. If modern bureaucracy is thought to hinder political development, then some alternative grounds for such hindrance must be specified. More positively, if bureaucratic overparticipation is conceded to hamper the chances of political development, the present findings suggest that modern bureaucracy may actually facilitate political development—for the relatively modern bureaucracies seldom overparticipate.¹⁰ This last point is directly in keeping with the view—for which the author has presented some preliminary evidence elsewhere¹¹—that the presence of a relatively modern national administrative system is a necessary precondition of, not a hindrance to, societal modernization, including political development.

¹⁰ Alternatively, according to a recent paper by Fred Riggs, "We may conclude . . . that a dominant bureaucracy will necessarily sacrifice administrative to political considerations, thereby impairing administrative performance." See Riggs's "Administrative Reform and Political Responsiveness: A Theory of Dynamic Balancing," *Sage Professional Papers in Comparative Politics*, Vol. 1, Series No. 01-010 (1970), p. 579. This interpretation, which is certainly plausible, reverses the causal sequence of the hypothesis tested here, and in the process renders Riggs virtually immune to disconfirmation by cross-sectional data; combining the hypothesis tested here with the later Riggs interpretation, bureaucratic overparticipation should be simultaneously correlated with both bureaucratic development and bureaucratic decay.

¹¹ Sigelman, "Modernization and the Political System . . ."

The Permanent Court of International Justice, the International Court of Justice, the League of Nations, and the United Nations: A Comparative Empirical Survey*

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The scholarly literature relevant to the PCIJ, ICJ, League and UN contains a number of complex dimensions.¹ Until recently, the literature has been primarily normative, concerned with prescriptive questions such as how international institutions might be designed to minimize violence. Both optimism and pessimism about the role of international institutions have been voiced during the past fifty years, with pessimism predominating over the long run.² Although the evaluative orientation continues to play a large role in the contemporary literature, the systematic development of concepts and testing of propositions has begun in recent years.³ Scholars have been increasingly inter-

ested in investigating systematically the actual as contrasted with the hoped-for role of international institutions, not because they consider prescriptive questions unimportant but because they wish to base their evaluative discussions on solid empirical grounds. In the long run, this methodological trend in the literature may dampen the optimism-pessimism variations so that more balanced theoretical positions can be developed.

These beginnings of empirical research have been noted by Alger⁴ and have become somewhat more apparent in recent issues of periodicals such as *International Organization*, but we still lack empirically tested or even testable theory about the role of international law and organization in the contemporary world. The purpose of this article is *not* to present empirically tested theory but rather to provide a mapping of some empirical relationships involving the four institutions. In other writings, both authors have applied some of the data presented in this article to a theoretical framework.⁵ In

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¹ For a general overview of the literature on international law, see William D. Coplin, "Current Studies of the Functions of International Law: Assessments and Suggestions," in *Political Science Annual*, II, ed. James A. Robinson (New York: Bobbs-Merrill, 1970), pp. 149-208. The literature on international organization is surveyed in Robert E. Riggs, Karen F. Hanson, Mary Heinz, Barry B. Hughes, and Thomas J. Volgy, "Behavioralism in the Study of the United Nations," *World Politics*, 22 (January, 1970), 197-237; and Chadwick F. Alger, "Research on Research: A Decade of Quantitative and Field Research on International Organizations," *International Organization*, 24 (Summer, 1970), 414-450.

² For example, compare F. P. Walter, *A History of the League of Nations* (New York: Oxford University Press, 1952) to Gerhard Niemeyer, "The Balance Sheet of the League Experience," *International Organization*, 6 (1952), 537-558; or, the various writers discussed in Inis L. Claude, Jr., *Power and International Relations* (New York: Random House, 1962); or, Morton A. Kaplan and Nicholas deB. Katzenbach, *The Political Foundations of International Law* (New York: John Wiley & Sons, 1961) to C. Wilfred Jenks, *The Common Law of Mankind* (New York: Praeger, 1958).

³ Many of the more systematic studies of the four institutions are discussed in Alger as well as Riggs et al. Those of particular relevance to this study, however, are Quincy Wright, *The Study of War*, 2nd ed. (Chicago: University of Chicago Press, 1965), pp. 1043-49; K. J. Holsti, "Resolving International Conflicts: A Taxonomy of Behavior and Some Figures on Procedure," *Journal of Conflict Resolution*, 10

(September, 1966), 272-297; William D. Coplin, "The World Court in the International Bargaining Process," in *The United Nations System and Its Functions*, ed. Robert W. Gregg and Michael Barkun (Princeton: D. Van Nostrand, 1968), pp. 317-333; and Ernst B. Haas, *Collective Security and the Future International System* (Denver: University of Denver, 1968). Most systematic studies of the World Court have focused on the question of influences on the voting behavior of the Judges. For example, see Thomas Hensley, "National Bias and the International Court of Justice," *Midwest Journal of Political Science*, 12 (November, 1968), 568-586. Systematic studies of the role of the United Nations in international conflict similar to the Haas study are beginning to appear. For example, see Thomas Hensley, "Conditions Related to the Conflict Management Activities of the United Nations," Diss. Iowa 1970; and James E. Harf, "Inter-Nation Conflict Resolution: Development and Test of a Causal Model," paper delivered at the annual meeting of the Midwest Political Science Association, 1971 (revised mimeo).

⁴ Alger, "Research on Research . . ."

⁵ The approach that examines the impact of international institutions on the international bargaining process is outlined in Coplin "Current Studies of the Functions of International Law," in Robinson, *Political Science Annual*, pp. 178-188. Rochester's dissertation (presently being completed), "The International System as a Prismatic Polity: Assessing Political Development Through a Comparative Em-

this article, we merely wish to present some empirical relationships that highlight similarities and differences among the four institutions that scholars might find useful in their empirical and theoretical pursuits. At a methodological level, we wish to demonstrate the usefulness of data collected across cases involving international institutions. Ultimately, we hope to contribute to empirical theory concerning the role of international organizations in bargaining relationships among states.

With those aims in mind, we first outline the basic dimensions of the data collected on the four international institutions. We then present selected findings and their theoretical implications, focusing primarily on whether structural differences (PCIJ and ICJ versus the League and UN) or historical differences (PCIJ and League versus ICJ and UN) account for the patterns in the data. A final section briefly discusses an analytical framework which helps organize the data presented in the article and which, more generally, maps suggested research strategies to be used in investigating the role of international institutions in the international bargaining process.

Description of the Data

The data used in this paper and described below includes all disputes (1) which occurred between 1920 and 1968, (2) which directly involved only two states, and (3) which were considered in at least one of the four institutions. Twenty-four cases were identified for the PCIJ, thirty-one for the ICJ, thirty-five for the League, and thirty-one for the UN.⁶ (See Appendix A-D)

pirical Analysis of the Role of the World Court, League of Nations and United Nations in Dyadic Disputes," employs the data in a systematic analysis of political development in the international system over the past fifty years. The analysis is based on a theoretical model constructed by synthesizing various developmental models found in the comparative politics literature, and represents an attempt to examine empirically some questions raised by (among others) Chadwick F. Alger in "Comparison of Intrnational and International Politics," *American Political Science Review*, 57 (June, 1963), 406-419.

⁶ With regard to the Court, only contentious disputes are included in this study; cases involving advisory opinions are omitted. Lists of cases can be found in Wright, *Study of War*, p. 1430; Haas, *Collective Security*, pp 46-47; and Holsti, "Resolving International Conflicts . . .," 293-296. More detailed but relatively brief information on the cases can be found in the *World Court Reports* and *The I.C.J. Reports* for the two Courts; Walter, *History of the League*, for the League; and *Synopses of United Nations Cases in the Field of Peace and Security, 1946-65* (New York: Carnegie Endowment

The decision to focus on two-party or dyadic disputes was made primarily for practical reasons, to avoid the complexity that would result if such cases as the series of Berlin crises or the Middle East confrontations were part of the data set. At the same time, the desire to compare the two Courts on the one hand and the League and UN on the other required the selection of two-party cases since dyadic cases outnumbered multilateral cases for the two Courts by fifty-five to five.

Table 1 lists the variables in each dispute which are used in this paper.⁷ The variables are listed in one of three classes: (1) those that focus on the characteristics of individual participants, such as the amount of participation or the level of economic development; (2) those that focus on the relationship between the states in the dyad, such as economic interdependence or political similarity; and (3) those that focus on the characteristics of the case. The specific definitions of each of these variables will be discussed in the course of the paper.

The Use of Statistics in the Present Analysis

The methodological rule of thumb applied by the authors throughout the paper is to employ the least complicated mode of presentation and statistical analysis possible, given the descriptive and theoretical purposes at hand. Comparison of the four institutions will be made by examining the frequency distributions on different variables for the four institutions. Tables containing these distributions will be

for International Peace, 1966), for the United Nations. Empirical data have been collected and analyzed by Ernst Haas and John Ruggie, University of California, Berkeley; Hayward R. Alker, Jr., Massachusetts Institute of Technology; James E. Harf, Ohio State University; and Thomas Hensley, Kent State University.

⁷ Space permits us to focus on only a few variables in this paper. Information on these selected variables as well as the complete list of variables employed in our larger data-gathering project is available from the authors upon request. Inquiries should be sent to William D. Coplin, International Relations Program, Syracuse University, Syracuse, New York 13210. The complete data set has been created in machine-readable form and can be obtained from the International Relations Archive of the Inter-University Consortium for Political Research. For those scholars who are also teachers, the data along with a discussion of how analysis in the study of international law and organization in the international bargaining process can be performed is available in inexpensive form. (*Learning Package #3: Dyadic [Two-Party] Disputes Before the World Courts, League and UN: Empirical Materials*. [Chicago: Markham Publishing Company, 1971].)

Table 1. Variables Employed in the Analysis of Two-Party Disputes

- | | |
|---|---|
| I. Participant Characteristics | |
| 1. Number of Participations Per State For Each Institution | a. One, b. Two, c. Three, d. Four, e. Five, f. Six, g. Over six |
| 2. Nature of the Political System | a. Closed, b. Semi-open, c. Open |
| 3. Economic Development | a. Underdeveloped, b. Intermediate, c. Developed |
| 4. Geocultural Region | a. Western Community, b. Latin America, c. Eastern Europe, d. Other |
| II. Relationships Between Participants | |
| 1. Political | a. Same, b. Different |
| 2. Economic | a. Same, b. Different |
| 3. Geocultural | a. Same, b. Different |
| 4. Military Distance | a. Direct allies, b. No military ties, c. Members of opposing alliances |
| 5. Economic Interdependence | a. Heavy, b. Moderate, c. None |
| 6. Geographical Proximity | a. Common border, b. One to five hundred miles between borders, c. Over five hundred miles between borders |
| 7. Relative GNP Rankings Between Participants in Unilaterally Initiated Cases | a. Same rank, b. Initiator bigger, c. Initiator smaller |
| III. Characteristics of the Case | |
| 1. Mode of Submission | a. Joint, b. Nonjoint |
| 2. Hostilities Involved | a. No hostilities, b. Hostilities |
| 3. Type of Issue | a. Contractual, b. Treatment of persons, c. Territorial, d. Threat of hostilities, e. Outbreak of hostilities |
| 4. Number of Months Between Submission of Dispute and Disposal by the Institution | a. One to twelve months, b. Over twelve months |
| 5. Significance of Action Taken by the Institution | a. Action with consequences, b. No action taken, or action without consequences |
| 6. Outcome of the Dispute | a. Settled by or with the help of the institution, b. Unsettled or settled outside the institution |

presented and discussed in the body of the paper.

For each variable discussed (e.g., type of submission), we are interested in determining: (1) the relative significance of historical (that

is, PCIJ/League vs. ICJ/UN) as opposed to structural (that is, Court vs. League/UN) factors in accounting for the frequency distribution pattern for the four institutions, and (2) the degree of difference between pairs of institutions (such as PCIJ vs. ICJ and League vs. UN). Statistical measures will be used to summarize the degree of difference between institutions on a given variable.⁸ Regarding the first concern, we will derive a summary measure of the relative impact of historical as opposed to structural factors on each variable by aggregating the frequency distributions for the four institutions in the following manner:

	Variable (Type of submission)	
	Joint	Nonjoint
PCIJ and League		
ICJ and UN		

(Matrix A)

	Variable (Type of submission)	
	Joint	Nonjoint
PCIJ and ICJ		
League and UN		

(Matrix B)

The PCIJ and League vs. ICJ and UN matrix tells us the impact of the *historical period* while the PCIJ and ICJ vs. League and UN matrix tells us the impact of *institutional structure*. An index produced by the formula

$$\frac{\text{Measure of Association for Matrix A}}{\text{Measure of Association for Matrix B}}$$

will indicate historical effects over structural effects such that a quotient greater than one

⁸ The variables on which data were gathered are either nominal or ordinal. Our statistic for dichotomous nominal variables is phi; for non-dichotomous nominal variables, lambda. Wilcoxon's signed-ranks test is used for the ordinal variables. See Linton C. Freeman, *Elementary Applied Statistics* (New York: John Wiley, 1965). All three of these statistics are commonly used as measures of association and are employed here as shorthand measures of the degree of difference between the distributions for the different institutions.

Table 2. Frequency Distribution of Participation in PCIJ, ICJ, League, and UN for Each State
(Percentage of States Using the Institutions)[†]

	Usage per State						
	1	2	3	4	5	6	More than 6
PCIJ							
*N=21	43	29	5	14	0	10	0
ICJ							
*N=32	72	13	3	3	0	3	6
League							
*N=29	39	25	18	7	0	11	0
UN							
*N=41	71	15	7	7	0	0	0

* Number of different states employing the institution.

[†] The usage per state variable was treated as an ordinal measure and the institutions as nominal categories. As a result, Wilcoxon's signed-ranks test was employed. The matrix values are:

	ICJ	League	
PCIJ	.27	.06	(Historical/Structural Index = .32/.02 = 16.0)
UN	.01	.36	

This indicates that historical period was far more important than the structural factor in affecting the differences between the institutions.

would suggest that historical period is more responsible for the frequency distribution pattern, and a quotient less than one would suggest that the pattern is more attributable to structural effects.

As for the second concern, statistical measures will be used to summarize the degree of difference on a given variable for each of the following pairs of institutions: PCIJ-ICJ, PCIJ-League, League-UN, and ICJ-UN. The summarizing statistics for these four comparisons on each variable will be presented in the following matrix format, with the value in cell A representing the summary measure for the PCIJ-ICJ comparison, the value in cell B representing the summary measure for the PCIJ-League comparison, etc.:

	ICJ	League
PCIJ	A	B
UN	C	D

All of the summarizing statistics (the Historical/Structural Index, as well as the summary measures of the four comparisons) appear as footnotes to the tables.

Participant Behavior and Characteristics

Because the state has traditionally been considered the primary actor in the international bargaining process, we find it plausible to begin our analysis of the role of the PCIJ, ICJ,

League and UN in the international bargaining process by studying the relationship between the characteristics of states and their behavior toward the four institutions when they are involved in two-party disputes. We will attempt to answer the following questions: (1) How frequently have states used these institutions? (2) In what ways are the institutions brought into the bargaining process in the first place? (3) How do domestic political characteristics, level of economic development, and geocultural attributes relate to the usage of the four institutions by states?

Frequency of Usage. In general, few states have made frequent use of any of the four institutions. Table 2 indicates that with two exceptions, no state used any of the institutions more than six times, and the majority of states used the institutions less than three times. One major difference in the pattern of usage between the PCIJ and League on the one hand, and the ICJ and UN on the other is that a larger number of states have used the post-World War II institutions only once.

One interpretation of this finding is that there appears to be a stronger negative learning effect in the postwar than the interwar period.⁹

⁹ To test the nature of "learning" among users of the institutions would require more microanalysis than is possible given the purposes of this paper. For a discussion of learning effects at the international system level see Hayward R. Alker, Jr., "On the Regulation of International Disputes: Toward a Stochastic Model of Directive Behavior," paper delivered at the 1969 APSA Annual Convention (mimeo).

Assessing the costs against the benefits, states have only been willing to take disputes to the ICJ or UN once. Both institutions have produced few "winners" in any dispute, because they have reached primarily compromise decisions; hence there is little gain to be expected from using the institutions. At the same time, domestic political elements within states can be expected to react negatively to the use of the institutions unless there is a clearcut victory.

Mode of Introduction. Let us now consider the ways in which the institutions first become involved in the international bargaining process. Although the mode of introduction between the two Courts on the one hand and the League and UN on the other differ substantially,¹⁰ the key distinction is between submission through joint agreement and submission by one of the primary parties.¹¹ Joint submission indicates some willingness on the part of *both* parties to resolve the issue or at least to give the international institutions a chance. In contrast, action by one of the parties with or without a third party indicates that the initiating side is attempting to enlist the institution in a fight against the other side. Lynn H. Miller, for example, argues in a case study of the Kashmir Dispute that when India "finally took the issue to the international forum, they went seeking a collective action against Pakistan rather than a neutral agency that would separate the contestants."¹²

Table 3 presents the cases broken down into joint submission and nonjoint submission for the four institutions. Given the structural differences of the institutions, one would expect a greater tendency for joint submissions to the Court than the League or UN. However, while

¹⁰ The original coding categories for the PCIJ and ICJ were: (1) Mutual submission, (2) Compulsory jurisdiction accepted by respondent, (3) Compulsory jurisdiction challenged by respondent but overruled by the Court, (4) Compulsory jurisdiction challenged by the respondent and upheld by the Court, (5) Application accepted by the second party and (6) Application refused by the second party. For the League and UN, the categories were (1) Joint submission, (2) Unilateral submission by one participant, (3) Unilateral submission by third party, (4) Multilateral by third parties without participants, (5) Multilateral by third parties with one of the participants and (6) Multilateral by third party with both participants.

¹¹ The sixfold categories were collapsed into dichotomies by including the first category for Court and League-UN as well as the sixth for League-UN in the joint category, and excluding categories three, four, and five for the League-UN. All of the rest were classified as nonjoint submissions.

¹² Lynn H. Miller, "The Kashmir Dispute," in *International Law and Political Crisis: An Analytic Casebook*, ed. Lawrence Scheinman and David Wilkinson (Boston: Little, Brown, 1968), p. 49.

Table 3. Distribution of Cases According to Method of Introduction for PCIJ, ICJ, League, and UN (Percentages)†

	Joint Submission	Nonjoint Submission
PCIJ N=24	42	58
ICJ N=31	10	90
League N=32*	25	75
UN N=31	3	97

* Three League cases were omitted since they involved third-party introduction.

† Since both the joint/nonjoint variable and the institutions are nominal categories, phi values were computed as a measure of difference between the institutions. The matrix values were:

	ICJ	League
PCIJ	.37	.18
UN	.13	.31

(Historical/Structural Index = .25/.10 = 2.5)

there is some structural effect in evidence, joint submissions on the whole were much more a part of the interwar period than the postwar period.¹³ States used both the PCIJ and League more so than the ICJ and UN as an independent third-party rather than as a political institution to enlist on its side. Both historical period and institutional structure appear to have had an impact on the mode of introduction.

Attributes of the Participants. In our analysis we have employed two variables to measure participation. The first is merely the frequency of use by each state, while the second is a weighted frequency. States were given a "3" for each initiated participation, a "2" for each participation in which they submitted a dispute jointly, a "1" for each participation in which they responded to a unilateral initiation by another state, and a "0" when they completely ignored a unilateral submission directed at them.¹⁴

¹³ The evidence for the sharp difference between the PCIJ and ICJ is even more substantial if one counts the four unilateral applications that were immediately accepted by the respondent parties in the interwar period as joint submissions. There were none of that type in the ICJ period.

¹⁴ The "0" weighting was applied to those states that refused to respond to a unilateral application in the World Court. In addition, the four cases in the PCIJ period in which the responding states accepted application were weighted as if they were joint submissions. This weighting decision was made because it appears that there had been previous tacit if not overt agreement between both states.

Table 4. Raw and Weighted Frequency Distributions of Competitiveness of Political Systems for Participation in the PCIJ, ICJ, League, and UN (Percentages)†

	Closed	Semi-Open	Open
PCIJ			
Raw N=48	29	21	50
Weighted N=96	23	21	56
ICJ			
Raw N=62	26	14	60
Weighted N=116	13	12	75
League			
Raw N=70	31	23	46
Weighted N=132	30	24	46
UN			
Raw N=62	37	29	34
Weighted N=120	38	30	32

† The open-closed variable was treated as an ordinal measure and the institutions as nominal. Hence, Wilcoxon's signed-ranks test was employed to measure difference. The non-weighted matrix is:

	ICJ	League	
PCIJ	.09	.05	(Historical/Structural Index
UN	.24	.12	= .01/.15 = .07)

The weighted matrix is:

PCIJ	.22	.09	(Historical/Structural Index
UN	.44	.13	= .05/.27 = .19)

Given these indices of participation as the dependent variable, we ask what type of state is most likely to use one of these institutions. Three of the national attributes more commonly cited in the literature are: (1) the nature of the political system (2) the stage of economic development, and (3) the sociocultural background of the state.¹⁵ Using the raw and weighted frequencies, we plot the distribution of each of these variables for the four institutions.

One of the dimensions most frequently employed to characterize entire political systems is the open-closed continuum. We have used the competitiveness among political parties within a state as an indicator of the degree of openness of a political system, according to Fred R. von der Mehden's framework for analyzing

party systems.¹⁶ Noncompetitive party systems such as the Soviet Union's were classified as "closed" political systems. Semi-competitive party systems like South Africa's were classified as "mixed," while competitive party systems like that of the United States were classified as "open." Given the assumptions of some scholars that open political systems are more subject to the influence of so-called "world public opinion" and are more likely to seek public forms of international diplomacy,¹⁷ the competitiveness of the party system seemed to be a satisfactory way to tap those aspects of the political system most closely related to participation in international institutions.

Table 4 indicates the raw and weighted frequency distributions of the use of the four institutions by each of the three types of political systems. There does appear to be a generally positive relationship between the openness of a political system and its utilization of the four institutions. But this relationship is not merely a simple proclivity for open political systems to employ all public forms of diplomacy. Rather, the Court is more likely to be used by open political systems than is the League or UN. Furthermore, this difference becomes stronger as one moves from the interwar to the postwar period. The weighted frequencies show an even more pronounced tendency for the open systems to resort more heavily to the Court. Since the weighting makes the willingness of the nations to initiate the use of the institutions more important in the overall frequencies, we can conclude that open systems tend not only to employ the Court more often than the League or UN but also to do the initiating.

The state's economic position is often viewed as a relevant variable in the usage of interna-

¹⁶ Fred R. von der Mehden, *Politics of the Developing Nations* (Englewood Cliffs: Prentice-Hall, 1964), pp. 53-64. For an extensive discussion of the open-closed continuum applied to political systems, see William D. Coplin, *Introduction to International Politics* (Chicago: Markham Publishing Co., 1971), Chap. 3.

¹⁷ This kind of assumption is more typical of "Wilsonian type thinking" (see Robert E. Osgood, *Ideals and Self-Interest in America's Foreign Relations* [Chicago: University of Chicago Press, 1953], pp. 172-195), based on the now discredited view that democracies are somehow morally superior to autocracies and that "world public opinion" is alive and well. Nevertheless, there is still reason to assume that political systems in which foreign policy issues are openly debated are more prone to employ international institutions if only to appease certain political factions within the country. For a list of other variables that measure political systems see Arthur S. Banks and Robert B. Textor, *The Cross-Polity Survey* (Cambridge: MIT Press, 1963).

¹⁵ A particularly popular argument is that international institutions serve the Western developed states. In terms of the comparative study of foreign policy, participation in international institutions might be viewed as a foreign policy behavior "determined" by such variable clusters as Rosenau's idiosyncratic, societal, role, governmental, and systemic variables. See James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics*, ed. R. Barry Farrell (Evanston: Northwestern University Press, 1966), pp. 27-93.

tional institutions. In particular, the World Court is frequently cited as being too expensive for use by the poorer nations.¹⁸ Using Rostow's concept of the stages of economic growth, we have discriminated three categories: (1) Underdeveloped (traditional + pre-take-off), (2) Intermediate (take-off) and (3) Developed (maturity + high mass consumption).¹⁹ Participants have been assigned into one of the three categories, with weighted as well as raw frequency distributions determined for the four institutions.

The results, presented in Table 5, indicate that institutional structure is the most important factor in accounting for differences among the institutions. More than 60 per cent of the PCIJ and ICJ cases involve participation by developed states while less than 40 per cent of the cases in the League and UN involve developed states. The difference is accentuated even more in the weighted frequency, where the evidence indicates that the developed states initiated more often in the Court while the underdeveloped states initiated more often in the League and UN. Once again, differences in the usage of the institutions are more pronounced over time, underdeveloped states using nonjudicial institutions increasingly in the postwar era.

A final national attribute often assumed to be important in shaping the attitudes and behavior of states toward international institutions might be termed geocultural region. The variable recognizes both the geographical and cultural factors that scholars like Adda B. Bozeman consider important in understanding the growth and development of international institutions.²⁰ Bruce M. Russett has developed the concept of the sociocultural region²¹ which corresponds closely to those geocultural factors

Table 5. Raw and Weighted Frequency Distributions of Economic Development for Participation in the PCIJ, ICJ, League, and UN (Percentages)†

	Underdeveloped	Intermediate	Developed
PCIJ			
Raw N=48	6	31	63
Weighted N=96	5	29	66
ICJ			
Raw N=62	8	19	73
Weighted N=116	7	18	75
League			
Raw N=70	16	47	37
Weighted N=132	17	49	34
UN			
Raw N=62	29	35	36
Weighted N=120	37	33	30

† The economic development variable was treated as an ordinal measure and the institutions as nominal. Hence, Wilcoxon's signed-ranks test was again employed to measure difference.

The non-weighted matrix is:

	ICJ	League	
PCIJ	.09	.28	(Historical/Structural Index
UN	.40	.10	= .03/.35 = .09)

The weighted matrix is:

	ICJ	League	
PCIJ	.09	.35	(Historical/Structural Index
UN	.50	.17	= .01/.43 = .02)

usually thought to be most relevant to the attitudes of states toward international institutions. From Russett's findings we have classified states as members of (1) the Western Community, (2) Latin America, (3) Eastern Europe and (4) the remaining states.²²

Table 6 indicates a substantial difference between the PCIJ and ICJ on the one hand and the League and UN on the other. A majority of the users of the Court are Western countries in both historical eras. The weighted frequencies indicate that an even larger proportion of Western states are the initiators. In contrast, there is more even spread among the users of the League and UN. Historical era, however, also appears to be important for the two international organizations, since there is a shift in the postwar period away from use by Eastern European and Western Community states toward higher use by the states of Asia and Africa. The findings illustrate an impact of both

¹⁸ For example, see Kaplan and Katzenbach, *Political Foundations*, p. 281.

¹⁹ Post-World War II data was obtained from Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, and Harold Lasswell, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), pp. 294-298. Pre-World War II status was based on the postwar standings in addition to economic reports in the *Statesman's Yearbook*.

²⁰ Adda B. Bozeman, *Politics and Culture in International History* (Princeton: Princeton University Press, 1960). Also see William D. Coplin, *The Functions of International Law* (Chicago: Rand McNally, 1966), Chap. 5.

²¹ Bruce M. Russett, *International Regions and the International System: A Study in Political Ecology* (Chicago: Rand McNally, 1967), pp. 14-33. The Russett groupings include both geographical and sociocultural considerations. Although a large number of classifications can be predicted by geography, a few cannot (e.g., Japan is in the Western group).

²² Russett, pp. 24-25. Russett actually has six categories (Afro-Asia, Western Community, Latin America, Semi-Developed Latins, Eastern Europe, and Unclassifiable). The Semi-Developed Latins were grouped under Latin America, and the five unclassifiable states under Afro-Asia.

Table 6. Raw and Weighted Frequency Distributions of Geocultural Regions for Participation in the PCIJ, ICJ, League, and UN (Percentages)[†]

	Western Community	Latin America	East- ern Europe	Other
PCIJ				
Raw N=48	61	4	29	6
Weighted N=96	68	4	23	5
ICJ				
Raw N=62	58	13	16	13
Weighted N=116	75	11	4	10
League				
Raw N=70	40	11	33	16
Weighted N=132	39	12	30	19
UN				
Raw N=62	27	13	10	50
Weighted N=120	23	13	8	56

[†] The geocultural region variable as well as the institutions constitute nominal scales. Hence, the lambda statistic was employed to measure difference. The non-weighted matrix is:

	ICJ	League	
PCIJ	.08	.02	(Historical/Structural Index
UN	.37	.32	= .21/.18=1.1)

The weighted matrix is:

	ICJ	League	
PCIJ	.18	.14	(Historical/Structural Index
UN	.51	.35	= .22/.35=.63)

institutional structure and historical period, then, on the geocultural attributes of the states using these institutions.

National Attributes and the Legitimacy of International Institutions

One might interpret the findings on the relationship between national attributes and institutional usage in the context of the notion of *legitimacy*. Most commonly discussed in the study of developing political systems, the concept of legitimacy ought to play a more central role in the analysis of international law and organization.²³ Scholars such as Almond and Powell suggest that the source of legitimacy for political institutions can usually be found in the strength of the symbols represented by the governmental authorities and in the effectiveness of those authorities in meeting the demands of the

people.²⁴ Using an analogy which assumes for reasons of ease of expression that when we talk about the "state" we are referring to foreign policy decision-makers and those who influence the decision-makers, one might argue that the legitimacy of international institutions will increase as: (1) the institutions are perceived to represent the symbols and values operating within the state's political culture, and (2) the institutions are perceived to be responsive to the interests of the states. If these two points are accepted, the findings on national attributes are interpretable.

The tendency for Western, open, and developed states to employ the PCIJ and ICJ suggests that both a symbolic basis and responsiveness are necessary to generate legitimacy for the Court in the international bargaining process.²⁵ At the symbolic level, the rule of law associated with open political systems and the Western geocultural regions serve as a basis of identity for the state and the international institution. At the responsiveness level, the general view that the Court tends to favor the status quo would lead developed states, particularly in their dealings with underdeveloped states, to perceive the Court as being responsive to their interests.²⁶ In addition, the tendency for "new" states—whether they be Eastern European states in the interwar period or the Asian-African states in the contemporary period—for the closed political systems and for the poorer states to avoid the Court suggests that neither the symbolic nor the responsive basis exists for the participation of a large part of the international political system in the operation of the Court.

Similarly, the finding that non-Western, closed, and underdeveloped states have been more willing to employ both the League and the UN suggests that a symbolic basis may not be as necessary for the legitimacy of these institutions as an image of responsiveness. Scholars have generally noted that the League and UN are better equipped to make decisions changing the status quo than is either the PCIJ or ICJ.²⁷

²³ Gabriel A. Almond and G. Bingham Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966), pp. 199-203.

²⁴ More than 50 per cent of the states that used the Courts had open political systems, had developed economies, and belonged to the Western Community.

²⁵ Kaplan and Katzenbach, *Political Foundations*, pp. 265-283.

²⁶ The assumption that the Courts support the status quo and the League and UN are more open to peaceful change is at the basis of the political-legal distinction and is common in the international

²³ Although Seymour Martin Lipset, in *Political Man* (New York: Doubleday, 1963), pp. 64-70, tries to distinguish between legitimacy and effectiveness, learning theory suggests that the former is generated by the latter. See Richard M. Merelman, "Learning and Legitimacy," *The American Political Science Review*, 60 (September, 1966), 548-561.

At the same time, the League and UN are probably viewed by the states themselves as being more effective than the PCIJ and ICJ in controlling to some degree the use of force by the strong against the weak. Both of these factors may play some role in creating an image of responsiveness to the interests of the underdeveloped states and hence in providing legitimacy for the League and UN in these states.

Relationships Between Participants

In this section, the relationship between the two participants in a particular dyad and the role of the four institutions in the international bargaining process will be examined. The discussion will focus on (1) the degree of similarity as well as interdependence between the two states in each dyad, and (2) the relative "power" rankings between the two states in the dyad.

Similarity and Interdependence. Similarity between states is often considered to be an important variable in determining their interactions. Karl W. Deutsch, for example, has argued that social and political similarities contribute to greater international cooperation.²⁸ One might employ a wide variety of variables to assess the similarity between states.²⁹ The three selected for this study, based on the national attributes discussed earlier, are political, economic, and geocultural similarities.

The data in Table 7 were generated by coding states as politically similar if they shared the same category of political system, economically similar if they shared the same category of economic development, and geoculturally similar if they shared the same geocultural region. If they did not share the same category, they were coded as dissimilar. There was no at-

Table 7. Political, Economic and Geocultural Similarities Between the Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Political		Economic		Geocultural	
	Same	Different	Same	Different	Same	Different
PCIJ N=24	50	50	50	50	46	54
ICJ N=31	35	65	61	39	39	61
League N=35	51	49	49	51	51	49
UN N=31	35	65	23	77	35	65

† Since both similarity and institution are nominal variables, phi was employed. The matrix values for political similarity are:

	ICJ	League
PCIJ	.15	.01
UN	.00	.16

(Historical/Structural Index = .15/.02 = 7.5)

The matrix values for economic similarity are:

	ICJ	League
PCIJ	.11	.01
UN	.39	.27

(Historical/Structural Index = .07/.20 = .35)

The matrix values for geocultural similarity are:

	ICJ	League
PCIJ	.07	.05
UN	.03	.16

(Historical/Structural Index = .12/.02 = 6.0)

tempt in this dichotomous classification to scale the states along these dimensions.

The pattern of similarity within dyads is complex. The PCIJ and League generally handled as many disputes involving politically dissimilar states as politically similar states, whereas the ICJ and UN were employed most often in cases involving politically dissimilar countries. While there is little difference in the pattern of economic similarity between the PCIJ and League, the ICJ and UN show opposite tendencies. In terms of geocultural similarity, historical era appears to have had a large effect, with the postwar period showing the impact of the universalization of the state system.

Moving from the similarity between members of the dyad to their interdependence, we have selected three of the more common measures—military distance, geographical proximity, and economic interdependence as measured by international trade. These three aspects tap relatively different dimensions, but all imply a degree of what Cobb and Elder call "mutual

law and organization literature. See, for example, *Report of a Study Group on the Peaceful Settlement of International Disputes* (London: David Davies Memorial Institute of International Studies, 1966), pp. 4-6.

²⁸ Karl W. Deutsch, "The Probability of International Law," in *The Relevance of International Law*, ed. Karl W. Deutsch and Stanley Hoffmann (Cambridge: Schenkman Publishing Co., 1968), p. 57.

²⁹ Wright's most systematic presentation of the concept of distance and the related framework of field theory can be found in *The Study of International Relations* (New York: Appleton-Century-Crofts, 1955), pp. 531-567. Also see Wright's *The Study of War*, pp. 1466-1472. For a systematic discussion of a large number of similarities as well as interdependence, see Roger W. Cobb and Charles Elder, *International Community: A Regional and Global Study* (New York: Holt, Rinehart, 1970), pp. 23-55.

Table 8. Military Distance Between the Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Direct Allies	No Military Ties	Members of Opposing Alliances
PCIJ N=24	4	79	17
ICJ N=31	22	52	26
League N=35	6	94	0
UN N=31	7	87	6

† Since military distance is treated conceptually as an ordinal variable, the Wilcoxon signed-ranks test was employed. The values in the matrix are:

	ICJ	League	
PCIJ	.07	.18	(Historical/Structural Index
UN	.04	.06	= .01/.10 = .10)

relevance"³⁰ as well as what Russett has termed "responsiveness"³¹ between states. Because mutual relevance and responsiveness can lead to both cooperation and conflict, it is difficult to postulate the degree to which interdependence would be related to the role of the four institutions in the international bargaining process (as was also the case in relating the similarity measures).

In determining the degree of military interdependence between the two states in each dyad, we attempted to include what might be termed the military distance between states. Dyads were coded in terms of whether the two states were direct allies, had no military ties, or, were members of opposing alliances.³² As a result, this variable includes something more than military interdependence *per se*. Table 8 indicates that the patterns for the League and UN are relatively similar, with an overwhelming majority of the cases involving states with no military ties. In contrast, the PCIJ and to a larger extent the ICJ, have a moderate percentage of cases involving states that were members of opposing alliances.

If one accepts the usual assumption that the

³⁰ See Cobb and Elder, *International Community*, pp. 22-132 for a discussion and operationalization of the concept of mutual relevance. Also, the book suggests a variety of approaches to measuring interdependence among states.

³¹ Bruce M. Russett, *Community and Contention: Britain and America in the Twentieth Century* (Cambridge: MIT Press, 1963), pp. 26-33.

³² The data on military alliance were taken from J. David Singer and Melvin Small, "Formal Alliances, 1815-1939: A Quantitative Description," *Journal of Peace Research*, 3, No. 1 (1966), 1-32.

Court is concerned with "legal" issues and the League and UN with "political" issues, the findings are somewhat contrary to what one would expect. It should be remembered that the sample of cases upon which this study is based is dyadic, and that members of opposing alliances are most likely to be involved in multilateral issues before the League and UN. Although this fact would explain the low number of cases involving members of opposing alliances for the League and UN, it does not explain the relatively high number of cases involving members of opposing alliances for the PCIJ and ICJ. Hence, it is fair to interpret the evidence as indicating that the PCIJ and even more so the ICJ have not been as apolitical as some might have hoped at the inception of the institutions. All of the ICJ cases involving members of opposing alliances can be attributed to friction between the United States or United Kingdom and the Warsaw Pact countries, and the implication from these instances is that the Court has been frequently used by parties for propagandizing or legitimizing purposes rather than with expectation of settlement.

The degree of economic interdependence was roughly approximated, based on reports of international trade in the *Statesmen's Yearbook*. Dyads were coded according to three categories of economic interdependence—heavy, moderate, and none.³³ Table 9 presents frequency distribution of dyads for each of the four institutions. The results indicate effects from both historical era (PCIJ/League vs. ICJ/UN) and institutional structure (Court vs. League/UN), with the former having the larger impact. The PCIJ and League distributions are somewhat similar while the ICJ and UN distributions differ in the same direction from the institutions of the interwar period. Economic interdependence, however, appears to be less negatively associated with ICJ involvement than it does with UN involvement.

Finally, there is the question of geographical proximity which writers have traditionally

³³ The following criteria were employed in classifying the dyads. The dyad was coded as having no economic interdependence if neither country was mentioned as a principal trading partner of the other in the *Statesmen's Yearbook* for the year of submission of the case. High interdependence was coded if both states were mentioned in each other's descriptions as principal trading partners, while moderate interdependence was assigned to those dyads in which only one state was mentioned in the two descriptions as a principal trading partner. The measure is exceedingly rough, but the lack of available data for the interwar period prevented the use of the "Direction of Trade" statistics now available for the post-war period.

Table 9. Economic Interdependence Between Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Heavy	Moderate	None
PCIJ N=24	38	50	12
ICJ N=31	26	29	45
League N=35	43	40	17
UN N=31	10	39	51

† The economic interdependence variable is treated as an ordinal measure and the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League	
PCIJ	.31	.02	(Historical/Structural Index
UN	.14	.48	= .40/.05 = 8.0)

viewed as influencing the ways states deal with each other.³⁴ Dyads were coded for common border and for distance (between nearest borders) either below or above 500 miles. Table 10 presents the data for the four institutions. Again, the structure of the institutions as well as the historical era appear to have an impact. The League and UN have generally dealt with cases between disputants having a common border, while the Courts have had fewer dyads in this category. The impact of historical era is indicated by the extreme positions of the League and ICJ on this variable, showing the combined effects of history and structure.

An aggregate view of the six similarity and interdependence findings suggests that it is more likely for the dyad using the institutions to be dissimilar and relatively independent than to be similar and highly interdependent. This finding contrasts with the viewpoint prevalent in the literature that high interdependence and similarity, other things being equal, are likely to contribute to the development of such regional international institutions as the European Economic Community.³⁵ Rather than contradicting the findings of this paper, however, the contrast suggests a basic distinction

³⁴ Geography is probably the oldest and most multifaceted relationship discussed in the international relations literature. Concern for the interaction of geography and relationships between states is not just a product of a geopolitical outlook (e.g., Sykman) but is also evidenced by students of integration (e.g., Cobb and Elder, *International Community*, pp. 84-92) and power politics (e.g., Hans Morganthau, *Politics Among Nations*, 3rd ed. [New York: Alfred Knopf, 1967], pp. 106-109).

³⁵ For example, Deutsch, "The Probability of International Law," in *Relevance of International Law*, p. 57; and Cobb and Elder, pp. 9-11.

Table 10. Geographical Distance between Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Common Border	1 to 500 Miles	More than 500 Miles
PCIJ N=24	41	17	42
ICJ N=31	19	20	61
League N=35	80	9	11
UN N=31	55	16	29

† Since geographical distance has been treated as an ordinal measure, the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League	
PCIJ	.25	.40	(Historical/Structural Index
UN	.40	.26	= .29/.42 = .69)

between regional functional organizations such as the EEC on the one hand, and the PCIJ, ICJ, League, and UN on the other. (The distinction elaborated below applies somewhat to global functional institutions, such as the World Health Organization, as well.)

The difference between the two types of institutions is related to the role of cooperation among the states using the organization and the structure of the bargaining process as a result of that cooperation. The regional functional organization symbolizes the realization among its members that a common set of problems exists. The bargaining process that takes place within the framework of the functional organization, therefore, is thoroughly shaped by the understanding that the decisions of the organization can benefit all of the states. Consequently, states bargain over the distribution of collective costs (e.g., financial contributions) and benefits (e.g., allocation of technical assistance).³⁶ In contrast, peace-promoting organizations attempt to deal with issues generated not by the perception of a common problem but by the assumption that the other party to the dispute is the cause of the problem. Bargaining takes place over private benefits between the major antagonists and consists primarily of each state's attempting to convince the other state to change its behavior.

³⁶ For a general discussion of the distinction between collective and private benefits see Mancur Olson, Jr., *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965). The distinction is applied to the study of international alliances by Philip M. Burgess and James A. Robinson, "Alliances and the Theory of Collective Action: Simulation of Coalition Processes," *Midwest Journal of Political Science*, 13 (May, 1969), 194-218.

"Power" Rankings Among States. Somewhat related to the question of similarity and interdependence is the traditional concern many scholars have had for power relations among states. Although the utility of the power concept in the study of international relations is seriously questionable,³⁷ its prevalence in the literature prompts our consideration of it. The preoccupation with power has origins in at least two different scholarly traditions. The first is the power politics school as exemplified by the writings of Organski and Morgenthau.³⁸ Generally, it is assumed that the powerful will dominate the powerless and that conflict will increase as the weaker states begin to gain equality with the stronger ones. The second tradition develops out of the sociological literature and has been applied to the study of international law by Ib Martin Jarvad.³⁹ Arguing that states rank themselves partially in economic and military terms and partially in other terms, Jarvad postulates that states attempt to maximize their rank. Both the power tradition and the rank tradition, then, assume that states are most concerned with maintaining or increasing their positions vis-à-vis each other, however those positions may be defined.

If the assumptions of both traditions are applied to the role of the four institutions in the international bargaining process, the weaker or lower-ranked states might be expected to employ institutions more often against the stronger or higher-ranked states. This expectation would be based on the view that the guarantee of equality before the institutions explicitly developed in the charters of the four institutions is a reality. Furthermore, given the spe-

cial position accorded great powers in both the League and UN, one would expect that for smaller states, the Courts would be even more appealing than the League or UN would be.

To test these ideas about what Jarvad has called "power versus equality" for the four institutions is far beyond the scope of this paper.⁴⁰ A preliminary test has been undertaken, however, by looking at the relative power as measured by total GNP of the states in each dyad and the manner in which these states employed the four institutions. The use of GNP as an indicator of power leaves much to be desired, but for our preliminary purpose it appears a satisfactory measure since it represents wealth and population size—two of the most commonly referenced indicators of power.⁴¹ All cases in which there was a unilateral initiator have been coded in terms of whether the GNP of the initiator was greater (by at least 25 per cent), less (by at least 25 per cent), or substantially the same as that of the responding state.⁴² (Cases introduced jointly or by a third party have been ignored since it is difficult to determine the degree to which the behavior of the members of the dyad was different.) Table 11 presents the results.

In terms of the theoretical questions posed by Jarvad, the data provide a striking contrast between the PCIJ and the ICJ on the one hand and the League and UN on the other. The majority of unilaterally submitted cases in the PCIJ and ICJ are initiated by the more powerful states, while the opposite is true in the League and UN. This finding suggests that states perceive the Court as the protector of the status quo and the League as well as UN as the medium for international change.⁴³ The substantial difference attributed to structure sug-

³⁷It appears that neither the power nor the rank tradition as applied to international relations has confronted the fact that different sources of influence operate in (1) different time periods, (2) for different states and (3) in different issue areas. Instead, it is assumed that power is a product of military and economic capability even though the writers might argue that other factors such as willingness to make commitments are involved. For a discussion of the problems in the literature see Coplin, *Introduction to International Politics*, Chap. 4.

³⁸Morgenthau, *Politics Among Nations*, and A. F. K. Organski, *World Politics*, 2nd ed. (New York: Alfred A. Knopf, 1968), pp. 338-377.

³⁹Ib Martin Jarvad, "Power versus Equality: An Attempt at a Systematic Analysis of the Role and Function of the International Court of Justice, 1945-1966," paper presented at the Second Conference of International Peace Research Association (IPRA), 1967 (revised mimeo). Jarvad suggests that the sociological literature relevant to the study of rank and international politics is identified in Johan Galtung, "International Relations and International Conflicts: A Sociological Approach," *Transactions of the Sixth World Congress of Sociology*, 1 (1966), 121-161.

⁴⁰Jarvad has applied this framework only to the ICJ, but it can be applied with equal usefulness to the other three institutions.

⁴¹See Bruce M. Russett, *Trends in World Politics* (New York: Macmillan, 1965) for a discussion of the use of GNP as a measure of power. Jarvad, p. 4, employs six variables, all of which are related to economic development. Again, the choice of operational indicators of rank as well as power shows the dependence on a simplistic notion of influence.

⁴²Data for GNP for dyads after the Second World War were taken from the *United Nations Statistical Yearbook*. For interwar data, rough approximations were made based in part on national income data in sources such as the *Statesmen's Yearbook*. Given the theoretical question and the choice of a rather arbitrary cutoff point, however, the reliability of the data seems adequate. Little difference would have resulted from raising or lowering the cutoff point ten percentage points.

⁴³This point is related to the findings reported above, indicative that a large percentage of the cases before the Courts involved the developed states.

Table 11. Relative GNP Rankings between Disputants in Unilaterally Initiated Cases Before the PCIJ, ICJ, League, and UN (Percentages)†

	Same Rank	Initiator Bigger (by at least 25%)	Initiator Smaller (by at least 25%)
PCIJ N=14	29	57	14
ICJ N=28	14	64	22
League N=24	25	17	58
UN N=30	0	20	80

† Since the GNP comparison was treated as a nominal scale, lambda was employed. The values in the matrix are:

	ICJ	League	
PCIJ	.00	.29	(Historical/Structural Index
UN	.57	.25	= .16/.43 = .37)

gests that neither type of institution is viewed as a neutral agent, but rather is considered to be an institution through which rank is maximized. In the case of the Courts, the strong appear to be maintaining or increasing their position, while in the case of the League and UN, the weak assume that they can enhance their position.

Characteristics of the Disputes and Institutional Responses

Salience of Bargaining Issues. Turning from an analysis of the actors and their relationship to each other, we now consider the characteristics of the dispute itself, in particular the salience of bargaining issues. The traditional distinction between legal and political disputes—a distinction that appears in both the international law and international organization literature—suggests that a major dimension that we should uncover in our typology is *salience*. This conclusion is based on the growing consensus that nothing inherent in the issue makes a dispute legal rather than political but that indeed the determination is made by the actors themselves.⁴⁴ A dispute is legal if the parties choose to consider it a legal dispute. By implication, then, the decision to consider a dispute primarily in the legal dimension, particularly for the side with the weaker legal argu-

⁴⁴ For a clear statement on this point see *Report of a Study Group on the Peaceful Settlement of International Disputes*, pp. 4-6.

Table 12. Existence of Hostilities in Cases before the PCIJ, ICJ, League, and UN (Percentages)†

	No Hostilities	Hostilities
PCIJ N=24	100	0
ICJ N=31	71	29
League N=35	51	49
UN N=31	42	58

† Since hostility categories constitute a dichotomous nominal scale, phi has been employed. The values in the matrix are:

	ICJ	League	
PCIJ	.40	.53	(Historical/Structural Index
UN	.26	.10	= .15/.38 = .39)

ment, occurs when the state stops attaching great importance to the issue.

In searching for a typology which focuses on the dimension of salience, we find that one of the more obvious factors is the presence of hostilities.⁴⁵ We can assume that the more salient a bargaining issue, the more likely it is that the parties will resort to violence. Table 12 indicates the frequencies of disputes that involved hostilities vs. those that did not.

A comparison of the frequency distributions in Table 12 suggests that the League and UN generally handle cases involving hostilities while the Courts do not. Cutting slightly across this impact of structure on the role of hostilities in cases is the role of historical era. For both types of institutions, the interwar period involves fewer hostilities than the postwar period does. It suggests that the disputes submitted to the postwar institutions have been of higher salience to the participants than those submitted in the interwar period.

⁴⁵ Future attempts at determining the salience of an issue in this area might borrow on the work of a number of scholars in collecting data and measuring international interactions. McClelland's World Event Interaction Series Project at the University of Southern California, or Rudolph Rummel's Dimensionality of Nations Project at the University of Hawaii might provide data on the more recent cases relevant to levels of hostility surrounding the relationships of states involved in a particular dispute. In a recent paper by William D. Coplin and Michael K. O'Leary, "A Simulation Model for the Analysis and Explanation of International Interactions" (delivered at the 1971 ISA National Convention), a different way of operationalizing and in fact conceptualizing the concept of salience was presented. Briefly, that method assumes that the broader the awareness of an issue within the political system of a state, the more salient the issue.

Table 13. Type of Issue in Cases Before the PCIJ, ICJ, League, and UN (Percentages)[†]

	Contractual	Treatment of Persons	Territorial	Threat of Hostilities	Outbreak of Hostilities	NA
PCIJ N=24	42	50	8	0	0	0
ICJ N=31	26	16	32	0	26	0
League N=35	14	12	34	6	34	0
UN N=31	3	7	16	10	58	6

[†] Assuming the issue typology represents an ordinal scale, the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League	
PCIJ	.46	.67	(Historical/Structural Index = .33/.54 = .61)
UN	.53	.39	

A second typology related to the salience dimension focuses on the issue-area of a particular dispute.⁴⁶ A fivefold typology has been developed: (1) contractual obligations, (2) the treatment of people, aliens, and minorities, (3) territorial disputes, (4) threatened hostilities, and (5) actual hostilities. A salience dimension is assumed to underlie the typology. This assumption is based on the viewpoint that broader and more powerful segments of the domestic political environment become mobilized as one moves from categories 1 through 5, which in turn results in increased salience for the foreign policy decision maker and hence the national actor taken as a collectivity.⁴⁷

Table 13 compares the frequency distributions for this typology for all four institutions. A sharp difference between the Court on the one hand, and the League and UN on the other, might be expected if one accepted the view that the Court deals with legal and therefore less salient disputes than the League and UN. In fact, this expectation is generally fulfilled. At the same time that there is a difference attributable to institutional structure, there is also one attributable to historical period. The salience of disputes increases for the ICJ over the PCIJ and for the UN over the League.

The Impact of the Institutions. The scholarly literature posits a wide variety of impacts for the four institutions on the international bargaining process. Although the categories are somewhat arbitrary, one can identify three relatively distinct roles scholars have ascribed to the four institutions in the international bargaining process: (1) environmental, (2) regulative, and (3) distributive.

International law and organization have been viewed as a medium in which bargaining among states takes place. In earlier writings, one of the authors has argued that international law may best be viewed as providing a bargaining environment for states because the law provides a set of orientations in which states relate themselves to each other.⁴⁸ Young maintains that during periods of crisis the United Nations "has some effect in setting the general atmosphere and climate of opinion in which bargaining among the principals (takes) place."⁴⁹ This view of international law and organization as performing an environmental role maintains that legal norms provide a set of concepts or symbols in which actors phrase their positions, and international institutions provide a set of routinized communication and interaction channels.⁵⁰

⁴⁶ The issue-area concept has been developed outside the international politics field. See Robert Dahl, *Who Governs?: Democracy and Power in an American City* (New Haven: Yale University Press, 1961). James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in Farrell, *Approaches to Comparative and International Politics*, pp. 77-93, has suggested how the concept might be applied to the study of international politics.

⁴⁷ See Coplin, *Introduction to International Politics*, Chap. 3, for a discussion of the relationship between issue-area and domestic political pressure.

⁴⁸ Coplin, "Current Studies of the Functions of International Law," in Robinson, *Political Science Annual*; and Coplin, "International Organizations in the Future International Bargaining Process," *Journal of International Affairs*, 25 (Summer, 1971), 287-301.

⁴⁹ Oran R. Young, *The Politics of Force: Bargaining During International Crisis* (Princeton: Princeton University Press, 1968), p. 169.

⁵⁰ Stanley Hoffman has criticized this view of international law and organization in his essay "International Law and the Control of Force," in *The Rele-*

The second approach sees international law and organization as regulators of the international bargaining process. This regulative view is expressed in many forms including the emphasis on peace keeping in many United Nations activities,⁵¹ Claude's concept of "preventive diplomacy,"⁵² and even the concept of collective security itself.⁵³ With regard to international law, for example, Falk has argued that the law serves as a "braking mechanism" in helping to prevent the automatic escalation of international conflict.⁵⁴

The third approach views international law and organization as distributive organs in the international bargaining process. Concepts of peaceful change and the pacific settlement of disputes developed within the Covenant and UN Charter as well as by a large number of writers imply that international institutions can serve in making authoritative distributions between states for a given bargaining issue.⁵⁵ In this view, the World Court and the collective security organizations are considered to be disinterested third parties making final decisions.

The three views are not necessarily incompatible. In fact, many writers combine two or all three views in their discussions. For example, while Young sees the UN as providing an environment in which states bargain, he also

Table 14. Length of Time for Disputes in the PCIJ, ICJ, League, and UN (Percentages)[†]

	1-12 Months	More than 12 Months
PCIJ		
N=24	33	67
ICJ		
N=31	32	68
League		
N=31*	45	55
UN		
N=21*	67	33

* It was not possible to determine the exact time of withdrawal of a dispute from the League and UN for a number of cases (4 for the League and 10 for the UN).

† Phi was employed in testing for historical and structural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.01	.13	(Historical/Structural Index
UN	.34	.20	= .05/.22 = .23)

sees it playing the role of third party intermediary in a regulative as well as distributive capacity.⁵⁶ Moreover, the institutions themselves play different roles in different situations. In the Berlin Blockade, the UN served primarily as an environment for Soviet-American communications, while in the Suez Crisis it played primarily a regulative role.⁵⁷

A variable that might be useful in assessing the degree to which the three functions are performed is the length of time that cases remain in the institutions. Table 14 indicates that the Court usually takes longer than either the League or the UN to deal with a case. In addition to the structural factor, historical effects also seem to be important, since the UN was more likely than the League to deal with cases in a year or less.

This difference suggests some ideas about the role of these institutions in the international bargaining process. It suggests that the Courts act as distributive institutions, taking their time to arrive at a decision and depending on disputes to become less salient over time. In contrast, it suggests that the role played by the League and even to a greater extent by the UN is not distributive but environmental and regulative. Since disputes containing high salience

vance of International Law, ed. Deutsch and Hoffmann, pp. 21-47. His criticism that it is necessary to look at international law as a coercive order even though it fails to approximate such a view is directed at those writers who view international law as part of the international environment. He does not deny that international law may be approached from such a viewpoint but rather that it must be viewed also in other terms.

⁵¹ See, for example, the discussions of the activities of the UN in the yearly *International Conciliation* volume published on issues before the General Assembly.

⁵² Inis L. Claude, Jr., *Swords into Plowshares: The Problems and Progress of International Organization*, 3rd ed. (New York: Random House, 1964), pp. 385-404.

⁵³ Claude, pp. 223-258.

⁵⁴ Richard A. Falk, "Confrontation Diplomacy: Indonesia's Campaign to Crush Malaysia," in *International Law and Political Crisis*, ed. Scheinman and Wilkinson, p. 162.

⁵⁵ Almost all writers impute the dispute-settling function to the League or UN or both. See, for example, Claude, *Swords Into Plowshares*, pp. 193-222, and Wolfram F. Hanrieder, "International Organizations and International Systems," *Journal of Conflict Resolution*, 10 (September, 1966), 297-313. Hanrieder cites three tasks of international organization, two of which—peaceful change and the peaceful settlement of disputes—might be considered distributive. Although this function was emphasized more strongly in the interwar period than the postwar period (see, for example, Coplin, *Functions of International Law*, ch. 3), the dispute-settling function is still emphasized today.

⁵⁶ Compare discussions of the UN in Young, *The Politics of Force*, pp. 146-171, to discussions of the UN in Young, *The Intermediaries* (Princeton: Princeton University Press, 1967).

⁵⁷ Compare Lawrence Scheinman, "The Berlin Blockade," pp. 1-41, to Wolfgang Friedmann and Lawrence A. Collins, "The Suez Canal Crisis of 1956," pp. 91-126, both in Scheinman and Wilkinson.

Table 15. Actions with External Consequences Taken by the PCIJ, ICJ, League, and UN (Percentages)[†]

	Action with Consequences	No Decision or Consequential Action
PCIJ N=24	67	33
ICJ N=31	26	74
League N=35	74	26
UN N=31	64	36

[†] Phi was employed in testing for historical and structural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.41	.26	(Historical/Structural Index
UN	.39	.11	= .26/.26=1.0)

have usually been submitted to the League and UN rather than to the Court, the impact of the League and UN has appeared to be one of keeping the peace rather than developing a distributive formula to resolve the disputes.⁵⁸

A second consideration that one ought to take into account when considering the impact of the institutions on the international bargaining process is the ability of the institution to take actions that have some consequences. For the Court, we can define this as taking decisions (judgments) that are substantive (i.e., deciding the merits of the case) as opposed to taking decisions that are strictly procedural or taking no decisions whatsoever. For the League and UN, we might define it as action in at least one of the following forms: inquiry, mediation, cease fire, truce supervision, enforcement through boycott, police action, insertion of the Secretary-General's or his deputies' presence, formation of a committee of experts, or the provision of material support. All of these activities involve more than the simple appeal to resolve the problem.⁵⁹

Table 15 lists the frequencies with which the four institutions have taken action that involves or does not involve some consequence as defined above. Both a historical and a structural

⁵⁸ One might even posit an efficiency rating for these institutions, based on the relationship between the length of time in which the institutions handle the dispute and the salience of the dispute. For high salience disputes, the longer the dispute remains in the institutions, the less efficient the institution. If this were true, the UN is clearly a more efficient institution than the League.

⁵⁹ These categories have been adapted from those supplied by Haas, *Collective Security*, pp. 38-39.

Table 16. Outcome of Disputes in the PCIJ, ICJ, League, and UN (Percentages)[†]

	Settled By or With the Help of	Unsettled or Settled Outside the Institution
PCIJ N=24	67	33
ICJ N=31	32	68
League N=35	49	51
UN N=31	19	81

[†] Phi was employed in testing for historical and structural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.34	.06	(Historical/Structural Index
UN	.15	.32	= .31/.13=2.38)

effect appears to be operating here. Generally, the interwar institutions had a better capacity to act, although the UN has done much better than the ICJ. From a theoretical point of view, the decline of the ICJ is most crucial because it represents a loss in the capacity of international institutions to make substantive decisions in bargaining among states. It suggests further that if scholars continue to view institutions as a means for settling disputes they will be disappointed.

This pessimism is further increased if we look at the record of the four institutions in settling disputes or contributing to the settlement of disputes. Using the categories suggested by Ernst Haas, we coded disputes as "settled" if both sides refrained from arguing the point (with acquiescence to a *fait accompli* not constituting settlement).⁶⁰ Table 16 indicates the distributions for the four institutions on this variable.

Again, historical and structural effects appear to be operating in the record of the four institutions although the historical trend is much stronger than it was in the case of the preceding variable. Combining the findings for Tables 15 and 16 leads one to conclude that not only is the ICJ less capable of making substantive decisions than its predecessor, but also the United Nations is less capable of resolving disputes than the League in spite of its relatively equal capacity to take actions. This would suggest that the UN is playing a role in the contemporary world that is different from

⁶⁰ Haas, p. 44.

the traditional role of conflict resolution or dispute settlement.

Generally speaking, the findings suggest that we ought to develop criteria of efficiency for the United Nations that do not require the resolution of conflict or the settlement of disputes. There is sufficient evidence here as well as in the general record of post-World War II international politics that states can deal with each other without resolving their conflicts. The UN appears to be contributing to a general situation in contemporary international politics that finds conflicts being frozen, routinized and ultimately ignored with no resolution in the normal sense of the word.⁶¹ If this is true, we need to develop categories that look at the capacity of international institutions to transfer, to render less important, and to freeze international conflicts.

Summary and Conclusion

The purpose of this article, as stated at its beginning, has been to present a data set that allows for a comparative empirical analysis of the PCIJ, ICJ, League, and UN in the international bargaining process. Although a variety of theoretical ideas now found in the scholarly literature were related to the patterns found in

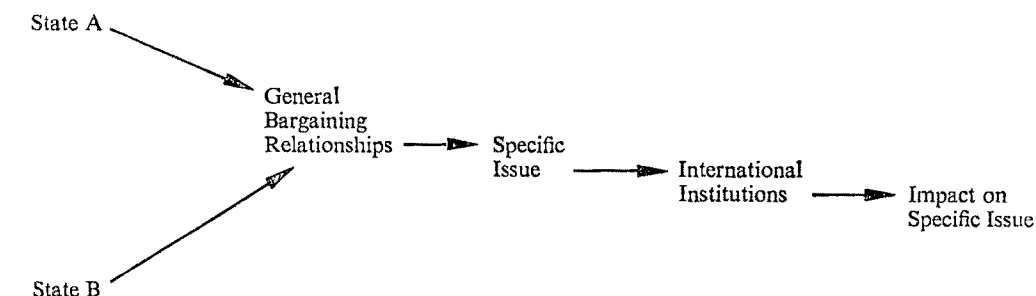
⁶¹ This point is developed in greater detail in Coplin, "International Organizations in Future International Bargaining Process."

the data, no attempt was made to develop a coherent theoretical model. In fact, the authors hope that this article will ultimately contribute to the development of empirically based theories relating international institutions to the international bargaining process.

As a method of summarizing what has been presented and providing some organizational guidelines for future scholarly work, Figure 1 presents a general framework identifying sets of variables that should be explored when examining the role of international institutions in bargaining among states. Although the scheme, and in fact the entire paper, has been focused on two-party bargaining relationships and the PCIJ, ICJ, League, and UN, it can be viewed as a general framework for studying all types of bargaining among states—including multi-lateral—and all types of intergovernmental organizations.

The diagram assumes that each group of variable sets operates on the one following it in the diagram and therefore indirectly on all the remaining variables. In addition, the impact of the institution on the specific bargaining issues is assumed to feed back through the foreign policy-making structures of the states and affects the entire international bargaining process. Those variable sets that are underlined have been used in one way or another in this paper.

Figure 1. Schematic Illustration of Framework Relating Variables in the Study of International Institutions in Bargaining Among States



RELEVANT VARIABLE SETS

- | | | | | |
|--|--|--------------------------|--|--------------------------------------|
| 1. <i>National Attributes</i> | 1. <i>Similarity in Attributes</i> | 1. <i>Sallence</i> | 1. <i>Entrance Patterns</i> | 1. <i>Environmental</i> |
| 2. <i>Previous Experience with Institution</i> | 2. <i>Interdependence</i> | 2. <i>Age of Dispute</i> | 2. <i>Processes Within Institution (e.g., consensus, voting)</i> | 2. <i>Regulative</i> |
| | 3. <i>Power or Rank Relationship</i> | 3. <i>Hostilities</i> | | 3. <i>Distributive</i> |
| | 4. <i>International Conditions (e.g., Bi-polarity)</i> | 4. <i>Issue-Area</i> | | 4. <i>Collective-problem-solving</i> |

The four diagrammed variable sets that have not been included in this study are of critical importance. Ernst Haas has shown how the conditions in the international environment relate to the role of the United Nations.⁶² A number of writers including James N. Rosenau have emphasized the importance of issue area for the study of foreign policy behavior.⁶³ One of the authors of this article, with Michael K. O'Leary, has recently written a paper arguing for the need to include issue area in any study of international interactions.⁶⁴ The processes that take place within institutions have probably been the single most studied set of phenomena in the international organization literature.⁶⁵ Finally, the category of collective problem-solving suggests that in addition to the three general functions that are now attributed to international institutions, we ought to employ criteria that assess the degree to which interna-

tional institutions contribute to the achievement of world public policy goals.⁶⁶

Another important aspect of this scheme is the critical points it suggests for further study. We ought to be concerned with the linkages between foreign policy-making processes and the bargaining interactions among states. We should also attempt to define how general bargaining relationships lead to disputes over specific issues. Furthermore, the path of specific issues into or around international institutions should be clearly specified and studied, as should the relationships between various institutional processes and the ultimate impact the institutions have on specific issues. Finally, it is necessary to look at issue impact as a learning process, through which the foreign policy-making structures within states develop predispositions toward international institutions, including international law, that may affect their future international bargaining behavior.

Whether or not scholars accept this particular scheme is not as important as the commitment to build and empirically test theories linking international institutions to the way states behave. Until we have developed our theories and created sufficient data and analytical capabilities to distinguish the myth from the reality of the role of international institutions in the contemporary world, we are bound to be tied to the oscillating excesses of pessimism and optimism that have characterized the literature of international law and organization. More importantly, if students of international institutions are able to clarify their own thinking, ultimately they may have some useful impact on shaping public policy.

⁶² Haas, *Collective Security* and Haas, "Dynamic Environment and Static System: Revolutionary Regimes in the United Nations," in *The United Nations System*, ed. Gregg and Barkun, pp. 162-197.

⁶³ See Rosenau, "Pre-Theories and Theories . . .," in *Approaches to Comparative and International Politics*, ed. Farrell.

⁶⁴ Coplin and O'Leary, "A Simulation Model . . ."

⁶⁵ See, for example, Chadwick F. Alger, "Interaction in a Committee of the United Nations General Assembly," in *Quantitative International Politics*, ed. J. David Singer (New York: Free Press, 1968), pp. 51-84; Hayward R. Alker, Jr. and Bruce M. Russett, *World Politics in the General Assembly* (New Haven: Yale University Press, 1965); John Hadwen and Johan Kaufmann, *How United Nations Decisions Are Made* (Leyden: A. W. Sythoff, 1962); Thomas Hovet, Jr., *Bloc Politics in the United Nations* (Cambridge: Harvard University Press, 1960); and Leon Gordenker, "Policy-Making and Secretariat Influence in the UN General Assembly: The Case of Public Information," in *The United Nations System*, ed. Gregg and Barkun, 136-155.

⁶⁶ See Coplin, *Introduction to International Politics*, pp. 136-155.

Appendix

A. Two-Party Cases for the Permanent Court of International Justice

Name of Case	Participants	Date of Introduction
1 Mavrommatis Palestine Concessions	Greece/Great Britain	5/24
2 Interpretation of Article 179, Annex, Paragraph 4, of the Treaty of Neuilly	Greece/Bulgaria	7/24
3 German Interests in Polish Upper Silesia and the Factory at Chorzow	Germany/Poland	5/25
4 Denunciation of the Treaty of November 2, 1865, Between China and Belgium	Belgium/China	11/26
5 The Lotus Case	France/Turkey	1/27
6 Rights of Minorities in Upper Silesia (Minority Schools)	Germany/Poland	1/28
7 Payment of Various Serbian Loans Issued in France	France/Serb-Croat-Slovene State	5/28
8 Payment of Brazilian Federal Loans	France/Brazil	4/28
9 Free Zones of Upper Savoy and the District of Gex	France/Switzerland	3/29
10 Legal Status of Eastern Greenland (includes SE Greenland case)	Denmark/Norway	7/31
11 Delimitation of the Territorial Waters between Castellorizo and Anatolia	Turkey/Italy	5/29
12 Administration of the Prince of Pless	Germany/Poland	5/32
13 Appeal from Judgment of Czechoslovak-Hungarian Mixed Arbitral Tribunal (Peter Pazmany University v. Czechoslovakia)	Czechoslovakia/Hungary	5/33
14 Polish Agrarian Reform and the German Minority	Germany/Poland	7/33
15 The Lighthouse Case	France/Greece	5/33
16 Oscar Chinn Case	United Kingdom/Belgium	5/34
17 Pajzs, Csaky and Eterhazy	Hungary/Yugoslavia	12/35
18 Losinger & Co.	Switzerland/Yugoslavia	11/35
19 Diversion of Water from the River Meuse	Netherlands/Belgium	8/36
20 The Borchgrave Case	Belgium/Spain	3/37
21 Phosphates in Morocco	Italy/France	3/36
22 Panevezys-Saldutiskis Railway	Estonia/Lithuania	11/37
23 Electricity Co. of Sofia and Bulgaria	Belgium/Bulgaria	1/38
24 Societe Commerciale De Belgique	Belgium/Greece	5/38

B. Two-Party Cases for the International Court of Justice

	Name of Case	Participants	Date of Introduction
1	Corfu Channel Case	United Kingdom/Albania	5/47
2	Fisheries Case	United Kingdom/Norway	9/49
3	Asylum Case	Columbia/Peru	10/49
4	Rights of Nationals of the U.S. in Morocco	France/United States	10/50
5	Ambatielos	Greece/United Kingdom	4/51
6	Anglo-Iranian Oil Co.	United Kingdom/Iran	5/51
7	The Minquiers and Ecrehos Case	France/United Kingdom	12/50
8	Nottebohm	Liechtenstein/Guatemala	12/51
9	Case of Certain Norwegian Loans	France/Norway	7/55
10	Case Concerning Right of Passage over Indian Territory	Portugal/India	12/55
11	Case Concerning the Application of the Conv. of 1902 Governing the Guardianship of Infants	Netherlands/Sweden	7/57
12	Interhandel Case	Switzerland/U.S.	10/57
13	Case Concerning Sovereignty over Certain Frontier Land	Belgium/Netherlands	11/57
14	Case Concerning Arbitral Award Made by King of Spain on 23 December 1906	Honduras/Nicaragua	7/58
15	Case Concerning Aerial Incident of 27 July 1955	Israel/Bulgaria	10/57
16	Case Concerning the Temple of Preah Vihear	Cambodia/Thailand	9/59
17	Case Concerning the Protection of French Nationals and Protected Persons in Egypt	France/Egypt	10/49
18	"Electricité de Beyrouth" Company Case	France/Lebanon	8/53
19	Treatment in Hungary of Aircraft and Crew of USA	USA/Hungary	2/54
20	Treatment in Hungary of Aircraft and Crew of USA (USSR)	USA/USSR	2/54
21	Aerial Incident of March 10, 1953	USA/Czechoslovakia	3/53
22	Antarctica Case	United Kingdom/Argentina	5/55
23	Antarctica Case	United Kingdom/Chile	5/55
24	Aerial Incident of Oct. 7, 1952	USA/USSR	5/55
25	Aerial Incident of July 27, 1955	USA/Bulgaria	10/57
26	Aerial Incident of July 27, 1955	United Kingdom/Bulgaria	11/57
27	Aerial Incident of Sept. 4, 1954	USA/USSR	7/58
28	Case Concerning the Barcelona Traction, Light and Power Co.	Belgium/Spain	9/58
29	Case Concerning the Compagnie du Port, des Quais et des Entrepôts de Beyrouth and the Société Radio-Orient	France/Lebanon	2/59
30	Aerial Incident of Nov. 7, 1954	USA/USSR	6/59
31	Case Concerning the Northern Cameroons	Cameroon/United Kingdom	5/61

C. Two-Party Cases for the League of Nations

	Name of Case	Participants	Date of Introduction
1	Enzeli	Iran/U.S.S.R.	5/20
2	Aaland Islands	Sweden/Finland	6/20
3	Vilna	Poland/Lithuania	9/20
4	Coto	Panama/Costa Rica	2/21
5	Upper Silesia	Poland/Germany	8/21
6	Eastern Carelia	Finland/U.S.S.R.	0/21
7	Tunis Nationality Decrees	United Kingdom/France	8/22
8	Hungarian Frontier	Hungary/Yugoslavia	5/22
9	Burgenland	Austria/Hungary	8/22
10	Salgo Tarsan	Hungary/Czechoslovakia	11/22
11	Hungarian Optants	Hungary/Romania	3/23
12	Jaworzina	Poland/Czechoslovakia	9/23
13	Corfu	Greece/Italy	9/23
14	Ecumenical Patriarch	Greece/Turkey	2/25
15	Dmir Kapu	Bulgaria/Greece	10/25
16	Albanian Minorities	Albania/Greece	12/25
17	Mosul Territory in Iraq	Turkey/United Kingdom	8/24
18	Cruiser "Salamis"	Greece/Germany	8/25
19	Bahrein Islands	Iran/United Kingdom	1/27
20	Gran Chaco I	Bolivia/Paraguay	12/28
21	Gran Chaco II	Bolivia/Paraguay	9/32
22	Rhodope Forest	Greece/Bulgaria	7/30
23	Manchuria	China/Japan	9/31
24	Bulgarian-Greek Debt	Bulgaria/Greece	0/31
25	Finnish Vessels	Finland/United Kingdom	9/31
26	Iraq-Syrian Frontier	France/United Kingdom	12/31
27	Letica	Peru/Columbia	9/32
28	Anglo-Persian Oil Co.	United Kingdom/Iran	12/32
29	Iraq Frontier	Iran/Iraq	12/34
30	Hungarian Frontier	Hungary/Yugoslavia	5/34
31	Marseilles Crimes	Hungary/Yugoslavia	12/34
32	Syria (Sanjak)	Turkey/France	12/36
33	Ethiopia	Ethiopia/Italy	3/35
34	China	China/Japan	8/37
35	Russo-Finnish War	Finland/U.S.S.R.	12/39

D. Two-Party Cases for the United Nations

Name of Case	Participants	Date of Introduction
1 Forces in Iran	Iran/U.S.S.R.	1/46
2 Thai Border	Thailand/France	5/46
3 Treatment of Indians	India/S. Africa	6/46
4 Corfu Channel	United Kingdom/Albania	1/47
5 Kashmir I	India/Pakistan	1/48
6 Intervention in China	China/U.S.S.R.	9/49
7 Threats to Yugoslavia	Yugoslavia/U.S.S.R.	11/51
8 Anglo-Iranian Oil Co.	United Kingdom/Iran	9/51
9 Morocco	Egypt/France	10/51
10 Forces in Burma	Burma/Nat. China	3/53
11 West Irian	Indonesia/Netherlands	8/54
12 Syria-Turkish Crisis	Syria/Turkey	10/57
13 Cambodian Border	Cambodia/Thailand	11/58
14 Sudanese Border	Sudan/Egypt	2/58
15 Tunisian Border	Tunisia/France	2/59
16 Laos Intervention	Laos/N. Vietnam	8/59
17 Eichman Kidnapping	Argentina/Israel	6/60
18 South Tyrol	Austria/Italy	6/60
19 Cuban Complaint	Cuba/United States	7/60
20 U-2 Incident	United States/U.S.S.R.	4/60
21 Mauritania	Morocco/France	12/60
22 Cyprus	Greece/Turkey	12/63
23 Goa Invasion	Portugal/India	12/61
24 Venezuelan Boundary	Venezuela/United Kingdom	8/62
25 Senegal Border	Senegal/Portugal	3/63
26 Ethiopian Border	Ethiopia/Somalia	2/64
27 Panama	Panama/United States	1/64
28 Haiti/Dom. Rep.	Haiti/Dominican Republic	5/63
29 Cambodian Border	Cambodia/South Vietnam	4/64
30 Gibraltar	United Kingdom/Spain	8/65
31 Mercenaries in Angola	Rep. of Congo/Portugal	9/66

The Swing Ratio and Game Theory

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It is well known that in electoral systems where a single representative is elected from each district or constituency on the basis of a simple plurality, the proportion of seats in the legislature a party can expect to win is not equal to the proportion of the overall vote it receives. In a two-party system the minority party can expect to be under-represented, while the party attracting more than 50 per cent of the popular vote usually wins a higher proportion of seats than votes. One indicator of this tendency is the *swing ratio* s , the change in representation per change in popular vote from one election to the next. If r is the proportion of seats a party wins, and p the proportion of the popular vote it receives, $s = \Delta r / \Delta p$, a ratio which has been found to be more or less constant in any one political context as long as p does not deviate too much from the 40–60 per cent range. Predictive models of the relationship between r and p may allow for a degree of statistical fluctuation. Such models predict $\bar{r}$ as a function of p , where $\bar{r} = E(r)$, the expected value of r . Then the quantity of interest is the expected swing ratio, $\bar{s} = E(s) = d\bar{r}/dp$, evaluated at $p = 0.5$, i.e., where both parties have 50 per cent of the vote.

In Britain, it has been found¹ that $s \approx 3$; in the U.S.,² s ranges between 2.5 and 3.3 depending on the mode of calculation and also on the legislative body (House or Senate) being considered. For Quebec, data from 27 elections are best fitted by an s -value of 3.8. One suggestion, that $\bar{s} = 3.0$ is characteristic of all political contexts with this type of electoral system, has been formulated in terms of the 'cube law', $\bar{r} = p^3/[p^3 + (1-p)^3]$. Unfortunately, no large-scale systematic surveys have been made to test this hypothesis. Nevertheless, it seems clear that s ranges between 2 and 4 and not between 1.0 and 1.2, say, nor between 8 and 16.

The question which has not been satisfactorily answered, and to which this paper is addressed, is this: Why does s tend to take on values only in this particular range, independent of country

and time? Kendall and Stuart³ have shown that any sampling explanation based on the unevenness of distribution of party supporters among constituencies must involve quantitative assumptions which are just as difficult to explain as the swing ratio effect!⁴ March⁵ proposed a reasonable model based on interparty negotiations and intra-constituency pressures; but to quantify this model would require assumptions about the values of a number of parameters. Furthermore the behavior of this model would tend to be rather sensitive to the values chosen for these parameters.

In this paper we define a two-person zero-sum game which is perhaps the simplest possible abstraction of this type of electoral contest. The idea is to try to account for all or part of the swing ratio effect in terms of resource allocation strategies by the two competing parties. There are no parameters to specify, and, as we shall show, under asymptotically optimal strategies,⁶ $\bar{s} \approx 2$.

The Game

There are two players, I and II, with resources θ and $1-\theta$, respectively, where $0 < 1-\theta < \theta < 1$. There are n boxes and each player must allocate, secretly, his resources among the boxes. After this allocation is completed, the amount I put in each box is compared with the amount II put in, and whoever put more, wins the box. The payoff is the proportion of boxes won by I. Player I tries to maximize the minimum payoff he can expect for any strategy II may play; while II tries to minimize the maximum payoff expected for any strategy available to I.

The particular case of this game where $n=3$ was discussed by Owen,⁶ but only for θ fixed at 0.5. The general case has not been discussed previously.

This game does not have a saddle point in pure strategies, except for the trivial case $\theta/n > 1-\theta$. We prove this in the next section and show that

³ Kendall and Stuart, "The Law of the Cubic Proportion . . ."

¹ M. G. Kendall and A. Stuart, "The Law of the Cubic Proportion in Election Results," *British Journal of Sociology*, 1 (September, 1950) 183–197.

² J. G. March, "Party Legislative Representation as a Function of Election Results," *Public Opinion Quarterly*, 21 (Winter, 1957–58), 521–542.

⁴ A recent study which assumes the cube law is Henri Theil, "The Cube Law Revisited," *Journal of the American Statistical Association*, 65 (September, 1970) 1213–1219.

⁵ March, "Party Legislative Representation . . ."

⁶ G. Owen, *Game Theory* (Philadelphia: W. B. Saunders, 1968), pp. 88–93.

even as $n \rightarrow \infty$, i.e., for large numbers of constituencies, the game has no solution or approximate solution in pure strategies. Extending the game to allow mixed strategies, we show how to construct strategies depending on θ and n in a way which is asymptotically optimal as $n \rightarrow \infty$.

Following this analysis we devote a section to its interpretation in electoral terms.

Mathematical Analysis⁷

THEOREM 1. For any pure strategy available to II, player I has an opposing pure strategy producing a payoff of 1. For any pure strategy available to I, II has an opposing one producing a payoff less than or equal to $1 - (1 - \theta)/\theta + 1/n$.

Proof. Let $y = (y_1, \dots, y_n)$ be any pure strategy available to II; i.e., y_1 is allocated to the first box, y_2 to the second, etc. For $i = 1, \dots, n$, set $x_i = y_i + (2\theta - 1)/n$. Then $x = (x_1, \dots, x_n)$ is a strategy for I because

$$\sum_{i=1}^n x_i = \theta,$$

and the payoff for x against y is 1 since $x_i > y_i$ for $i = 1, \dots, n$.

Now let x be any pure strategy available to I. Without loss of generality we can assume

$$x_1 \leq x_2 \leq \dots \leq x_n. \quad (1)$$

Let m be such that

$$\sum_{i=1}^m x_i < 1 - \theta, \quad \sum_{i=1}^{m+1} x_i \geq 1 - \theta. \quad (2)$$

Clearly $m \geq 1$ unless $\theta/n \geq 1 - \theta$, in which case the theorem is trivially true. Otherwise set

$$y_i = \begin{cases} x_i + \left(1 - \theta - \sum_{j=1}^m x_j\right) / m, & \text{for } i = 1, \dots, m \\ 0, & \text{for } i = m + 1, \dots, n. \end{cases}$$

The payoff for x against $y = (y_1, \dots, y_n)$ is $1 - m/n$. An upper bound for this quantity can be found as follows: From (1) and the second inequality of (2), we have

$$x_{m+1} \geq \frac{1 - \theta}{m + 1}.$$

⁷ We would like to thank Professors Mark Kac, Jean-Jacques Moreau, and Frank Stenger for advice, and Professor Guillermo Owen for information about this type of game. As far as possible we shall keep to the terminology and notation of Owen, *Game Theory*.

By (1), this inequality must also be satisfied by each of $x_{m+2}, \dots, x_n$. Thus

$$[n - (m + 1)] \frac{1 - \theta}{m + 1} \leq \sum_{i=m+2}^n x_i. \quad (3)$$

But from (2),

$$\sum_{i=m+2}^n x_i \leq \theta - (1 - \theta). \quad (4)$$

Inequalities (3) and (4) together yield

$$n \frac{1 - \theta}{m + 1} - (1 - \theta) \leq \theta - (1 - \theta),$$

$$1 - m/n \leq 1 - (1 - \theta)/\theta + 1/n.$$

This theorem shows that the game with pure strategies has no saddle point (except for trivial cases) and that when this game is played with many boxes, it is as far away from having a solution as when it is played with a few.

In contrast to this, we show in Theorem 2 how to construct a pair of *mixed* strategies which come within $3/2n$ of constituting a solution. For large n , such as encountered in electoral systems (say 100–1000), these strategies specify, for all practical purposes, a value for the game. Before proving this theorem we require some definitions.

For pure strategies x versus y the payoff to I will be denoted

$$A(x, y) = (1/n) \sum_{i=1}^n \chi(x_i, y_i),$$

where

$$\chi(a, b) = \begin{cases} 1, & \text{if } a > b \\ \frac{1}{2}, & \text{if } a = b \\ 0, & \text{if } a < b. \end{cases} \quad (5)$$

A mixed strategy for a player may be considered as a probability distribution over all the strategies available to him. Thus we work with the set of cumulative distribution functions on the n -dimensional interval $U = [0, 1]^n$ which concentrate all their mass on the hyperplane defined by

$$\sum_{i=1}^n x_i = \theta$$

in the case of player I, or

$$\sum_{i=1}^n y_i = 1 - \theta$$

in the case of II. Since we are using mixed strategies, we are interested not just in the payoff, but in the *expected* payoff

$$E(F, G) = \int_U \int_U A(x, y) dF(x) dG(y) \quad (6)$$

where I plays mixed strategy F and II plays G . If G assigns probability 1 to the pure strategy y

$$\begin{aligned} E(F, G) &= E(F, y) \\ &= \int_U A(x, y) dF(x) \end{aligned}$$

and an analogous remark may be made about $E(x, G)$. The values of the game⁸ for I and II are v_I and v_{II} respectively where

$$\begin{aligned} \sup_{F \text{ } y} \inf E(F, y) &= v_I \\ &\leq v_{II} = \inf_G \sup_x E(x, G). \end{aligned}$$

THEOREM 2.

$$v_I + 1/2n \geq (3\theta - 1)/2\theta \geq v_{II} - 1/n.$$

Proof. Let F be the strategy for I which assigns probability $1/n!$ to each permutation of the vector $(u, 3u, \dots, [2n-1]u)$ where $u = \theta/n^2$. Let H be any mixed strategy for II. From (5) and (6)

$$\begin{aligned} E(F, H) &= (1/n) \int_U \int_U \sum_{i=1}^n \chi(x_i, y_i) dF(x) dH(y) \quad (7) \\ &= (1/n) \int_U \sum_{i=1}^n \sum_{j=1}^n (n-1)! \\ &\quad \cdot \chi([2j-1]u, y_i) / n! dH(y) \end{aligned}$$

since F assigns probability $1/n!$ to $n!$ different vectors, $(n-1)!$ of which have $[2j-1]u$ as the i -th coordinate, for each $j=1, \dots, n$. Then

$$\begin{aligned} E(F, H) &= (1/n^2) \int_U \sum_{i=1}^n \sum_{j=1}^n \chi([2j-1]u, y_i) dH(y) \quad (8) \\ &\geq (1/n^2) \int_U \sum_{i=1}^n (n - (y_i + u)/2u) dH(y), \quad (9) \end{aligned}$$

⁸ Optimal strategies exist if

$$\max_F \min_y E(F, y) = v = \min_G \max_x E(x, G),$$

in which case v is called the value of the game.

since $\chi([2j-1]u, y_i) = 1$ except when $y_i \geq [2j-1]u$; i.e., except for $j=1, \dots, r$ where r is the largest integer in $(y_i + u)/2u$. For H to be a strategy for II, it must concentrate all its mass on the hyperplane

$$\sum_{i=1}^n y_i = 1 - \theta,$$

where the integrand in (9) becomes $n^2 - (1 - \theta)/2u - n/2$. Therefore

$$\begin{aligned} E(F, H) &\geq (1 - (1 - \theta)/2n^2u - 1/2n) \int_U dH(y) \quad (10) \\ &= (3\theta - 1)/2\theta - 1/2n, \end{aligned}$$

which is a lower bound for v_I , since H was chosen arbitrarily.

Similarly, let G assign probability $(n-m)!/n!$ to each distinct permutation of $(w, 3w, \dots, [2m-1]w, 0, \dots, 0)$ where $w = (1 - \theta)/m^2$ and m is the largest integer in $n(1 - \theta)/\theta$. Let H be any mixed strategy for I. As in (7)-(10),

$$\begin{aligned} E(H, G) &= (1/n^2) \int_U \sum_{i=1}^n \left[\sum_{j=1}^m \chi(x_i, [2j-1]w) \right. \\ &\quad \left. + \sum_{j=m+1}^n \chi(x_i, 0) \right] dH(x) \\ &\leq (1/n^2) \int_U \sum_{i=1}^n [(x_i + w)/2w + n - m] dH(x) \\ &= 1 + m[2n - m\theta/(1 - \theta)]/2n^2 + 1/2n. \end{aligned}$$

Using the fact that $n(1 - \theta)/\theta - 1 < m \leq n(1 - \theta)/\theta$, it follows that

$$E(H, G) \leq (3\theta - 1)/2\theta + 1/n,$$

and this completes the proof.

Electoral Interpretation

Theorem 2 shows that in a game with many boxes, if I and II are playing nearly optimal strategies (or optimal strategies if these exist), the average payoff will be approximately $(3\theta - 1)/2\theta$.

How can this game be related to electoral systems? The n boxes, of course, represent n constituencies, and the payoff represents the proportion of these constituencies won by the majority party. The simplest way to interpret the resource variable θ is to equate it to p , the proportion of the total popular vote won by the majority party. This leads immediately to the prediction $\bar{r} = (3p - 1)/2p$, and $\bar{r}$, evaluated at $p = 0.5$, is 2. Thus, if the parties in

a two-party system could freely allocate their total support among the constituencies, we should expect a swing ratio equal to 2.

This model is, of course, excessively simple. It would be preferable to relate the variable θ to the sum total of electoral resources: financial, organizational, press goodwill, etc. Some of these resources can be freely allocated; others, however, are bound to certain constituencies. If we were somehow able to identify θ with the freely allocatable resources, there would still remain the problem of predicting the vote in each constituency. Part of the resources bound to a particular constituency is the proportion of hard-core voters. A realistic model would have to combine⁹ the hard-core vote, which is relatively independent of free resources allocated, with the vote of the previously uncommitted constituents, which is more proportional to free resources allocated to the constituency.

⁹ Thiel, "The Cube Law Revisited," shows one way of doing this.

It is easy to see how such a model would have a higher swing ratio than the pure game-theory model. Assuming the hard-core vote is $(1-z)/2$ for both parties in most constituencies, the game would be played to capture a majority of the uncommitted vote in as many constituencies as possible. Then $\bar{r} = (3\theta - 1)/2\theta$ as before, but instead of $p = \theta$, we have $p = \theta z + (1-z)/2$, and hence $\bar{s} = 2/z$ when $\theta = p = 1/2$. So if one third of the voters, say, are hard-core, $\bar{s} = 3$.

Parameters other than z , perhaps related to differences between constituencies or to the turnout proportion, could also be added to the game. Some of these would tend, like z , to amplify the expected swing ratio, and others would tend to attenuate it. The important point about the game model is that it gives a reasonable basic value of $\bar{s}$ without the introduction of *any* parameters. It is a very simple model. Nevertheless, we feel that it summarizes the strategic problem faced by parties in this type of electoral system and that the asymptotic solution captures the most important component of the swing ratio effect.

The Strategy of Ambiguity: Uncertainty and Electoral Competition*

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Nicholas Biddle, the manager of William Henry Harrison's campaign for the presidency, is reported to have given his candidate the following advice:

Let him say not one single word about his principles, or his creed—let him say nothing—promise nothing. Let no Committee, no convention—no town meeting ever extract from him a single word, about what he thinks now, or what he will do hereafter. Let the use of pen and ink be wholly forbidden as if he were a mad poet in Bedlam.¹

More recently, presidential candidates have followed Biddle's seemingly ageless advice with ambiguous pronouncements on the salient political issues of the time.² In the 1968 presidential campaign, both Nixon's "I have a plan" statements on the Vietnam issue and Humphrey's "law and order with justice" slogan on "the social issue" suggest that the equivocal pronouncement during the course of a campaign is a common and recurrent theme in American electoral politics. Sartori places this phenomenon in a still broader context:

In his pungent essay, *Politics and the English Language*, George Orwell pointed out that 'the great enemy of clear language is insincerity' and that there is therefore a 'special connection between politics and the debasement of language [Orwell, *Selected Essays*, (London, 1957), pp. 152, 154, 157].' Ordinary political language is insincere language, for it is less an instrument for expressing thought than a means 'for concealing or

preventing thought.' Actually, except in the case of some intellectuals, political discourse is more often than not either a device for achieving and maintaining power, or a device for stimulating action . . . I do not wish to sound cynical. I am only trying to point out the special difficulties that a student of politics is bound to encounter. *For he has to work in a field where it may be considered useful to becloud problems, and definitely not useful to clarify them. Indeed, an important part of politics may be described as the art of confusing political issues (and this is not necessarily the wicked part).*³

That candidates are seen as vague and ambiguous in their campaigning and that the observed ambiguity typically involves precisely those issues on which the election hinges may discourage the traditional democrat. The ballot, a fundamental feature of democratic government, presumably provides an opportunity for the citizen to register his preferences on the important issues facing the collectivity. Citizens, however, vote for candidates, not policy alternatives. Ambiguous campaigning, reasons the traditional democrat, therefore compromises the "ballot box principle" by preventing citizens from identifying candidates with policy positions.

The aim of this paper is to explicate the role of ambiguity in electoral competition. I examine the effects of uncertainty on the decisions of voters and the strategy choices of candidates. The underlying theme here is the basic incompatibility of the hopes of the traditional democrat on the one hand and the desires of the politician on the other. It is argued that the rules of the democratic game often conflict with underlying normative principles—that incentives to equivocate are perceived by candidates for office, much to the consternation of the "responsible democrat."

A second aim of this paper is the introduction of mathematical decision theory and its integration with formal models of political phenomena. Choice, it is maintained, is the basic social act that transforms the essentially private thoughts and values of individuals into "public activity." And since "public activities" comprise the set of observables that concerns the behavioral scientist, a theory of choice—decision theory—is a most appropriate tool of analysis and theory building. In particular, a theory of decision making in contingencies of risk and uncertainty permits us to consider rational actors who are constrained

* Giovanni Sartori, *Democratic Theory* (Detroit: Wayne State University Press 1962), p. 207 (emphasis added).

* Several closely related papers including the one presented here have benefitted from close readings and careful criticism by a number of people. For their efforts I acknowledge and thank Richard Niemi, Alvin Rabushka, William Riker, John Sprague, and Herbert Weisberg, as well as the anonymous referees of this paper. A Washington University Faculty Summer Research Grant provided time to prepare and revise various drafts of this paper.

¹ Nicholas Biddle, *Correspondence*, R. C. McGrane, ed. (Boston: Houghton, Mifflin, 1919), p. 256. Cited in Arthur M. Schlesinger, Jr., *The Age of Jackson* (Boston: Little, Brown, 1945), p. 211. I thank Professor Robert Lineberry for bringing this citation to my attention.

² For example, William Leuchtenberg reports the following remarks by a Harding advisor during the 1920 presidential campaign: "From his deathbed, Boies Penrose the Pennsylvania machine politician advised: 'Keep Warren at home. Don't let him make speeches. If he goes out on tour somebody's sure to ask him questions, and Warren's just the sort of damned fool that will try to answer them.'" William E. Leuchtenberg, *The Perils of Prosperity, 1914-1932* (Chicago: University of Chicago Press, 1958), p. 88. I thank Risé S. Shepsle for this citation.

in their choice behavior by environmental limitations on information as well as by their preference structures.

Theoretical Precursors

A concern with ambiguity in electoral competition is not novel. Various students of politics have acknowledged the manifestations and consequences of ambiguity, minimally with guarded pessimism, and more often with outright anxiety. "Why," ask Pendleton Herring, to cite an example,

cannot there be more direction and less confusion in our political life? Surveying the history of our parties, we find discord among their members and frequent evasion in their platforms. Our parties have generally side-stepped the issues that have divided the nation most deeply.⁴

Or, to cite another example, V. O. Key, Jr., after observing that "... politicians prefer to be sure that if they act they will not find themselves far out on a limb,"⁵ suggests that owing to the unpredictable reactions of the electorate to volatile, salient issues, politicians become "addicted to equivocation and ambiguity."⁶

For yet another example, one need only peruse the pages of Downs's *An Economic Theory of Democracy*. There he argues—and this argument is examined carefully below—that on the "critical issues" parties perceive incentives to equivocate, to "becloud their policies in a fog of ambiguity."⁷ The disturbing consequence of this kind of behavior, Downs claims, is that parties are led to appeal to the irrational in the electorate. The "fundamental tension" in his model—the conclusion that "rational behavior by political parties tends to discourage rational behavior by voters"⁸—produces difficulties for a governmental arrangement in which the governors are to be responsive to the wishes and preferences of the governed.

As these examples suggest, the strategy of ambiguity in electoral competition has been a source of empirical interest and normative concern for a wide range of scholars.⁹ To those we have cited above, one might add a host of econo-

mists and management scientists whose interests include the behavior of firms, consumers, and investment markets in an uncertain world.¹⁰ The ambiguity of signals, whether its source is the platform and pronouncements of a political party, the pricing and production decisions of a firm, or the ever variable movements of the Dow-Jones average, pervades many aspects of social life.

Scholarly attention to the nature and implications of ambiguity and, more generally, of uncertainty is widespread.¹¹ But with the notable exception of Downs, little serious, systematic attention has been devoted to its impact and consequences in the political realm.¹² This paper examines one kind of uncertainty in the electoral context. The theory proposed below is formal, in effect a slight mathematical generalization of Downs's model of party competition. Its central feature is the exploitation of formal theories of decision making under risk.

The Theory of Ambiguity: A Generalization of Downs

In the Downsian model, it will be recalled, there are two kinds of actors—citizens and candidates for office—each motivated by different sets of incentives. The Downsian citizen has preferences about issues of public policy and uses his

¹⁰ Three monographs provide a sampling of this literature. See Karl Henrik Borch, *The Economics of Uncertainty* (Princeton: Princeton University Press, 1968); Howard Raiffa, *Decision Analysis* (Reading, Mass.: Addison-Wesley, 1968); and Kenneth J. Arrow, *Essays on the Theory of Risk-Bearing* (Chicago: Markham, 1971).

¹¹ The importance of these inquiries is suggested in a passage from a paper by Arrow:

There is no need to enlarge upon the importance of a realistic theory explaining how individuals choose among alternate courses of action when the consequences of their actions are incompletely known to them. It is no exaggeration to say that every choice made by human beings would meet this description if attention were paid to the ultimate implications. Risk and the human reactions to it have been called upon to explain everything from the purchase of chances in a "numbers" game to the capitalist structure of our economy; according to Professor Frank Knight, even human consciousness itself would disappear in the absence of uncertainty.

See Kenneth J. Arrow, "Alternative Approaches to the Theory of Choice in Risk-Taking Situations," *Econometrica*, 19 (October 1951), 404. Also see Frank H. Knight, *Risk, Uncertainty, and Profit* (New York: Houghton-Mifflin, 1921).

¹² This statement needs some qualification if the specialized literature in the field of constitutional choice—essentially the social choice of a social decision process under conditions of uncertainty—is included. See, for example, James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962); and Douglas W. Rae, "Decision Rules and Individual Values in Constitutional Choice," *American Political Science Review*, 63 (March 1969), 40–56.

⁴ Pendleton Herring, *The Politics of Democracy: American Parties in Action*, 2nd ed. (New York: Norton, 1965), p. 188.

⁵ V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1961), p. 424.

⁶ V. O. Key, Jr., *Politics, Parties and Pressure Groups*, 4th ed. (New York: Crowell, 1958), p. 241.

⁷ Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 136.

⁸ Downs, p. 136.

⁹ For an effort to explicate the role of ambiguity in the context of administrative decision making, a paper recently brought to my attention is Julian Wolpert's "Consequences of Inadvertent and Purposeful Ambiguity in Locational Strategies," presented at the 65th Annual Meeting of the American Political Science Association, New York 1969.

resources (or chooses not to) to affect the direction of government policy. Citizens are assumed to be rational in the sense that (1) they are capable of making choices whenever alternatives are placed before them (connectivity),¹³ and (2) the choices they make possess a certain modicum of consistency (transitivity).¹⁴ In effect, then, each citizen in Downs's model is represented by a *preference ordering* defined over public policy issues.

The typical candidate of the Downsian model polity is an office-seeker. His behavior is determined solely by the effect it is likely to have on his quest for office. "Politicians in our model are motivated by the desire for power, prestige, and income . . . their primary objective is to be elected."¹⁵ A candidate's own policy preferences are of marginal importance since "politicians . . . never seek office as a means of carrying out particular policies; their only goal is to reap the rewards of holding office *per se*."¹⁶

The mechanism that captures the dynamic of electoral competition is the well-known spatial model. Since electoral outcomes depend upon the aggregation of voter choices and since individual voter choices depend upon individual preferences, candidates for office choose policy positions and utter policy-related statements with an eye to voter preferences and the likely aggregate of voter choices. Stokes characterizes Downs's one-dimensional version of the spatial model well:

The root idea of Downs's model is that the alternatives of government action on which political controversy is focused can be located in a one-dimensional space, along a left-right scale. At least for illustration, Downs interprets this dimension as the degree of government intervention in the economy . . . Each voter can be located on the scale according to how much government control he wants and each party according to how much government control it advocates.¹⁷

Each voter is characterized by a point on the one-dimensional space (his most-preferred point) and by a preference metric which relates "distance" from his most-preferred point to preference. Each candidate for office is simply a single point in the space. With appropriate assumptions (see

¹³ If alternatives A and B are on the agenda, then each citizen either prefers A to B, B to A, or is indifferent between them.

¹⁴ Whereas connectivity is a dyadic property, transitivity imposes constraints on triads of alternatives. Thus, for the dyad (A, B), if a citizen prefers A to B or is indifferent; if, for the dyad (B, C), he prefers B to C or is indifferent; then it cannot be the case that he prefers C to A when the dyad (A, C) is offered. That is, for (A, B, C), $A \geq B$ and $B \geq C \rightarrow \sim (C > A)$, where $\geq$ is the symbol for preference or indifference.

¹⁵ Downs, *Economic Theory of Democracy*, p. 30.

¹⁶ Downs, p. 28.

¹⁷ Donald E. Stokes, "Spatial Models of Party Competition," *American Political Science Review*, 57 (June 1963), 368-377 (quoted at p. 368).

below) about the preference metric the voter casts his vote (if he votes at all) for the candidate who locates himself "closest" to the voter's most-preferred point.

In order to generalize Downs's model for the purposes of examining the effects of uncertainty, it is useful to begin with an explicit recognition of the assumptions required. Typically, spatial models begin with assumptions about the space of available policy alternatives, the nature of preferences defined on the space, and motives for choice. For the purposes of this paper the following assumptions are made:

(boundedness and A.1: The space of alternatives is unidimensionality) a bounded continuum, [A, B].

(single-peakedness) A.2: Each v_i in the set of citizens $V = \{v_1, v_2, \dots, v_n\}$ possesses a continuous, differentiable (at all but at most a finite number of points), single-peaked, von Neumann-Morgenstern preference function $u_i(x)$ ($x \in [A, B]$), consistent with A.3.

(quasisymmetry) A.3: The metric "Euclidean distance from most preferred point" induces a preference relation such that

$$\begin{array}{c} > \\ \rho(y, x_i) - \rho(z, x_i) \Rightarrow \\ < \\ < \\ u_i(y) - u_i(z), \\ > \end{array}$$

where ρ is the Euclidean distance metric and x_i is the most-preferred point of v_i .

(utility maximization) A.4: Citizens are nonstrategic (expected) utility maximizers.

(universal participation) A.5: All citizens vote.

(entrepreneurial incentives) A.6: Political entrepreneurs employ lexicographic criteria to guide their behavior: they are primarily concerned with winning office; a secondary concern is maximizing votes.

The first assumption is, in a sense, restrictive but does not detract from the major points to be made below. The restrictive feature of this assumption is, of course, unidimensionality. Elections typically involve a myriad of issues for which A.1 and A.2 are not compatible. For the purposes of making several points about the utility of ambiguous strategies, however, it is convenient to assume unidimensionality.¹⁸ The boundedness

¹⁸ However, one is not required to suppose this dimension is ideological in nature, as Downs does. The prospects for relaxing this assumption are ex-

assumption adds no difficulties and is theoretically quite general.¹⁹ In particular, it permits without loss of generality the consideration of that subinterval of the policy continuum found between the "extreme voters" most preferred points in either direction.

Assumption A.2—single-peakedness—requires that all voters possess a "bliss point" (most preferred point), with preference or utility decreasing for points increasingly distant from that point.²⁰ Assumption A.3 specifies the relationship between distance and preference, namely that Euclidean distance from most preferred point and preference are negatively correlated. In combination, then, A.2 and A.3 require a set of voters with single-peaked utility functions, for some particular ordering of the alternatives, such that the Euclidean distance metric provides a measure of preference. An electorate conforming to A.2 and A.3 is called an *electoral profile*.²¹

Assumption A.4 requires little comment except the emphasis of the word "nonstrategic." It is assumed that the election contest involves individual citizens comparing the alternatives available and deciding on how to use their vote by appealing to their individual preferences. Neither cooperative-collusive behavior nor "sophisticated" voting is considered. That is, the Downsian model, and ours as well, is equivalent to a two-person zero-sum game played by the competing candidates for office. The voters are *not*

tremely good, as has been indicated by the multidimensional generalizations of Downs in the research of scholars at Carnegie-Mellon University. For a summary of this work, see Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June 1970), 426-448; and Melvin Hinich, John Ledyard, and Peter Ordeshook, "A Theory of Electoral Equilibrium: A Spatial Analysis Based on the Theory of Games" (Carnegie-Mellon University, 1971, unpublished manuscript).

¹⁹In fact, it may be shown that the competitive situation in an unbounded space may be transformed to a bounded space with no effect on strategic considerations. See Kenneth A. Shepsle, *Essays on Risky Choice in Electoral Competition* (unpublished doctoral dissertation, University of Rochester, 1970), Chap. 4. Also see Robert G. Samberg, "Collective Decision-Making in a Bounded Prospect Space," a paper delivered at the Sixty-sixth Annual Meeting of the American Political Science Association, Los Angeles, 1970.

²⁰For early discussion of the single-peakedness concept, see Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958). For a generalization of single-peakedness, see Amartya Sen, "A Possibility Theorem on Majority Decisions," *Econometrica*, 34 (April 1966), 491-499.

²¹The term is adapted from Benjamin Ward, "Majority Voting and Alternative Forms of Public Enterprise," in Julius Margolis, ed., *The Public Economy of Urban Communities* (Baltimore: The Johns Hopkins University Press, 1965), pp. 112-126.

players in the game; rather, they serve a more mechanistic role, providing signals and structuring the payoff function for the "actual" participants—the candidates for office.²²

That nonvoting has important implications for entrepreneurial behavior, in both the strategic sense (e.g., policy pronouncements) and the tactical sense (e.g., canvassing and other precinct-level activities) has been suggested by Garvey, Hinich, Kramer, Ordeshook, and others.²³ As their research has shown, however, abstention is an incredibly complex phenomenon to model. We avoid these complexities by postulating A.5: universal participation. Our results must be interpreted with this restriction in mind.

Finally, we assume that political entrepreneurs are office seekers and choose electoral strategies with this goal in mind. Among those behaviors which are perceived as equivalent in terms of office seeking, a secondary criterion, vote maximization, is employed. Plurality maximization might have been employed as the secondary criterion instead since, by A.5, vote maximization and plurality maximization are equivalent. In any event, assumption A.6 renders votes the only political currency of interest to the candidates for office.²⁴

²²For an examination of the dynamics of building a majority coalition, and a less "mechanistic" treatment of the voter, see Steven Brams, "Positive Coalition Theory: The Relationship Between Postulated Goals and Derived Behavior," in Cornelius P. Cotter et al., eds., *Political Science Annual: Conflict, Competition, and Coalitions*, IV (Indianapolis: Bobbs-Merrill, 1972), forthcoming. "Sophisticated voting" as treated in Robin Farquharson, *Theory of Voting* (New Haven: Yale University Press, 1969) is also not considered here. Voters consult only their own preference schedules in making their voting choice. They do not engage in speculation or calculation about the behavior of other voters. In the context of two-candidate elections (which is all we consider in this paper), with single-peaked preference functions and universal participation (A.5), "sophisticated voting" is not crucial. For electoral contingencies in which it is important, see, in addition to Farquharson, Gerald H. Kramer, "Sophisticated Voting Over Multidimensional Choice Spaces," a paper delivered at the Meetings of the Public Choice Society, Blacksburg, Virginia, 1971.

²³Gerald Garvey, "The Theory of Party Equilibrium," *American Political Science Review*, 60 (March 1966), 29-38; Melvin Hinich and Peter Ordeshook, "Abstentions and Equilibrium in the Electoral Process," *Public Choice*, 7 (Fall 1969), 81-106; Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in Joseph L. Bernd, ed., *Mathematical Applications in Political Science*, II (Dallas: Southern Methodist University Press, 1966), pp. 137-160.

²⁴We do not need to be more explicit in stating this assumption since, in the remainder of this paper, we shall be concerned only with *winning* strategies—those strategies that are "dominant" in a sense to be specified below.

Assumptions A.1 through A.6 define a variant of Downs's model of party competition and form the basis for generalization below. Other assumptions that may arise are made explicit wherever practical.

Although it has been subject to a good deal of criticism, Downs's model has survived more than a decade as a basis for the formal examination of electoral processes and governmental decision making. Moreover, it has generated a great deal of research, some of which has been cited, that attempts to remove weaknesses and relax excessively restrictive formulations. Our essay follows this pattern. We examine a structural feature of the environment—the *risk environment*—and its implications for voter and candidate decision making.

Given A.1–A.6, suppose that candidates for office are represented not by points in $[A, B]$ but rather by *probability distributions* over points in $[A, B]$. That is, candidates are perceived by voters as *lottery tickets* over policy alternatives, and voters (like gamblers) must choose between uncertain prospects.²⁵ Candidates, on the other hand, must choose their electoral strategies from the set of all probability distributions over $[A, B]$ i.e., from the set $P[A, B]$.

It is quite clear that our conceptualization of candidates as probability distributions over $[A, B]$ effects a simple mathematical generalization of Downs's original treatment since any point $x \in [A, B]$ is a degenerate probability distribution. That is, the new strategy set available to candidates includes the original one proposed by Downs: $[A, B] \subset P[A, B]$. However, not only is there mathematical generality afforded by this con-

ceptualization; additional substantive scope is purchased as well.

First, we can accept with Downs the assumption that politicians do not lie—that false information does not enter the communications system—while still acknowledging the politician's advantage in speaking “half truths” and in varying his appeals with variations in audience and political climate. Our new arrangement permits a device for capturing these incentives to equivocate and provides a method (see below) for specifying the electoral contingencies in which they are likely to be found.

A second justification for this peculiar treatment of electoral competition follows from the non-teamlike behavior of political parties. Downs chose to treat the competition for office as a contest between *teams* of activists. The internal workings of each team are assumed to involve complete cooperation among individuals with identical preferences so that the team may be treated as a unified entity. The simplification effected, however, ignores the “deviant” behavior of the candidate whose official career depends on an electorate different from those of his team colleagues. This observation “underscores the limits to the empirical usefulness of models that assume that party leaders always act in common An awareness that party leaders (i.e., politicians) act for private motives ought to prompt an extension of models of rational behavior. . . .”²⁶ The lottery concept permits us to retain (if we wish) the team notion by tempering its unique, unambiguous identity.²⁷

Third, the introduction of uncertainty through our probabilistic representation of candidates permits the formal inclusion of limitations in voter information. Possessing neither complete nor perfect information, the voter is simply uncertain of the policies a candidate represents. At best the voter possesses enough information to develop some educated expectations. This limitation is captured by a probabilistic description of electoral possibilities.²⁸

²⁵ Decision theories ordinarily distinguish among contingencies of certainty, risk, and uncertainty. In the case of *certainty*, each action or choice is known to lead to a specific (set of) outcome(s). In contingencies of *risk*, each action is probabilistically related to outcomes in the sense that action a_i results in outcome o_j with probability p_{ij} . Thus, for the set of outcomes $O = \{o_1, o_2, \dots, o_n\}$, the i th action or choice is a *lottery ticket*: $a_i = (p_{i1} o_1, p_{i2} o_2, \dots, p_{in} o_n)$, where the usual probability constraints on the p_i 's apply. In the case of *uncertainty*, each action is probabilistically related to outcomes, but the nature of the relationship depends on an *unknown* parameter θ , labeled “the state of nature.” Thus, $a_i = p_{i1}^{(\theta)} o_1, p_{i2}^{(\theta)} o_2, \dots, p_{in}^{(\theta)} o_n$. We have argued elsewhere that the three-part distinction above is, for our purposes, somewhat artificial, and that we may conceive of all contingencies as falling under the rubric of risk—a known probability distribution over outcomes. We shall, however, often use the common parlance, referring (somewhat confusingly, we admit) to uncertainty. Let us reiterate that we shall always mean risk. See Kenneth A. Shepsle, “Parties, Voters, and the Risk Environment: A Mathematical Treatment of Electoral Competition Under Uncertainty,” in *Probability Models of Collective Decision-Making*, Richard G. Niemi and Herbert F. Weisberg, eds. (Columbus: Charles E. Merrill), forthcoming.

²⁶ Robert Schoenberger, “Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-Making in the 1964 Congressional Elections,” *American Political Science Review*, 63 (June 1969), 515–520 (quotation at pp. 519–520).

²⁷ In fact, throughout this essay we refer to competition among candidates rather than teams of activists.

²⁸ It should be noted here that, by virtue of A.4 and A.5, voters do not concern themselves, in a strategic fashion, with the effect of their vote on the aggregate outcome. Rather they treat the voting decision as one of individual decision making, maximizing *expected* utility. Thus, the voter's information about the intentions of other voters (an additional source of uncertainty) is not germane in this model. For an

Finally, the introduction of uncertainty via the lottery representation permits an explicit demarcation of the "scope of conflict." Far too frequently, spatial models of electoral competition permit *complete spatial mobility*.²⁹ When this is not the case, the restrictions on mobility tend to be somewhat arbitrary and unsystematic.³⁰ In this paper candidates and their strategic options are distinguished according to their electoral role. In particular, we distinguish between incumbent and challenging candidates. The incumbent (or representative of the incumbent party) has been the prime political mover during his period of incumbency. Not only is his visibility enhanced by the media, which transmit his messages (intended or otherwise) to the voter; in addition, he is in a position to communicate directly with the voters through his control of government activities which alter their utility streams.³¹ The challenger, on the other hand, is an advocate. Neither his image nor his intentions, we suppose, are well-known to the electorate on election day. We formalize these role distinctions with the following strategic *constraints* on the actors:

- (strategic constraints)
- C.1: The incumbent is restricted in his choice of electoral strategy to *degenerate* probability distributions. We write his available strategies as $S_I = \{x/x \in [A, B]\}$.
- C.2: The challenger is restricted in his choice of electoral strategy to *nondegenerate* probability distributions. We write his available strategies as $S_{II} = \{p(x)/x \in [A, B] \text{ and } p(x) \neq 1 \text{ for any } x\}$.

In addition, we add a further assumption about voters:

- (information consensus) A.7: All v_i
- (i) perceive the incumbent's position as a certainty;

initial examination of the voting decision in contingencies of strategic interaction among voters, see William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March 1968), 25-42.

²⁹ This is analogous to the assumption of free entry and exit in economic models. For an interesting point of view on this assumption, see Albert O. Hirschman, *Exit, Voice and Loyalty: Responses to Decline in Firms, Organizations, and States* (Cambridge: Harvard University Press, 1970).

³⁰ More recently efforts have been made to deal with this question in a more systematic fashion. See Peter Aranson and Peter Ordeshook, "Spatial Strategies for Sequential Elections," in Niemi and Weisberg, *Probability Models*; James S. Coleman, "The Positions of Political Parties in Elections," in Niemi and Weisberg; Otto A. Davis and Melvin J. Hinich, "Spatial Competition Under Constrained Choice," in Niemi and Weisberg.

³¹ That image, however, may be (instantaneously) altered by a dramatic act, e.g., a halt in the bombing of North Viet Nam in 1968.

- (ii) perceive the challenger as inherently risky; and
- (iii) agree about their perceptions, although they will, in general, differ on preferences.

With A.7, then, Stokes's criticism of a common perceptual space is acknowledged and disposed of by assumption.^{32,33}

Before tracing some of the logical implications of this model, the *risk environment* is formally specified. To do so, it is useful to examine one behavioral implication.

Voter Choice and the Risk Environment

Our argument to this point is that the act of voting, like that of gambling or purchasing insurance, is one involving "risky" alternatives. Or, to put the thesis somewhat differently, at least one (and possibly both for constraints other than C.1) of the alternatives (candidates) in the voter's choice set is inherently ambiguous. From the point of view of the candidate, the strategic problem at hand is that of selecting a (possibly risky) strategy from his set of permissible strategies, S_i , $i=I, II$. In effect, then, we have replaced the Downsian candidate of the certain world—a point on the policy continuum—with the risky candidate. Analytically, the risky candidate is a probability distribution or *lottery ticket* defined on $[A, B]$.

Given this representation the *risk environment* is identified by two characteristics. First, the nature of risky competition depends on the *shape of voter utility functions*. A voter's evaluation of a risky alternative—via the expected utility hypothesis (A.4)—depends on the functional form of his utility schedule.³⁴ To see this, consider three voters (Figure 1) each of whom prefers x to y and y to z (and x to z by transitivity). Further suppose y is the midpoint of the interval $[x, z]$. Note that although voters a , b and c have identical preference *orderings* over the alternatives, the *shapes* of their utility functions differ.

³² Stokes, "Spatial Models."

³³ Quite obviously, C.1 and C.2 are somewhat arbitrary. Moreover, in an historically dynamic world the ability of incumbents to appear equivocal renders C.1 overly restrictive. Attempts to relax them are given in Shepsle, *Essays* . . . , Chap. 3. For the purposes of this essay, however, they provide a well-specified electoral contingency suitable for analysis.

³⁴ Down's reliance on the *distribution of most-preferred points* in the electorate, and not on the functional form of utility schedules, prevented him from correctly specifying the possibilities of a strategy of ambiguity. For an early treatment of the importance of utility function form in decision making under risk and uncertainty, as well as some fascinating economic applications, see the classic paper by Milton Friedman and L. J. Savage, "The Utility Analysis of Choices Involving Risk," *Journal of Political Economy*, 56 (1948), 279-304.

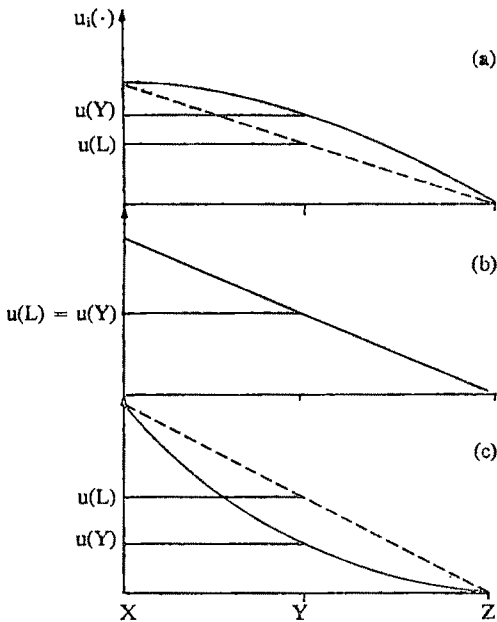


Figure 1. Voter utility functions.

Suppose now that these voters must choose between two policy alternatives:

- (i) y for certain, or
- (ii) $L = (1/2x, 1/2z)$, i.e., an ambiguous alternative assigning equal probability to x and z .

The utility of option (i) is determined directly from the utility functions displayed in Figure 1. The expected utility of L is $1/2 u_i(x) + 1/2 u_i(z)$ and is represented by the midpoint of the chord (dotted lines in Figure 1) connecting the points $(x, u(x))$ and $(z, u(z))$ on the graphs of the utility functions. As is demonstrated in Figure 1, voter a prefers (i) to (ii), b is indifferent between the two, and c prefers (ii) to (i). In short, *despite identical preference orderings, each voter reacts differently to the ambiguous alternative depending upon the shape of his preference function*. Generally, voters like a , who possess concave preference functions, prefer the expectation of a gamble for certain to the gamble itself and are thus said to be *risk averse*. Voters like b , with linear preference functions, are indifferent between a gamble and the certainty of its expectation and are called *risk neutral*. Finally, voters with convex utility functions are, like c , *risk acceptant*, preferring a gamble to its expectation.³⁵

The risk environment is characterized, secondly, by the sets of strategy constraints that govern candidate behavior. In particular, the distribu-

tional characteristics of available lottery strategies—domain and density—determine the viability of purposeful ambiguity. The domain of a given strategy, demarcating the region of the policy continuum over which the strategy is operative, is an important datum by virtue of our previous point on the functional form of preferences. If, as we shall see below, voters possess convex preferences over certain ranges of the policy continuum, and if the domain of strategies compatible with strategy constraints includes those same ranges, then ambiguity may be encouraged. In view of the central role it plays in the expected utility calculation, the probability density of any available strategies is also important.

Given these two characteristics, as well as the assumption of expected utility maximization and a well-defined electoral profile, the risk environment provides a suitable framework for examining electoral dynamics under uncertainty. Voters in our "game" somewhat mechanistically choose among the competing alternatives, some of which may be ambiguous, so as to maximize expected utility. *Candidates, on the other hand, may employ the risk environment (subject to strategy constraints) to their electoral advantage.*

Candidate Strategy Selection: Some Theoretical Implications³⁶

Our task in this section is to derive some implications of A.1–A.7 and C.1–C.2 for candidate strategy selection and success in elections in which an incumbent candidate faces a challenger. With this arrangement,³⁷ in which candidate strat-

the midpoint of $[x, z]$. The three voter types may be defined mathematically:

If	then
$u''(x) < 0$ (concavity)	$u(\bar{p}) > \sum u(x)p(x)$ (risk-averse)
$u''(x) = 0$ (linearity)	$u(\bar{p}) = \sum u(x)p(x)$ (risk-neutral)
$u''(x) > 0$ (convexity)	$u(\bar{p}) < \sum u(x)p(x)$ (risk-acceptant)

³⁵ For further results see Kenneth A. Shepsle, "Parties, Voters, and the Risk Environment," *passim*.

³⁷ Two other arrangements are possible in two-party competition. The first, in which two incumbents face each other (by virtue, say, of redistricting so that two elected officials, formerly representing distinct constituencies, now must compete with each other to represent a single constituency), is simply the original Downsian model in which constraint C.1 applies to both candidates. The second, in which two challengers face each other, is somewhat more complex. In this case, which might occur when an incumbent does not seek re-election or fails to obtain his party's nomination, and when his replacement is not considered his hand-picked successor, both candidates are governed by constraint C.2. This case is treated briefly in Shepsle, "Parties . . ." In a future paper, I expect to examine this case in greater detail.

³⁶ The expectation of a gamble $p(x)$, $\bar{p}$, is the expected value (not to be confused with the expected utility) of that gamble: $\bar{p} = \sum xp(x)$ for discrete probability distributions and $\bar{p} = \int xp(x)dx$ for continuous distributions. In the example above y is the expectation of $(1/2 x, 1/2 z)$ by construction, i.e., it is

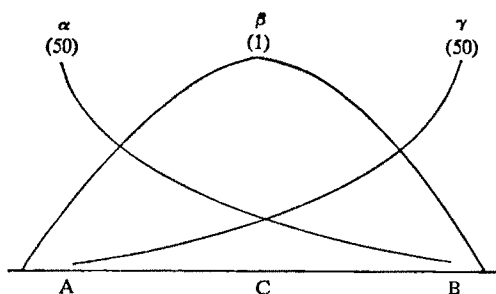


Figure 2. An electoral profile.

egy sets are defined by C.1 and C.2, respectively, it is of interest to identify the kinds of *electoral profiles* for which an electoral strategy in one candidate's strategy set is preferred by a majority of the voters to *all* strategies in his opponent's strategy set. We call an electoral strategy (or set of strategies) with this characteristic *winning*, and the contingency an *equilibrium situation*.³⁸

It will be useful in this discussion to have a specific case in mind, although the results below are not restricted to this case. Suppose, then, that an electorate of 101 voters is to choose between two candidates according to their policy positions in $[A, B]$. The electoral profile is provided in Figure 2. Fifty voters possess type α preference functions, fifty type γ , and the single remaining voter type β . Assume that C is the midpoint of $[A, B]$.

In earlier versions of the spatial model under contingencies of *certainty*, the well-known result of Duncan Black obtains:

BLACK'S THEOREM: For an electorate composed of n voters (assume n odd), each of which has a single-peaked preference function arrayed along the issue dimension from most extreme in one direction to most extreme in the other, the policy position represented by the median most-preferred point

³⁸ The *nonuniqueness* of winning strategies poses no theoretical problems. The candidate simply chooses from among those strategies the one which maximizes votes (A.6). If two or more winning strategies are vote-maximizing, then any arbitrary standard may be employed for final selection. The question of *mathematical existence*, however, raises interesting theoretical possibilities to be explored below. Several observations are appropriate here. First, it is obvious that *both* candidates cannot possess winning strategies. Otherwise some majority would have to prefer $x^* \in S_I$ (the incumbent's alleged winning strategy), to all $y \in S_{II}$, on the one hand, while some other majority prefers $y^* \in S_{II}$ (the challenger's alleged winning strategy) to all $x \in S_I$, on the other. But this condition is contradictory, since any two majorities contain at least one person in common. Second, it is possible that *neither* candidate possesses a winning strategy. In this case, as in two-person zero-sum games without pure strategy saddle points, a problem in conjectural variation arises. Although there are optimal mixed strategies (technically, e -

receives at least a simple majority against every other point, and it is the only point that can do so.³⁹

That is, the preferred point of the lone type β voter—the median most-preferred point—is the predicted position of the winning candidate. In fact, the median most-preferred point is a *winning strategy* in spatial competition under *certainty* governed by A.1–A.6 and C.1 (but not C.2).

The “drawing power” of the median is one of the more frequent and recurring themes of the spatial modeling literature. It is not difficult to show, however, for the electoral profile of Figure 2 and strategy constraints C.1 and C.2, that once uncertainty is permitted the median is no longer winning. Suppose, for example, that the incumbent governed by C.1, chooses the median most-preferred point (the point C in Figure 2). If the challenging candidate chooses the strategy:

$$p(x) = 1/2 \quad \text{for } x = A \quad \text{or } x = B \\ = 0 \quad \text{otherwise}$$

a strategy consistent with C.2 then he will win a majority of votes. That is, the *ambiguous* strategy $p(x)$ defeats x_{med} (the median most-preferred point). To see this, consider the individual calculation made by each voter type. The type β voter, of course, prefers the incumbent, who is located at his “bliss point,” to *any* lottery proposed by the challenger. The calculations of the type α and type γ voters, however, displayed in Figure 3, yield a different result. In Figure 3(a) the type α voters' evaluation of the risky candidate involves examining the midpoint of the chord connecting $(A, u(A))$ and $(B, u(B))$. Given the probability weights of $1/2$ on the points A and B , the expected utility of $p(x)$ is:

optimal since this is an infinite game), this will not be our concern. In this essay the concern is the set of conditions under which *winning strategies* exist. In the last section of the paper the “non-existence problem” is addressed. There the relationship between winning strategies and electoral equilibrium is considered. On the relationship between spatial models and the theory of infinite games, the reader may consult Shepsle, *Essays . . .*, Chap. 4; and Hinich, Ledyard, and Ordeshook, “A Theory of Electoral Equilibrium.” For an examination of social choice situations with ambiguous alternatives in which no winning strategy exists, see Richard Zeckhauser, “Majority Rule with Lotteries on Alternatives,” *Quarterly Journal of Economics*, 83 (November 1969), 696–703; and Kenneth A. Shepsle, “Uncertainty and Electoral Competition: The Search for Equilibria,” a paper presented at the Mathematical Collective Decisions Seminar, Hilton Head Island, South Carolina, August, 1971.

³⁹ This result is discussed in Black, *The Theory of Committees and Elections*, pp. 14–18. This discussion parallels that of Downs, *Economic Theory of Democracy*, Chap. 8.

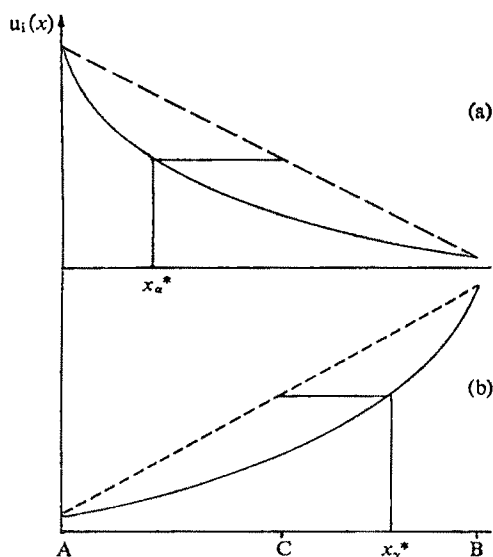


Figure 3. Lottery evaluations.

$$u_\alpha[p(x)] = 1/2u_\alpha(A) + 1/2u_\alpha(B),$$

which is the midpoint of the chord. But

$$u_\alpha[p(x)] = u_\alpha(x_\alpha^*),$$

where x_α^* is the *certainty equivalent* of the lottery $p(x)$,⁴⁰ and $u_\alpha(x_\alpha^*) > u_\alpha(C)$ by A.2 and A.3. Thus, by transitivity $u_\alpha[p(x)] > u_\alpha(C)$ and the fifty type α voters prefer the risky challenger. Similarly (see Figure 3(b)), the fifty type γ voters prefer the challenger. *The result is that the median is defeated by the vote of 100-1!* The important point, of course, is that Black's theorem does *not* generalize to all contingencies of uncertainty, and, moreover, that there may indeed be incentives for the rational candidate to "cloud his policies in a fog of ambiguity." Shortly this result is stated and proved formally. First, however, some theorems concerning conditions for which Black's theorem generalizes to contingencies of uncertainty are proved.

In essence, Black's theorem demonstrates that x_{med} defeats all *degenerate risks*, i.e., all certain alternatives. But it has even greater generality:

THEOREM 1: If *all* voters are risk averse (and hence possess concave preference functions in $[A, B]$), then x_{med} defeats any risk, degenerate or non-degenerate.

The proof is straightforward. We must show, for

⁴⁰ For technical definitions, see John W. Pratt, Howard Raiffa, and Robert Schlaifer, "The Foundations of Decision Under Uncertainty: An Elementary Exposition," *Journal of the American Statistical Association*, 59 (1964), 353-375.

any $p(x)$, that x_{med} is preferred to it by a majority. Consider three mutually exclusive, collectively exhaustive classes of $p(x)$

- (i) $E[p(x)] = \int_A^B xp(x)dx < x_{\text{med}}$
- (ii) $E[p(x)] > x_{\text{med}}$
- (iii) $E[p(x)] = x_{\text{med}}$

For the first class choose one of the voters v_i whose most-preferred point $x_i \geq x_{\text{med}}$. By the assumption of risk-aversion, $u[p(x)] < u(E[p(x)])$. But $E[p(x)] < x_{\text{med}}$ by construction, and, from A.2 and A.3,

$$u(E[p(x)]) < u(x_{\text{med}})$$

(since in this region v_i 's preference function is downward sloping to the left). By transitivity, then, $u[p(x)] < u(x_{\text{med}})$, and v_i prefers x_{med} to $p(x)$. But v_i is an arbitrarily selected voter from among those for whom $x_i \geq x_{\text{med}}$, and this collection of voters constitutes a majority by definition of the median. Therefore, a majority prefers x_{med} to any $p(x)$ in class (i). By analogous reasoning, a majority prefers x_{med} to any $p(x)$ in class (ii). To complete the proof, one need only observe that, since all voters are risk averse, all voters prefer the expectation $E[p(x)]$ to the risk, $p(x)$, itself, i.e., $u(E[p(x)]) > u[p(x)]$ for all v_i . In class (iii) $E[p(x)] = x_{\text{med}}$. Therefore, for all voters, $u(x_{\text{med}}) > u[p(x)]$ for $p(x)$ in class (iii). Thus, we have shown that the median is preferred to all $p(x)$, given the premise of the theorem.

In order to give serious attention to the substantive implications of Theorem 1, a meaningful political interpretation of risk aversion is required. While this would take us too far afield,⁴¹ several brief comments are appropriate. If by risk aversion one has in mind an individual preference structure characterized as low-keyed, i.e., an unwillingness by individuals to "take a chance" on achieving a more preferred outcome when one can "settle" for a middle-ranked alternative, then such a situation as is presumed in Theorem 1 yields a *winning strategy* for the incumbent. That is, an *equilibrium situation* obtains when the issue being contested in the election is one on which preferences are not intense.⁴² Thus, for electoral

⁴¹ What is required here is a serious discussion of the "units problem." This problem is usually avoided in the spatial modeling literature, but when the *shape* of preference functions is under consideration, it is particularly important to have the units of the strategy space specified. See Shepsle, "Uncertainty and Electoral Competition."

⁴² For a mathematical definition of intensity based on reactions to risk, see Alvin Rabushka and Kenneth A. Shepsle, *Politics in Plural Societies: A Theory of Democratic Instability* (Columbus: Charles E. Merrill, 1972), Chap. 2.

profiles involving nonintense preferences, there is a simple explanation for the electoral dominance of incumbent officials.⁴³

Closely related to the existence of a winning strategy is the importance of *commitment* in risk environments permitting ambiguity.⁴⁴ For the class of issues on which voters are risk averse, the incumbent need not discredit his opponent's position. He need only discredit the *certainly* of that position. Where the premises of Theorem 1 are satisfied, vagueness is as costly an electoral posture as nonoptimal policy selection.

Finally, it may be noted that the electorate found in the early work of Davis and Hinich conforms to the premises of Theorem 1.⁴⁵ Their electorate is composed of voters with quadratic loss functions which, in terms of preferences, yields concave utility functions. Thus, the implication of the theorem applies to their work as well.

The assumption that *all* voters in Theorem 1 are risk averse is quite restrictive. Any weakening of that assumption, however, requires a specification of the distribution of risk-averse voters in order to provide determinate results. Since we would like our theorems to be independent of distribution assumptions (which otherwise restrict the generality of the results), we finesse the problem with a less determinate result:

THEOREM 2: If a *majority* of voters is risk averse, then the challenger possesses no winning strategy.

Theorem 2 asserts that for any electorate with at least a simple majority of risk-averse voters, there exists no strategy in the challenger's strategy set, S_{II} , which dominates all strategies in the incumbent's strategy set, S_I . Alternatively, it states that for any $p(x) \in S_{II}$, there exists a $y \in S_I$ which is preferred by at least a simple majority.

The proof of this theorem follows directly from the definition of risk aversion. For any $p(x) \in S_{II}$

(recall that S_{II} is the set of strategies consistent with C.2), choose the point $y \in S_I$, where

$$y = \int_A^B x p(x) dx.$$

That is, y is the expectation of $p(x)$. Since $[A, B]$ is a bounded continuum, the point y always exists. For any risk-averse voter, say v_i ,

$$u_i[p(x)] < u_i[E[p(x)]]$$

by the definition of risk aversion. But

$$y = E[p(x)], \text{ by construction,}$$

so that

$$u_i(y) > u_i[p(x)].$$

Since v_i is an arbitrarily selected risk-averse voter, and since there are a majority of such voters by assumption, it follows that y defeats $p(x)$. Moreover, since $p(x)$ is any strategy in S_{II} , there exists some $y \in S_I$ that defeats *any* $p(x) \in S_{II}$. This completes the proof.

It should be emphasized here that Theorem 2 says nothing about the existence of a winning strategy for the incumbent. In fact, in the absence of assumptions about the distribution of risk-averse voters, it is impossible to say whether an equilibrium situation obtains. This is somewhat surprising in light of the interpretation ordinarily given to single peakedness, namely that there always exists a winning strategy.

To this point, then, Black's theorem has been generalized to some contingencies of uncertainty, re-emphasizing "the power and importance" of the median most-preferred point (Theorem 1). Moreover, under somewhat weaker conditions, it has been shown that constraint C.2 implies that S_{II} contains no winning strategy, thus placing the challenger at a strategic disadvantage. However, at the same time we have called into question the "existence" part of Black's theorem, suggesting that there may well be electoral profiles for which *no winning strategy exists for either candidate*. In the next section this point is pursued in more detail. Before addressing ourselves to that question, we prove one final result, a generalization of the example with which this section was introduced.

Theorems 1 and 2, as well as the commentary that followed, apply to risk-averse electoral profiles only. Once the restriction of risk-aversion is removed, an entirely different picture emerges:

THEOREM 3: If a *majority* of voters is risk acceptant in some interval $I \subseteq [A, B]$, containing x_{med} , then any $p(x)$ defined on I with expectation at

⁴³ As well, we may now assess the probable success of campaign rhetoric that encourages the electorate to resist changing horses in midstream: it is successful when voters are not risk-acceptant on the issue in question.

⁴⁴ On the importance of commitment in more general strategic situations, see Thomas C. Schelling, *The Strategy of Conflict* (New York: Oxford University Press, 1963), *passim*.

⁴⁵ Otto A. Davis and Melvin J. Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in *Mathematical Applications* . . . J. Bernd, ed., Vol. II, pp. 175-208; "Some Results Related to a Mathematical Model of Policy Formation in a Democratic Society," in Bernd, Vol. III, pp. 14-38; "On the Power and Importance of the Mean Preference in a Mathematical Model of Democratic Choice," *Public Choice*, 5 (Fall 1968), 59-72.

x_{med} defeats the median most-preferred point.

To see that Theorem 3 is true, choose a $p(x)$ over an interval $I \subseteq [A, B]$ such that

- (i) $x_{\text{med}} \in I$, and
- (ii) the expectation of $p(x)$ is x_{med} , i.e.,

$$E[p(x)] = \int_I x p(x) dx = x_{\text{med}}.$$

It is clear that one can choose a $p(x)$ in this fashion since a uniform probability distribution (over an interval containing x_{med} and symmetric about that point) satisfies (i) and (ii) above. Now choose a voter, v_i , who is risk-acceptant. By the definition of risk acceptance,

$$\int_I u_i(x) p(x) dx > u_i(E[p(x)]).$$

But, by construction, $E[p(x)] = x_{\text{med}}$. Thus,

$$\int_I u_i(x) p(x) dx > u_i(x_{\text{med}}).$$

That is, v_i prefers $p(x)$ to x_{med} . Since v_i is an arbitrarily selected risk-acceptant voter, and since a majority of voters is risk-acceptant, it follows that the last inequality must hold for at least a simple majority. Thus, $p(x)$ defeats x_{med} , and the theorem is proved.

A situation conforming to the premises of Theorem 3 is displayed in Figure 2. The type α and type γ voters—a majority in total—are risk acceptant over the entire interval $[A, B]$. Thus, there are strategies in S_{II} , one of which was given in the example, that defeat x_{med} .

Theorem 3 is significant. It should disabuse the reader of the universal superiority of the median. Elsewhere I have argued that the premises of the theorem provide a convenient definition of intensity of preference, i.e., a risk-acceptant voter is one willing to "take a chance" on obtaining his most-preferred alternative, even at some rather large risk of obtaining considerably worse alternatives, rather than to settle for a "middling" alternative.⁴⁶ With this interpretation, Theorem 3 asserts that the incumbent finds his position tenuous on those issues which polarize the electorate into

⁴⁶ See Kenneth A. Shepsle, "Intensity, Salience, and the Risk Environment: Some Theoretical Aspects of Conflict in the Plural Society," a paper presented at the Conference on Theories of Collective Behavior, Fels Center of Government, University of Pennsylvania, December, 1970. For an empirical application of this notion of intensity, see Rabushka and Shepsle, *Politics in Plural Societies*.

groups with intense, conflicting preferences. *It is precisely these kinds of "critical issues," then, which encourage deliberate ambiguity by candidates.*

Theorem 3, like Theorem 2, does not imply the existence of a *winning strategy* or an equilibrium situation. In particular, though we know that $p(x) \in S_{II}$ exist that defeat x_{med} , we do not know whether any $p(x)$ defeat *all* $x \in S_I$.⁴⁷ It is to this important question that we now turn our attention.

The Search for Equilibrium

Given the last theorem, suppose that for each $p(x)$ that defeats x_{med} some other point $x^* \in [A, B]$ exists that defeats $p(x)$. That is,

- (i) $p(x)$ defeats x_{med} (Theorem 3)
- (ii) x^* defeats $p(x)$ (by construction)
- (iii) x_{med} defeats x^* (Black's Theorem)

Assertion (i) simply restates the conclusion of Theorem 3 (here, by assumption, the premises are met). Assertion (ii) is a supposition. Assertion (iii) draws on Black's Theorem and the property of the median most-preferred point. But assertions (i) – (iii) jointly yield a collective intransitivity—the so-called cyclical majority problem. *Thus, if an $x^* \in [A, B]$ satisfying (ii) exists, then there are no winning strategies and no equilibrium situations.* That such an x^* can exist seems intuitively plausible. In this analysis, however, we must cast intuition aside and rely on more formal means.⁴⁸

Since this implication (if it is true) is important, it is reiterated. If it is the case that whenever challenging elites perceive incentives to equivocate in their electoral campaigning (i.e., whenever the premises of Theorem 3 are met) that the incumbent possesses a counterstrategy in his strategy set, then it follows that no equilibrium situation exists. The consequences for spatial models of electoral competition in various risk environments, then, are considerable, namely the search for equilibrium will be in vain—none exists!

⁴⁷ Note that the median-defeating strategies $p(x) \in S_{II}$ need not be unique. This lack of uniqueness, however, is inconsequential for our argument.

⁴⁸ Since $x_{\text{med}} \in S_{II}$, strategy x^* is not threatened by it, so that we do not technically have a contingency of collective intransitivity. But a median-defeating strategy $p^*(x) \in S_{II}$ can be found, distributed entirely in an interval of length ϵ about x_{med} , for arbitrarily small ϵ . It may then be shown that $p^*(x)$ defeats x^* . If this $p^*(x)$ is invulnerable, then a collective intransitivity is averted. However, if for any median-defeating strategy in S_{II} there is an x^* which in turn defeats it, then a nonequilibrium situation is insured. It is of theoretical interest to specify the conditions under which these events transpire. Our intentions in what follows are more modest and these fundamental theoretical issues are left as open questions.

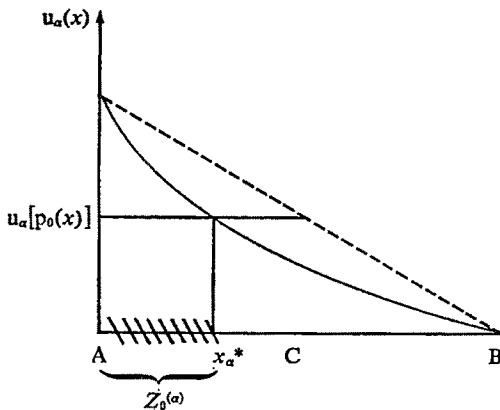


Figure 4. Interval of points preferred to the lottery.

For the case of a *finite* number of alternative strategies (as opposed to a continuum of strategies), a finding by Zeckhauser is important.⁴⁹ Zeckhauser has proved that if there are a *finite* number of basic alternatives (i.e., elements in S_T), if voter preference functions are single-peaked, if *lotteries* (rather than probability density functions) among basic alternatives are permitted, and if some lotteries are able to defeat the median alternative (Theorem 3), then it is always the case that an x^* exists which defeats such lotteries. That is, with finite alternatives, contingencies which provide incentives to equivocate (to choose ambiguous strategies) invariably yield collective intransitivities.

In order to trace the implications of Zeckhauser's Theorem for our model, a proposition for the bounded continuum (A.1) analogous to Zeckhauser's (which, to repeat, applies to the case of finitely-many alternatives) is constructed. It is then shown that this proposition, unlike Zeckhauser's Theorem, is *not* generally valid.⁵⁰

Suppose the premises of Theorem 3 are met. Then median-defeating probability distributions exist. Write this collection of strategies as

$$P = \{p(x)/u_i[p(x)] > u_i(x_{\text{med}})\}$$

for at least a simple majority}.

Since Theorem 3 establishes the existence of median-defeating strategies, we know that P is non-

⁴⁹ Zeckhauser, "Majority Rule."

⁵⁰ Elsewhere I have examined Zeckhauser's Theorem for electoral profiles with nonsingle peaked preferences, proving that equilibrium situations in these contingencies may exist. See Kenneth A. Shepsle, "A Note on Zeckhauser's 'Majority Rule with Lotteries on Alternatives': The Case of the Paradox of Voting," *Quarterly Journal of Economics*, 84 (November 1970), 705-710; and "The Paradox of Voting and Uncertainty," in Niemi and Weisberg, forthcoming.

empty, i.e., $P \neq \emptyset$. Thus we may select some particular probability distribution in P , say $p_0(x)$, and define

$$Z_0^{(i)} = \{x/u_i(x) > u_i[p_0(x)]\}.$$

$Z_0^{(i)}$ is the set of points in $[A, B]$ which are preferred by the i th voter, v_i , to $p_0(x)$. For example, if $p_0(x)$ is the probability distribution over $[A, B]$ for the electoral profile in Figure 2, then $Z_0^{(\alpha)}$ for the type α voters is the interval $[A, x_\alpha^*]$, shown in Figure 4. Any point in $[A, x_\alpha^*]$ is preferred by a type α voter to $p_0(x)$. Generally speaking, $Z_0^{(i)}$ is an interval, a consequence of the nature of postulated preferences (A.2-A.3).

Since there are N voters $\{v_1, v_2, \dots, v_N\}$, there are $\binom{N}{d} = m$ simple majority coalitions, where

$$d = \begin{cases} \frac{N+1}{2} & \text{if } N \text{ is odd} \\ N/2 + 1 & \text{if } N \text{ is even.} \end{cases}$$

We may arbitrarily label these minimum winning coalitions $c_1, c_2, \dots, c_m$, and identify the collection of such coalitions by

$$C = \{c_1, c_2, \dots, c_m\}.$$

Define $\cap_j Z_0^{(i)} = \{x/x \in Z_0^{(i)} \text{ for each } v_i \in c_j, c_j \in C\}$. That is $\cap_j Z_0^{(i)}$ is the set of points common to the $Z_0^{(i)}$ of each v_i in the j th winning coalition. Clearly, if for any of the c_j , $\cap_j Z_0^{(i)}$ is nonempty, then there are points common to the $Z_0^{(i)}$ of at least a simple majority of voters. That is, there are points preferred by a majority to $p_0(x)$. Consequently, if nonempty $\cap_j Z_0^{(i)}$ exist for all $p_0(x) \in P$, then no winning strategies exist and a cyclical majority problem is generated.

The generalization of Zeckhauser's Theorem may be stated:

PROPOSITION: There exist $c_j \in C$

such that $\bigcap_j Z_0^{(i)} \neq \emptyset$.

To see that the proposition implies a nonequilibrium situation, it suffices to prove the following

CONSEQUENCE:

$$x_{\text{med}} \notin \bigcap_j Z_0^{(i)} \quad \text{for any } c_j.$$

A cyclical majority problem follows immediately:

- (i) $p_0(x)$ defeats x_{med} (Theorem 3)
- (ii) $x^* (\neq x_{\text{med}})$ defeats $p_0(x)$,

where $x^* \in \cap_j Z_0^{(i)}$ for some c_j (Definition of $Z_0^{(i)}$ and Consequence)

- (iii) x_{med} defeats x^* (A.2, A.3, and Black's Theorem).⁵¹

⁵¹ See footnote 48.

Proof of the Consequence: Suppose $x_{\text{med}} \in \cap_j Z_0^{(i)}$ for some $c_j \in C$. Then, by definition of $\cap_j Z_0^{(i)}$, a simple majority prefers x_{med} to $p_0(x)$. But, by construction and Theorem 3, $p_0(x)$ is preferred by a simple majority to x_{med} . Since any two majorities contain at least one person in common (this is easily shown),⁵² that person simultaneously prefers $p_0(x)$ to x_{med} and x_{med} to $p_0(x)$. Contradiction! Thus, $x_{\text{med}} \notin \cap_j Z_0^{(i)}$ and the Consequence is established.

With this Consequence, then, we can be assured that if the Proposition is valid, the search for equilibrium will be an unrewarding one.

For a detailed treatment of the logical status of this Proposition, the reader is referred elsewhere.⁵³ For our purposes it only need be reported that some electoral profiles can be constructed for which the Proposition is true, others for which it is false. Thus, the Proposition is neither generally true nor generally false. That is, electoral profiles exist, on the one hand, for which the Proposition and concomitant disequilibrium obtain. On the other, electoral profiles exist for which the Proposition fails to hold. In these latter situations, the challenger possesses a strategy, consistent with C.2, which is *winning*.

Although this result may be disconcerting to the spatial modeler, it sheds some important light on the problems of electoral stability. In particular, it forces us to be attentive to the nature of *electoral profiles*, as defined earlier, rather than solely to the form of voter distribution, typically the concern of the spatial modeler.⁵⁴ Moreover, the result suggests that electoral instability, *even granting the rather restrictive assumption of single-peakedness*, is a sufficiently likely theoretical possibility that is deserving of our attention. Indeed, it may be the rule rather than the exception! It is to these points, as well as a summary of what has preceded, that we turn in the concluding section of this essay.

Conclusions

Anthony Downs has argued that (1) since "ambiguity . . . increases the number of voters to whom a party may appeal," (2) since "this fact encourages parties to be as equivocal as possible about their stands on each controversial issue," and (3) since "this makes it more difficult for each citizen to vote rationally," it follows that "rational behavior by political parties tends to discourage

rational behavior by voters."⁵⁵ Our analysis has restricted the validity of the premises of Downs's argument. Thus, the conclusion is not generally valid.

First, it was demonstrated in Theorems 1 and 2 that ambiguity actually decreases the appeal of a candidate, that a candidate restricted to ambiguous strategies is positively disadvantaged, and that in the contingencies presumed in the theorems it is *irrational* for candidates not so constrained to behave equivocally. That is, if all voters are risk-averse, an equivocal challenger is vulnerable to a median-adopting, nonequivocating incumbent. Moreover, if only a majority of voters is risk-averse, a challenger possesses no winning strategy. Indeed, the overriding strategic problems in contingencies of risk aversion are those of commitment and credibility, not equivocation.

Second, it was shown in Theorem 3 that, *for a different electoral profile*, ambiguity and equivocation may be positively rewarding. The effect of this theorem is a limitation of the scope for which Downs's premises are valid. Specifically, if a majority of voters is risk-acceptant (and, by interpretation, possesses intense preferences, thus rendering the issue "critical"), then parties may be "encouraged to be as equivocal as possible about their stands . . ."

The "may be" in the assertions above needs to be underscored, however, for, third, we have shown through a generalized version of Zeckhauser's Theorem that the dominance of an ambiguous strategy cannot be determined without a further specification of the electoral profile.⁵⁶

Fourth, Downs's third premise above, which derives from an unnecessarily narrow definition of rationality, is called into question. If uncertainty renders rationality problematic, then the rational decision maker is an empirical myth since, empirically, decisions are never made under conditions of perfect certainty. Thus, Downs's inference about the effects of uncertainty on rationality, leads to one of two actions:

- (1) eliminate the rational man concept entirely, since uncertainty is pervasive in any empirical setting, or

⁵² Downs, p. 136.

⁵³ In "Parties, Voters, and the Risk Environment . . .," I conjectured that under the conditions of THEOREM 3 there does exist a winning strategy in the challenger's strategy set. However, the more recent generalized version of Zeckhauser's Theorem makes that conjecture questionable, or at least incompletely specified. For a recent rather general mathematical treatment of the question of social choice under conditions of uncertainty, see Peter Fishburn, "Lotteries and Social Choice," a paper presented at the Mathematical Collective Decisions Seminar, Hilton Head Island, South Carolina, August, 1971.

⁵⁴ See John G. Kemeny, J. Laurie Snell, and Gerald L. Thompson, *Introduction to Finite Mathematics*, 2nd ed. (New York: Prentice-Hall, 1966), p. 82.

⁵⁵ In particular, see Shepsle, "Uncertainty and Electoral Competition . . ."

⁵⁶ See Downs, *Economic Theory of Democracy*, Chap. 8, and Davis, Hinich, and Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process."

- (2) provide a different interpretation for the concept of rationality.

I have opted for the latter alternative, arguing that rationality simply implies the efficient use of contextual information so as to produce actions consistent (*a priori*) with preferences. The expected utility maximization rule, our rule of voter decision making, is compatible with this view.

Finally, the theoretical appropriateness of searching for equilibrium situations when disequilibrium may be the theoretically interesting state was questioned. In the derivation of the generalized version of Zeckhauser's Theorem, and in the claim that examples exist which render this Proposition both true and false (but not simultaneously, of course!), it was suggested that the "search for equilibria" may not be the appropriate theoretical posture—that instability and the absence of equilibrium (in the dominance sense) may be the rule rather than the exception, and that the interesting behavior to model is the strategic adaptation of actors to these nonequilibrating contingencies.

The particular way in which uncertainty was introduced in the context of spatial models of

electoral competition may be inadequate, and is most certainly incomplete. Not included, for example, are any manifestations of uncertainty in candidate decision making except as they arise in the game-context of strategy selection. Thus, it was supposed that there is no uncertainty in candidate information about voter preferences or strategy constraints.⁵⁷ It would be of great interest to incorporate questions of this sort. Moreover, it should be emphasized that the results in this paper depend upon highly restrictive assumptions. Increased mathematical generality, then, is a first-order concern.

Caveats aside, the results derived in the body of this paper suggest, minimally, that the risk environment is not neutral. And, along with the electoral profile and strategy constraints, the risk environment is conceptually useful in providing nonobvious insights about the strategic behavior of political elites.

⁵⁷ For an interesting essay that deals with candidate uncertainty about the parameters of the voter distribution and the effect of this on the decision to allocate resources for information collection, see Jane Gilbert, "Who Gets Information, Where, and When" (University of Rochester, 1970, unpublished manuscript).

Cluster-Bloc Analysis and Statistical Inference*

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Introduction

The analysis of legislative voting and judicial decisions has long been regarded as a proper domain of study for the political scientist. But, because of the vast amount of work involved, few systematic studies have emerged. In addition the prospective researcher must choose from at least five methodological approaches: (a) the construction of indices of cohesion for particular groups, (b) cluster-bloc analysis, (c) Guttman scaling, (d) factor analysis, and (e) "complete analysis." The main use of all the methods is to provide a description of the structure of the various groups in the legislature. Guttman scaling is the only method in which the voting groups must be described with reference to a particular issue set of related roll calls; the other methods may be used with either a set of related roll calls or with *all* the issues on which votes were taken.¹

Hovet has examined the voting records of the United Nations General Assembly and its main committees for the first sixteen UN Sessions.² He was concerned to discover how cohesive the various groups, that meet behind the scenes in caucus, actually are in their roll-call voting. For each group he constructed percentage indices of identical voting, "solidarity" voting (when the dissenting members abstain rather than vote against the group), and divided voting. Lijphart has shown that these indices are not sound for comparing groups of ten or less members.³ Lijphart also suggested

that we can understand the voting structure of the UN much better when we empirically determine the voting blocs than when we examine the record of predetermined groupings. Lijphart suggested that cluster-bloc analysis, as developed by Beyle in 1931 from earlier work by Rice in the 1920s, was a more satisfactory methodology.

The three other alternative methods also allow the groups to emerge empirically from the voting records. Guttman scaling is well known and is conceptually simple to understand. But for the analysis of UN voting, which is the author's main interest, it has only been used to examine one UN Session,⁴ although it has been used more extensively to study other bodies. Guttman scaling is not necessarily an alternative to cluster-bloc analysis but can serve as a useful complement to it. Once blocs have been discovered, we can then see whether they can be described in terms of the issues which unite them. We can ask if the bloc has a low variance in its scores on a Guttman scale.

The two remaining methods, factor analysis⁵ and "complete analysis,"⁶ have the advantage that they are firmly based on well developed statistical methods. Many political scientists, however, remain skeptical about applying such high-powered methods to simple YES, NO, ABSTAIN data.⁷ What is substantively more im-

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¹ In everyday use the words "vote," "roll call" and "issue" may be used with a high degree of interchangeability. To avoid confusion in this article, the use of the nouns will be restricted as follows: "vote"—one decision of Yes/No/Abstain by one individual; "roll call"—the aggregate result of all the members publicly voting on one resolution, bill, amendment, case or procedural question; "issue"—an underlying dimension that is common to a set of related roll calls.

² Thomas Hovet, *Bloc Politics in the United Nations* (Cambridge, Mass.: Harvard University Press, 1960); Thomas Hovet, *Africa in the United Nations* (Evanston, Ill.: Northwestern University Press, 1963).

³ Arend Lijphart, "The Analysis of Bloc Voting in the General Assembly: A Critique and a Proposal,"

American Political Science Review, 57 (December 1963), 902-917.

⁴ Leroy N. Rieselbach, "Quantitative Techniques for Studying Voting Behavior in the UN General Assembly," *International Organization*, 14, (Spring 1960), 291-306.

⁵ On UN voting, Hayward R. Alker and Bruce M. Russett, in *World Politics in the General Assembly* (New Haven: Yale University Press, 1965), used factor analysis to discover issue dimensions and described states in terms of factor scores. Bruce M. Russett, in "Discovering Voting Groups in the United Nations," *American Political Science Review* 60 (June 1966), 327-339, used the alternative Q-type factor analysis.

⁶ Keith Hope, "Complete Analysis: A Method of Interpreting Multivariate Data," *Journal of Market Research Society* (U.K.) 2 (1969), 267-284. The example given in this article used data from the 17th session of the UN.

⁷ John E. Mueller, "Some Comments on Russett's 'Discovering Voting Groups in the United Nations,'" *American Political Science Review*, 51 (March 1967), 146-148. Oran R. Young, "Professor Russett: Industrious Tailor to a Naked Emperor," *World Politics*, 21 (April 1969), 486-495. Robert E. Riggs, Karen F. Hanson, Mary Heinz, Barry B. Hughes and

portant is that, although these methods may successfully discover voting blocs, they tell us very little about the relationships *between* blocs. As Truman puts it, cluster-bloc analysis "has the considerable advantages of keeping close to the data and of requiring few assumptions in manipulating them."⁸ For his study of the 81st Congress, Truman observed that the method offered other advantages. "Given the availability of the paired agreement scores, it was possible to perform a number of specialized analyses for particular subgroups within the parties . . . such as the seniority leaders, the elective leaders, the policy committees and the like."⁹ Truman comments that in using this method "one is obliged to pay a price, namely foregoing most of the refined techniques of statistical inference that are at least theoretically possible with more elaborate methods."¹⁰ The aim of this paper is to show that it is not necessary to pay such a price. The cluster blocs need only show paired agreement scores that are statistically acceptable at stipulated level of significance.

The Concept of Random Voting

The first step in cluster-bloc analysis is to calculate an agreement score for every possible pair of members of the legislative body. Then these scores must be scanned to see whether they qualify for inclusion in a table or a matrix or a diagram, (depending on the researcher's preference), to illustrate those groups of members that together form a cluster bloc. The resulting bloc is thus a subgroup of the legislature that shows greater than average cohesion. Every member of the bloc is in high agreement with every other member of the bloc. The problem is what constitutes "high agreement"? The following quotations (with italics added) show how this problem has worried all those who have used cluster-bloc analysis. Truman in his study, *The Congressional Party*, said that The choice of this cut-off point is an *arbitrary one* . . . Nevertheless, the choice need not be haphazard. If it is set high the blocs will be relatively small . . . But the number of votes on which all members of the bloc are agreed will be relatively high. If it is set low, the blocs will be larger . . . but the number of unanimous votes within the blocs will be smaller. . . . In the Senate sets [of roll calls] agree-

ments on more than half of the roll calls were considered.¹¹

Lijphart faced the problem in studying UN voting:

The figure can be *arbitrarily set* at 95, 90, or 80 per cent for instance . . . it is preferable to analyze the Indices of Agreement at successively lower levels, beginning with Indices of 100 per cent, in order to discover the exact level at which the blocs emerged.¹²

Schubert was concerned with the analysis of judicial decisions:

On the basis of limited empirical application . . . for the tables of overall inter-agreement, which are expressed in percentages, the average of the ratios of the included pairs can be used as an Index of Inter-Agreement, with .70 or better considered high, .60 to .69 moderate and less than .60 low.¹³

As long ago as 1931, Beyle tried to tackle this problem of determining a significant level of agreement. He started by considering what would happen if the legislators were voting purely at random and pointed out that

If only chance factors were operative, there would be a 50-50 chance of a legislator voting on one side of a question, one half of unity. Similarly, the chance of a second legislator so voting would be also one half of unity. The chance that both legislators would so vote, and thus be in agreement, would be one-half times one-half, or one fourth of unity.

Up to this point Beyle is entirely correct in his reasoning. He goes on:

Given 8 votes . . . their chance agreement on a given side of the questions would be one fourth of eight or two agreements. [An index may be constructed such that] the numbers between two and eight would be represented by proportionate degrees between zero and 100. Thus, the index number representing the significance of three agreements would be 16.6; the index number for four agreements would be 33.3; for 5, 50.0; for 6, 66.6; and for 7, 83.3. Index numbers for 33.3 may be taken to be particularly significant as indicating cohesion equal to, or greater than two times chance.¹⁴

Hayes pointed out in 1938 that this index has no meaning or value. "The *degree* of relation-

¹¹ Truman, p. 324.

¹² Arend Lijphart, "Analysis of Bloc Voting . . .," p. 913.

¹³ Glendon A. Schubert, "The Study of Judicial Decision-Making as an Aspect of Political Behavior," *American Political Science Review*, 52 (December 1958), 1007-1025: quotation from pages 1012-13. (There is actually a substantial text covered by the ellipsis dots, but Schubert's meaning is not violated.)

¹⁴ Herman C. Beyle, *Identification and Analysis of Attribute-Cluster-Blocs* (University of Chicago Press, 1931), 29-31.

Thomas J. Volgy, "Behavioralism in the study of The United Nations," *World Politics* 22 (January 1970), 192-326, see especially p. 211.

⁸ David Truman, *The Congressional Party* (Wiley, New York, 1959), p. 329.

⁹ Truman, p. 326.

¹⁰ Truman, pp. 321-322.

ship and the *probability* of relationship are not at all the same thing. Degree of relationship is independent of the size of the sample involved, while probability is directly affected by the size of the sample."¹⁵ Beyle is right that one would expect by chance to get two agreements on one side, out of eight roll calls, but he is wrong to say that twice the expected result is significant. Significance is not determined by the result being two, three, or any multiple of the expected value of a sampling distribution but by the result being so many standard deviations from the expected value (assuming normality).

Unfortunately, Hayes himself then pursued a completely fallacious argument; he attacked Beyle's assumption that random voting was the proper standard of comparison, claiming that for two legislators

the probability of their agreements being within the possible range of chance factors depends upon the frequency with which each of them votes "aye" and "nay" . . . Only when given those frequencies can one calculate the probability of their agreeing with each other. For instance if two legislators each voted "aye" on every bill that came up, of course they would *agree* on every bill. However, that would be no indication that they were significantly related (in statistical terminology) to each other.¹⁶

Intuitively this conclusion makes no sense. If two legislators are in 100 per cent agreement on enough roll calls, there *must* be common political factors at work. Either they cooperate, or they are both loyal to the majority leader, or they both believe in following a Congressional consensus, or some other political process is operating. No person can be regarded as having an innate probability of voting "aye" of anything other than 0.5, unless he is motivated by some factor such as a desire to follow the consensus. Such a discovery would be in itself of interest and be politically significant.

As a result, Beyle's basic tenet must be upheld: an agreement score must be regarded as significantly high if it is larger than any value that would be likely to occur with random voting. Beyle was only concerned with agreement occurring on one side of a question. Most people now use indices of identical voting that count joint agreement either for *or* against the question; such indices will be discussed in the remainder of this paper.

If two legislators A and B, both vote at ran-

dom on a roll call, there are four possible outcomes to the voting:

	A's vote	B's vote	
1)	Yes	Yes	]—Identical voting
2)	No	No	
3)	Yes	No	]—Disagreement
4)	No	Yes	

With the probability of voting Yes equal to the probability of voting No and both equal to 0.5, the probability of each result is $0.5 \times 0.5 = 0.25$. The probability of agreement is then the probability of occurrence of either the first outcome or the second outcome, i.e., $0.25 + 0.25 = 0.5$. Similarly the probability of disagreement is 0.5, for any one roll call. Having started by considering just one legislator voting Yes or No, we have now moved one step further and derived the probability of agreement for a pair of legislators jointly voting on the same roll call. The symbol p_f will be used to signify the probability of agreement and p_d the probability of disagreement. Although the original assumption of random voting with $P(\text{Yes}) = P(\text{No}) = 0.5$ leads to a conclusion with a similar result, namely $p_f = p_d = 0.5$, it must be remembered that we are now dealing with the joint behavior of a pair of legislators and not with an individual legislator.

What happens if the two legislators each vote on eight separate roll calls? There are nine possible *agreement* patterns. The two legislators may fail to agree on any of the eight roll calls, or they may agree on 1, 2, 3, . . . or even all eight roll calls. This gives nine possible agreement scores, ranging from 0 per cent to 100 per cent. The probability of each score's occurring by random voting can be calculated using the general laws of probabilities and the binomial distribution.¹⁷

$P(r \text{ agreements in } N \text{ roll calls})$

= No. of ways of getting r agreements times
Probability for each way.

$$= C_N^r p_f^r p_d^{N-r}$$

As emphasized above, the "number of ways of getting r agreements" concerns the joint behav-

¹⁵ Samuel P. Hayes, "Probability and Beyle's 'Index of Cohesion,'" *Journal of Social Psychology*, 9 (1939), 161-167; quotation from p. 161, italics in the original.

¹⁶ Hayes, p. 163, italics in the original.

¹⁷ A discussion of probability theory and the binomial distribution is given in most statistical textbooks. See for example Blalock, *Social Statistics* (New York: McGraw-Hill, 1960) for a particularly clear exposition. The argument at this stage is identical to the conventional discussion of how many heads will be expected in N tosses of a coin.

Table 1. Probability of Obtaining Each Agreement Score, with Eight Roll Calls

1	2	3			4	5	6
No. of Agreements	% Score	Probability of each Score				Cumulated Probabilities	
0	0%	C_0^8	p_I^0	p_d^8	.00391	.00391	1.00000
1	12.5%	C_1^8	p_I^1	p_d^7	.03125	.03516	.99609
2	25.0%	C_2^8	p_I^2	p_d^6	.10937	.14453	.96484
3	37.5%	C_3^8	p_I^3	p_d^5	.21875	.36328	.85547
4	50.0%	C_4^8	p_I^4	p_d^4	.27344	.63672	.63672
5	62.5%	C_5^8	p_I^5	p_d^3	.21875	.85547	.36328
6	75.0%	C_6^8	p_I^6	p_d^2	.10937	.96484	.14453
7	87.5%	C_7^8	p_I^7	p_d^1	.03125	.99609	.03516
8	100.0%	C_8^8	p_I^8	p_d^0	.00391	1.00000	.00391

ior of two legislators. Thus there is only "one way" of getting no agreements at all: they are opposed to each other on every one of the eight roll calls.¹⁸

The probability equations for 8 roll calls have been set up in column 3 of Table 1. The C_r^N term is evaluated according to the formula:

$$C_r^N = \frac{N!}{r!(N-r)!}$$

We have already seen that $p_I = 0.5$ and also that $p_d = 0.5$. When these values are substituted in the expressions given in column 3 of Table 1, the results obtained are in column 4. When the probabilities for the nine possible agreement scores are summed, the total comes, as it always must, to 1.0. Figure 1 shows that, even in voting on as few as eight roll calls, the sampling distribution approaches a normal curve.

The values of the agreement scores, that are significantly different from the "expected" mean value 50 per cent, are found by examining the tails of the sampling distributions. At the .001 level of significance, with eight roll calls no score is significant. Even 100 per cent agreement could occur by chance as often as four times in a thousand. But, using a single-tail test, 100 per cent is significant at the .01

level. When we move up to the .025 level of significance 100 per cent is still the only significant score. By the time we consider a .05 level of significance and are prepared to take the greater risk of being wrong in our inference, a score of 87.5 per cent agreement may also be considered significant. Thus in performing cluster-bloc analysis on eight roll calls the cutoff points for forming blocs should be either 87.5

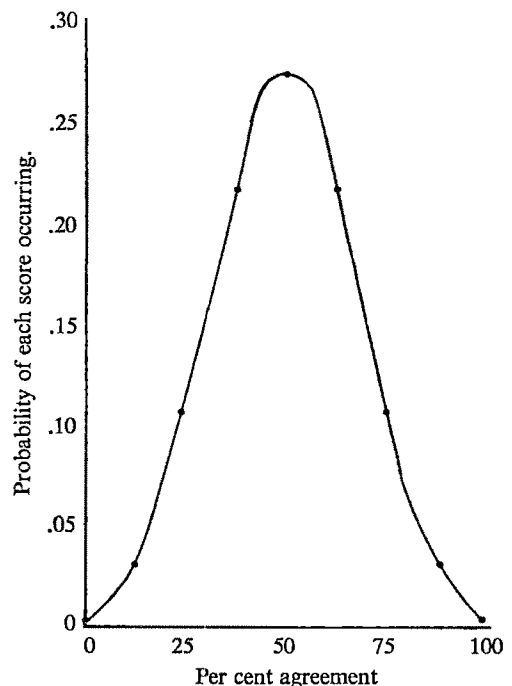


Figure 1. Sampling distribution for random voting. 8 roll-calls, Simple Percentage Index of Agreement.

¹⁸ This result could occur if legislator A voted Yes on all roll calls and B voted No on all roll calls; or the reverse could occur; or A could vote Yes on the first four and No on the second four, with B voting the reverse way; etc. etc. These variations which all produce the same agreement score of 0 per cent already been taken into account by the derivation of p_I and p_d from the four original Yes-No voting patterns.

per cent or 100 per cent, depending on the level of significance at which the researcher wishes to operate.

As the sampling distribution is completely symmetrical we can also consider the second tail of the distribution and find scores so low that they represent *significant disagreement*. Thus, on eight roll calls, 0 per cent is significant at the .01 or .025 level, and 12.5 per cent or less is significant at the .05 level. To the author's knowledge no other researchers have considered particularly low scores in their analyses of voting patterns. In any large assembly it is difficult enough to handle all the information generated about high agreement, and in any case most assemblies have enough consensus operating that scores below 30 per cent or so would be unusual. If, however, an assembly were found to be divided into two or more distinct cluster blocs, it would be well worth considering whether the divisions are so deep that there is also significant disagreement between the blocs or between some of their members.

So far the discussion has only considered voting on eight roll calls. If a different number of roll calls were under consideration, then the whole argument must be repeated to find the cutoff points for the agreement scores on N roll calls. The general formula is that J or fewer

the cutoff points higher in the table.¹⁹ Thus, if 57 roll calls were being considered and the .05 level of significance were adopted, one should use the figure given for 50 roll calls (cutoff = 64.0 per cent) rather than that for 60 roll calls (cutoff = 61.7 per cent).

The Definition of Indices

We must now examine more carefully the assumption that random voting is the proper standard of comparison for deciding which scores are significantly high. Surely it will be possible in a legislature or assembly that operates by achieving high consensus and avoiding roll calls on divisive issues, to obtain the result that every member of the legislature is in significant agreement with every other member of the legislature? Of course this is true, but it would be a major finding about the behavior of the legislature. We would not have discovered any cluster blocs within the legislature but we would have firmly established that such divisions as there were either were very weak or were not expressed in the roll calls. Therefore, the first examination of the scores should always be against the null hypothesis of random voting. Indeed we now have for any legislature of M members an Index of Agreement, I_a , that is statistically based.

$$I_a = \frac{\text{Number of pairs of members in significant agreement}}{\text{Total possible number of pairs of members}} \\ = \frac{\text{Number of pairs in significant agreement}}{M(M-1)/2}$$

Similarly, for a legislature that has deep divisions, we have an Index of Significant Disagreement, I_β ,

$$I_\beta = \frac{\text{Number of pairs in Significant Disagreement}}{M(M-1)/2}$$

agreements will occur on N roll calls with a probability of

$$P = \sum_{I=0}^{I=J} C_I^N \cdot p_f^I \cdot p_d^{N-I}$$

Fortunately this expression can be evaluated by computer. The results, for random voting on up to 250 roll calls, are given in Table 2. After 30 roll calls the results are only given for every 10, and after 80 roll calls for every 20, since the differences in cutoff points become very slight. To err on the side of caution, one should use

The two give an overall Index of Cohesion, $I_\gamma = I_a - I_\beta$. In theory it will be possible for I_γ to obtain a negative value by the number of pairs in disagreement being higher than the number of pairs in agreement, but this is not very likely to occur in practice. I_γ should not be reported without the corresponding values of I_a and I_β , and all

¹⁹ This is not always true as there are some discontinuities in the results due to percentaging on a relatively low base, e.g., at the .05 level, the cutoff for 110 roll calls is 59.1 per cent which is higher than that for 100 roll calls. More detailed results are available on request.

Table 2. Percentage Cutoff Points for Cluster-Bloc Analysis: Simple Indices of Identical Voting*

No. of Roll calls	Significance Levels for Single-Tailed Tests							
	Disagreement				Agreement			
	.001	.01	.025	.05	.05	.025	.01	.001
4	—	—	—	—	—	—	—	—
5	—	—	—	0.0	100.0	—	—	—
6	—	—	0.0	0.0	100.0	100.0	—	—
7	—	0.0	0.0	0.0	100.0	100.0	100.0	—
8	—	0.0	0.0	12.5	87.5	100.0	100.0	—
9	—	0.0	11.1	11.1	88.9	88.9	100.0	—
10	0.0	0.0	10.0	10.0	90.0	90.0	100.0	100.0
11	0.0	9.1	9.1	18.2	81.8	90.9	90.9	100.0
12	0.0	8.3	16.7	16.7	83.3	83.3	91.7	100.0
13	0.0	7.7	15.4	23.1	76.9	84.6	92.3	100.0
14	7.1	14.3	14.3	21.4	78.6	85.7	85.7	92.9
15	6.7	13.3	20.0	20.0	80.0	80.0	86.7	93.3
16	6.3	12.5	18.8	25.0	75.0	81.3	87.5	93.8
17	5.9	17.7	23.5	23.5	76.5	76.5	82.4	94.1
18	11.1	16.7	22.2	27.8	72.2	77.8	83.3	88.9
19	10.5	21.1	21.1	26.3	73.7	79.0	79.0	89.5
20	10.0	20.0	25.0	25.0	75.0	75.0	80.0	90.0
21	14.3	19.1	23.8	28.6	71.4	76.2	81.0	85.7
22	13.6	22.7	22.7	27.3	72.7	77.3	77.3	86.4
23	13.0	21.7	26.1	30.4	69.6	73.9	78.3	87.0
24	16.7	20.8	25.0	29.2	70.8	75.0	79.2	83.3
25	16.0	24.0	28.0	28.0	72.0	72.0	76.0	84.0
26	15.4	23.1	26.9	30.8	69.2	73.1	76.9	84.6
27	18.5	25.9	25.9	29.6	70.4	74.1	74.1	81.5
28	17.9	25.0	28.6	32.1	67.9	71.4	75.0	82.1
29	17.2	24.1	27.6	31.0	69.0	72.4	75.9	82.8
30	20.0	26.7	30.0	33.3	66.7	70.0	73.3	80.0
40	22.5	30.0	32.5	35.0	65.0	67.5	70.0	77.5
50	26.0	32.0	34.0	36.0	64.0	66.0	68.0	74.0
60	28.3	33.3	35.0	38.3	61.7	65.0	66.7	71.7
80	31.3	36.3	37.5	40.0	60.0	62.5	63.8	68.8
100	34.0	37.0	39.0	41.0	59.0	61.0	63.0	66.0
120	35.0	38.3	40.0	41.7	58.3	60.0	61.7	65.0
140	36.4	39.3	40.7	42.1	57.9	59.3	60.7	63.6
160	37.5	40.0	41.9	43.1	56.9	58.1	60.0	62.5
180	37.8	40.6	42.2	43.3	56.7	57.8	59.4	62.2
200	38.5	41.5	42.5	43.5	56.5	57.5	58.5	61.5
250	40.0	42.4	43.6	44.4	55.6	56.4	57.6	60.0

* Probability of Agreement=0.5; Probability of Disagreement=0.5.

three should be given at the chosen level of significance. This is because both I_a and I_b may become higher at a lower level of significance.

Just as these three indices may be used to examine the cohesion of the whole legislature they may also be used to examine specified subgroups. The following results, for example, were obtained in a study of the behavior of the African group in the United Nations General Assembly.²⁰

When the internal cohesion of specified groups is under consideration it may also be of interest to know how strong is the relationship *between* the groups. For example, are the Africans closer to the NATO or to the Warsaw Pact powers? Since the first step in cluster-bloc analysis would be to calculate agreement scores for every state with every other state, we may examine the level of agreement between every member of NATO and every member of the

²⁰ Peter Willetts, "The Behavior of the African Group in the UN General Assembly" (unpublished

M.Sc. Dissertation, University of Strathclyde, Scotland).

African group. For example, if the agreement score between France and Madagascar were found to be significantly high, this would constitute a "link" between the two groups. In general if the groups are of size M_1 and M_2 we may define the following indices to examine the relationship between the groups:

quent sessions; by the 20th Session, the two groups show no significant differences at all. The increase in cohesion of the African group as a whole was mainly due to the decrease in hostility between the two rival subgroups. Several explanations of such patterns of change are possible. In this case the increased adhesion is

Inter-Group Agreement

$$IG_\alpha = \frac{\text{No. of significant links between the groups}}{\text{Total possible no. of links between the groups}} \\ = \frac{\text{No. of significant links}}{M_1 \times M_2}$$

Inter-Group Disagreement

$$IG_\beta = \frac{\text{No. of significant disagreements between the 2 groups}}{M_1 \times M_2}$$

Inter-Group Adhesion

$$IG_\gamma = IG_\alpha - IG_\beta$$

We have already seen in Table 3 that the African Group was relatively disunited in the United Nations 15th Session, but in subsequent sessions it became more united until in the 20th Session I_α attained 100 per cent at the .001 significance level. The earlier lack of unity might be explained by the fact that during 1960 the African states formed two rival groups that met regularly outside the United Nations. They became known as the Casablanca and Brazzaville groups. The Inter-Group Indices may be used to examine whether or not the differences within the African group in the UN were an expression of the split between these two rival groups.²¹

The indices in Table 4 clearly show a split in the 15th Session and a rapid change in subse-

due not so much to *rapprochement* of the two groups as to the resolution or diminution of the four main disputes that originally led to the formation of the groups: Mauritania gained admission to the UN; Algeria gained its independence from France; the bitterness of the disputes in the Congo were somewhat eased; and Nkrumah had to accept failure for his own militant brand of Pan-Africanism.

One of the original reasons for suggesting that cluster-bloc analysis was a more useful methodology was the advantage it offered of permitting empirical determination of the voting groups. But by using the voting agreement scores and the above indices we can also examine any specified group that is of interest. The indices are statistically based and are simple to comprehend. They have the same meaning when groups of different sizes are compared. By contrast, if Hovet tells us that a group is divided 50 per cent of the time, we do not know whether there is just one deviant member (in which case I_α would remain quite high), or whether the group

Table 3. Cohesion of the African Group in the UN

Session	$p \leq .001$			$p \leq .05$		
	I_α	I_β	I_γ	I_α	I_β	I_γ
15th	56.0%	0%	56.0%	72.7%	1.0%	71.7%
16th	90.8%	0%	90.8%	100.0%	0%	100.0%
17th	77.1%	0%	77.1%	96.2%	0%	96.2%
18th	89.5%	0%	89.5%	99.6%	0%	99.6%
20th	100.0%	0%	100.0%	100.0%	0%	100.0%

²¹ Willetts, Chap. 4. Both Tables 3 and 4 are based on the use of Lijphart's Index of Agreement for UN votes, discussed later in this paper.

Table 4. Casablanca-Brazzaville Inter-Group Agreement

Session	p ≤ .001			p ≤ .05		
	IG _α	IG _β	IG _γ	IG _α	IG _β	IG _γ
15th	1.8%	0%	1.8%	12.7%	5.4%	7.3%
16th	45.0%	0%	45.0%	100.0%	0%	100.0%
17th	48.6%	0%	48.6%	100.0%	0%	100.0%
18th	73.6%	0%	73.6%	98.6%	0%	98.6%
20th	100.0%	0%	100.0%	100.0%	0%	100.0%

is regularly split into two different factions (in which case I_{α} would be low and I_{β} might be high).

Significant Relative Agreement

As suggested earlier, a group might achieve such a high consensus that every member was in significant agreement with every other member. Also we have seen that this extreme result did occur for the African group in the United Nations 20th Session. Knowing this does not mean that one cannot say anything about the structure of such a group. It simply means that the group is homogenous and united, as compared to any model of random voting.

In any one session of a legislature or assembly, however, the issues on which the members are called upon to vote do not present a random stimulus. One session may produce a very high consensus simply because no controversial issues arise or lead to roll calls during the period that it is meeting. In this situation we may take the total votes on any one roll call as an approximate measure of the stimulus provided by the roll call. Thus if the U.S. Senate votes 63-37 in favour of a bill, we may say for any individual Senator the probability of voting "Aye" was 0.63 and the probability of voting "Nay" was 0.37. The resulting probability that any two Senators will agree is

$$p_f = 0.63^2 + 0.37^2 = 0.534$$

and the probability that they will disagree is

$$p_d = 2 \times 0.63 \times 0.37 = 0.466$$

If such a calculation is made for each roll call during the session, the average probability of agreement for the session can be obtained. Then, by substituting these new values of p_f and p_d in the equations given earlier, a different sampling distribution is generated. Since random voting, with an average 50-50 split on all roll calls, represents the maximum possible division within a legislature, any values of p_f calculated empirically from the voting marginal

totals are likely to be higher than 0.5. The sampling distribution could then no longer be normal but would be skewed toward 100 per cent. As the argument has not rested on the assumption of normality, the skew does not mean we cannot obtain significant cutoff points. In fact, the exact probability for every point on the sampling distribution can be calculated. Therefore we still obtain exact significance levels from the tails of the distribution. To distinguish these cutoff points from the ones obtained by comparison to random voting we may term them the levels of significant *relative* agreement. They may only be used in a particular assembly to examine alignments on a given set of roll calls.²²

Judicial Decisions

Very often in the study of judicial decisions, a large number of cases are summarized by using cluster-bloc analysis. Indeed, in many ways this is an ideal use of the methodology, because it is possible to gain a better understanding of all the interrelationships in a small body such as a court. Courts may operate, however, with a high degree of unanimity and therefore, against the null hypothesis of random voting, the court may appear to be completely united, with every judge in significant agreement with every other judge. One response to such a situation is to regard it as essential rather than optional to study significant *relative* agreement. An alternative response is to eliminate from the study the unanimous decisions and perhaps those with only a single dissenter. Schubert has analyzed split decisions and has even constructed matrices of agreement scores solely for marginal decisions of the U.S. Supreme Court.²³

²² Details of the computer program for those that need to calculate cutoff points for significant relative agreement are available on request. Tables for values of $p_f = 0.6, 0.7, 0.8$, and 0.9 have already been computed and are also available.

²³ Glendon A. Schubert, *Quantitative Analysis of Judicial Behavior* (Glencoe, Illinois: Free Press, 1959). Unanimous decisions constituted about 30 per cent of all cases (p. 81). For marginal decision matrices see p. 117 and pp. 149-53.

Russell in analyzing the Canadian Supreme Court had to omit as many as 70 per cent of the decisions because they were unanimous.²⁴ Such omissions are perfectly acceptable so long as the focus of analysis is the nature of the divisions within a court. But if we wish to study whether division is increasing or decreasing over time, then unanimous decisions cannot be ignored.

Because of the special nature of a dissenting legal opinion, several authors have taken to studying agreement in assent and agreement in dissent separately from the overall pattern of agreement.²⁵ When this is done, we are reverting to Beyle's preoccupation with agreement on one side of an issue. The null hypothesis of random voting then gives a probability of agreement of 0.25 (derived from either Yes/Yes pairs or No/No pairs on their own, according to whether agreement in assent or agreement in dissent is taken). This probability value makes the cutoff points much lower (table available on request). Even though the cutoff points are low, joint dissent is likely to be so infrequent that no bloc structure is revealed amongst the agreement-in-dissent scores. This is not so likely to occur when the agreement-in-assent scores are examined.²⁶ If we use the *relative* probability of agreement in dissent, it is likely to be much lower even than 0.25, and the *relative* probability of agreement in assent will be much higher.

It frequently happens in a court that there is only one voice in dissent. This is in contrast to a legislative body, where one person will probably avoid being seen in such isolation. In legislatures, the dissenter may either be absent for the vote or a roll call may be avoided and thought unnecessary by the majority. In the courts, when there is only one dissenter, he obviously cannot be in agreement with anyone else. This means that such decisions cannot be taken into account in calculating the average probability of agreement in dissent, though they *can* be used in calculating the average probability of agreement in assent. Given this

²⁴ Peter Russell, *The Supreme Court of Canada as a Bilingual and Bicultural Institution* (Ottawa: Queen's Printer, 1969), 127.

²⁵ C. Herman Pritchett, *Civil Liberties and the Vinson Court* (Chicago: University of Chicago Press, 1954); C. Herman Pritchett, *The Roosevelt Court: A Study in Judicial Politics and Values* (New York: Macmillan, 1948); see also Schubert, *Quantitative Analysis*. . . ; and Russell *The Supreme Court of Canada*. . .

²⁶ For example, on p. 132 of Russell the two joint dissent matrices only yield one pair of judges in significant agreement, whereas on p. 133 the joint assent matrices clearly show a bloc structure.

problem, it is not possible to approximate to the average probability of dissent by simply taking (total number of dissents)/(total possibilities for dissent). If there is a large number of decisions with single dissenters, the average probability of agreement in dissent will be very low and it may not be possible to find significant pairs.

United Nations Voting

When we wish to study voting in the United Nations, the problem is complicated by abstentions. The General Assembly accepts this as a recognised form of voting that is entered into the Official Records along with the details of those voting Yes or No. It is not equivalent to a state being absent and is used to express limited opposition to a proposal. Both Rieselbach and Meyers took this into account by including joint abstentions in their indices of identical voting for pairs of states.²⁷ On the other hand Lijphart went further and suggested that allowance should also be made for the fact that Yes-Abstain or No-Abstain pairs are in "partial agreement," he defined an Index of Agreement:

$$I_A = \frac{(f + \frac{1}{2}g)}{t} \times 100,$$

where

f = the number of identical roll calls, for the two states,

g = the number in which the states showed partial agreement,

t = the number that both states participated in.²⁸

The assumption of random voting in the UN would require: $P(\text{Yes}) = P(\text{No}) = P(\text{Abstain}) = 1/3$. There are then nine possible outcomes, when two states vote, and each has a probability of $1/3 \times 1/3 = 1/9$.

A 's vote	B 's vote	
1) Yes	Yes	]—Agreement
2) No	No	
3) Abstain	Abstain	
4) Yes	Abstain	]—Partial Agreement
5) Abstain	Yes	
6) No	Abstain	
7) Abstain	No	]—Disagreement
8) Yes	No	
9) No	Yes	

Using the same symbols, p_f = probability of agree-

²⁷ Rieselbach, "Quantitative Techniques . . .," pp. 292–293. Benjamin D. Meyers, "African Voting in the United Nations General Assembly," *Journal of Modern African Studies*, 4 (1966), 213–227.

²⁸ Lijphart, "Analysis of Bloc Voting . . ." p. 910

ment, and p_d = probability of disagreement, with the new symbol p_a = probability of partial agreement, we have:

$$p_f = 1/3 \quad p_a = 4/9 \quad p_d = 2/9 \quad \text{and}$$

$$p_f + p_a + p_d = 1.0$$

For those such as Reiselbach and Meyers, who use simple indices of identical voting to study the UN, the argument given above still applies. The same table may be used as for any other legislature, such as the Congress, which uses simple Yes-No voting. The only difference is that random voting would not give $p_f = 0.5$, but $p_f = 0.3333$ and as partial agreement is ignored $p_d = 0.6666$. The levels of significant relative agreement are found as before by calculating the average empirical value of p_f .²⁹ For N roll calls:

$$p_f = \frac{\sum [P(\text{Yes})^2 + P(\text{No})^2 + P(\text{Abstain})^2]}{N}$$

For those who wish to use Lijphart's Index of Agreement, the situation is a little more complicated. Not only does the use of partial agreements give more discrete values that the index may assume, but it also means that the same value of the index may be produced by different voting patterns. For example when votes are taken on 6 roll calls, there are not seven but thirteen possible scores, which may be formed from twenty-eight possible voting patterns, as given in Table 5.

The probability equations are also more complicated, as multinomial distributions are generated for each agreement score, e.g.,

$$\begin{aligned} P(I_A = 50.0\%) &= C_3^6 \cdot p_f^3 \cdot C_3^3 \cdot p_a^0 \cdot p_d^3 \\ &\quad + C_2^6 \cdot p_f^2 \cdot C_4^4 \cdot p_a^2 \cdot p_d^2 \\ &\quad + C_1^6 \cdot p_f^1 \cdot C_5^5 \cdot p_a^4 \cdot p_d^1 \\ &\quad + C_0^6 \cdot p_f^0 \cdot C_6^6 \cdot p_a^6 \cdot p_d^0 \\ &= 20 \left(\frac{1}{3} \right)^3 \left(\frac{2}{9} \right)^3 \\ &\quad + 90 \left(\frac{1}{3} \right)^2 \left(\frac{4}{9} \right)^2 \left(\frac{2}{9} \right)^2 \\ &\quad + 30 \cdot \frac{1}{3} \left(\frac{4}{9} \right)^4 \cdot \frac{2}{9} \\ &\quad + \left(\frac{4}{9} \right)^6 \\ &= 0.001 \end{aligned}$$

²⁹ Tables with $p_f = 0.333, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9$ or details of computer program available on request.

Table 5. All possible outcomes when two states vote in the U.N. on six roll calls

	$f + \frac{1}{2}g$	I_A (%)	Structure of the voting pattern		
			Agreement	Partial Agreement	Disagreement
1)	6.0	100.0	6	0	0
2)	5.5	91.7	5	1	0
3)	5.0	83.3	5	0	1
4)	5.0	83.3	4	2	0
5)	4.5	75.0	4	1	1
6)	4.5	75.0	3	3	0
7)	4.0	66.7	4	0	2
8)	4.0	66.7	3	2	1
9)	4.0	66.7	2	4	0
10)	3.5	58.3	3	1	2
11)	3.5	58.3	2	3	1
12)	3.5	58.3	1	5	0
13)	3.0	50.0	3	0	3
14)	3.0	50.0	2	2	2
15)	3.0	50.0	1	4	1
16)	3.0	50.0	0	6	0
17)	2.5	41.7	2	1	3
18)	2.5	41.7	1	3	2
19)	2.5	41.7	0	5	1
20)	2.0	33.3	2	0	4
21)	2.0	33.3	1	2	3
22)	2.0	33.3	0	4	2
23)	1.5	25.0	1	1	4
24)	1.5	25.0	0	3	3
25)	1.0	16.7	1	0	5
26)	1.0	16.7	0	2	4
27)	0.5	8.3	0	1	5
28)	0.0	0	0	0	6

Even on random voting the distributions are not normal but slightly skewed toward 100 per cent. Although the procedure is more complicated, the probability of each point on the distribution can still be calculated, and the cutoff points for significant agreement or for significant disagreement are found at the tails of the distribution. The results for varying numbers of roll calls, with the assumption of random voting, are given in Table 6.

When Lijphart studied 44 roll calls on colonial issues in the 11th and 12th Sessions of the

Table 6. Percentage Cutoff points for Cluster-Bloc Analysis. Lijphart Index of Agreement for UN voting*

Number of Roll Calls	Significance Levels for Single-Tailed Tests							
	Disagreement				Agreement			
	.001	.01	.025	.05	.05	.025	.01	.001
2	—	—	—	0.0	—	—	—	—
3	—	—	0.0	0.0	100.0	—	—	—
4	—	0.0	12.5	12.5	100.0	100.0	—	—
5	0.0	10.0	10.0	20.0	90.0	100.0	100.0	—
6	0.0	16.7	16.7	25.0	91.7	91.7	100.0	—
7	7.1	14.3	21.4	28.6	85.7	85.7	92.9	100.0
8	12.5	18.8	25.0	25.0	81.3	87.5	93.8	100.0
9	11.1	22.2	27.8	27.8	83.3	83.3	88.9	94.4
10	15.0	25.0	30.0	30.0	80.0	85.0	85.0	95.0
11	18.2	27.3	27.3	31.8	77.3	81.8	86.4	90.9
12	20.8	25.0	29.2	33.3	75.0	79.2	83.3	91.7
13	19.2	26.9	30.8	34.6	76.9	80.8	80.8	88.5
14	21.4	28.6	32.1	35.7	75.0	78.6	82.1	89.3
15	23.3	30.0	33.3	36.7	73.3	76.7	80.0	86.7
16	25.0	31.3	34.4	37.5	75.0	75.0	78.1	84.4
17	26.5	32.4	35.3	38.2	73.5	76.5	79.4	85.3
18	25.0	33.3	36.1	38.9	72.2	75.0	77.8	83.3
19	26.3	34.2	36.8	39.5	71.1	73.7	76.3	84.2
20	27.5	35.0	37.5	40.0	72.5	75.0	77.5	82.5
21	28.6	33.3	38.1	40.5	71.4	73.8	76.2	81.0
22	30.0	34.1	38.6	40.9	70.5	72.7	75.0	81.8
23	30.4	34.8	39.1	41.3	69.6	71.7	76.1	80.4
24	31.3	35.4	39.6	41.7	70.8	72.9	75.0	79.2
25	30.0	36.0	40.0	42.0	70.0	72.0	74.0	80.0
26	30.8	36.5	38.5	42.3	69.2	71.2	73.1	78.9
27	31.5	37.0	38.9	42.6	68.5	70.4	74.1	77.8
28	32.1	37.5	39.3	42.9	67.9	71.4	73.2	78.6
29	32.8	37.9	39.7	43.1	70.0	70.7	72.4	77.6
30	33.3	38.3	40.0	43.3	68.3	70.0	73.3	76.7
40	36.3	41.3	42.5	45.0	66.3	67.5	70.0	75.0
50	38.0	42.0	44.0	46.0	65.0	67.0	69.0	72.0
60	40.0	43.3	45.0	46.7	64.2	65.8	67.5	70.8
80	41.9	45.0	46.9	48.1	63.1	64.4	65.6	68.8
100	43.5	46.5	48.0	49.0	62.0	63.0	64.5	67.5

* Probability of Agreement=0.333, Probability of Partial Agreement=0.444, Probability of Disagreement=0.222.

General Assembly, he used cutoff points of 95.5 per cent and 87.5 per cent to produce two diagrams of the cluster blocs. At the 95.5 per cent level, he found one bloc consisting of the Communist States, three small blocs of Western States, and an Arab bloc. Twelve other states had one or two isolated links but did not form a bloc. At the 87.5 per cent level, the Communist and Arab blocs merged and were highly intermeshed with the other Afro-Asians; the Western States and some of the Latin Americans also formed another large bloc, and the remainder of the Latin Americans had some links with the Afro-Asians. By using these high cutoff points Lijphart did find the core of the

bloc structure but he also overemphasised the divisions within the Assembly. At the $p < .001$ level of significance, which is a high level of significance to use, it is necessary to go down to a cutoff point of 73.0 per cent for 44 roll calls. This would be almost certain to reveal a continuous structure of overlapping blocs, with the Latin Americans acting as a crucial group in the middle, having links with both sides. Thus the choice of cutoff points is not without substantive significance. By taking them too high one may overemphasize the splits in the Assembly.

Yet again it is possible to use cutoff points to indicate significant *relative* agreement for

United Nations voting. But as there are three variables, p_f , p_g , and p_d to take into account, there will be a greater variability in the reference tables.³⁰ The values of the relative probabilities of agreement, partial agreement, and disagreement may be calculated as follows:

$$p_f = \frac{\sum [P(\text{Yes})^2 + P(\text{No})^2 + P(\text{Abstain})^2]}{N}$$

$$p_g = \frac{\sum 2[P(\text{Yes}) + P(\text{No})] \cdot P(\text{Abstain})}{N}$$

$$p_d = \frac{\sum [2P(\text{Yes}) \cdot P(\text{No})]}{N} = 1 - p_f - p_g$$

It is also possible to use the Indices I_α , I_β , and I_γ to examine the cohesion of groups in the UN and the Indices IG_α , IG_β , and IG_γ to examine the adhesion between groups in the UN. Indeed the earlier examples of the use of these indices were taken from data on voting in the General Assembly.

The Investigation of Issue Dimensions

In factor analysis the votes on various roll calls may be regarded as observations about the individual legislators (or states) and the factors generated represent blocs that vote in a similar way. Alternatively the votes by various assembly members may be regarded as observations on the nature of the roll calls. Then the factors represent groups of roll calls that reflect a common issue dimension. Nobody has yet attempted such an approach with cluster-bloc analysis, but it can in fact be used in the two comparable ways. The technique need not be limited to the investigation of the voting blocs among assembly members but can also be used to investigate whether there are any *issue sets* that evoke a common response.

To discover issue sets it is first necessary to construct agreement scores for every possible pair of the roll calls. If just identical voting is considered, then Issue Similarity,

$$I_s = \frac{\text{No. of Assembly Members casting Identical Votes}}{\text{Total No. of members voting on both roll calls}} \cdot 100$$

Or if it is decided to allow for abstentions in United Nations voting, then Issue Similarity,

$$I_s = \left(\frac{i + \frac{1}{2}h}{s} \right) \cdot 100,$$

³⁰ More detailed computations against the null hypothesis of random voting or for other specified values of p_f , p_g , and p_d will be undertaken on request.

where

i = No. of states that cast identical votes on the two roll calls;

h = No. of states partially agreed on similarity of the roll calls, i.e., voting Abstain on one and Yes or No on the other;

s = Total No. of states that vote on both roll calls.

Once the Issue Similarity scores have been calculated we wish to know which scores are high enough to give pairs of significantly similar roll calls. Two new symbols may be used.

p_i = probability of identical voting on the roll calls;

p_o = probability of opposite voting on the roll calls.

With a simple index which ignores abstentions, a legislator may vote in one of only four different ways on two roll calls (1) Yes/Yes, (2) No/No, (3) Yes/No and (4) No/Yes. With random voting each has a probability of one in four, and thus $p_i = 0.5$ and $p_o = 0.5$. The equations for Issue Similarity scores will be the same as those for voting agreement between legislators. Therefore Table 2 gives the same cutoff points for significantly similar roll calls.

If a group of roll calls form a set in which every roll call is significantly similar to every other roll call in the set, then we may take it that the roll calls all represent different aspects of a common underlying issue dimension. But according to the logic it is also possible to find pairs of roll calls that are significantly dissimilar. In this case, we would also conclude that the two roll calls represent the *same* dimension. Very often in roll-call voting there is a switch in the way those with the same attitudes must vote in order to express that attitude, e.g., a conservative American Senator who considers that India is becoming socialist might vote *in favor* of an amendment to the Foreign Aid Bill to deny all aid to India, and then when the amendment fails would vote *against* the Bill as a whole. Such a pair of roll calls would then

have a high dissimilarity and should be part of the same issue set. There must of course be a congruent pattern to any sets made up from roll calls that are significantly similar to some roll calls while being significantly dissimilar to others in the set. In general if A and B are similar while A and C are dissimilar, then B and C

must be dissimilar as well, in order to make A, B, and C form a set.

If Issue Similarity scores are calculated for United Nations roll calls including Abstain/Abstain pairs but ignoring partial similarity pairs, then $p_i = 0.3333$ and $p_o = 0.6667$. If partial similarity is included in the Index, then the probability of identical voting $p_i = 0.3333$, of opposite responses $p_o = 0.2222$, and of partially similar responses $p_h = 0.4444$, and the cutoff points are the same as those in Table 6.

Just as we may study the significant relative agreement of legislators in relation to a particular set of roll calls, it is also possible to study the *significant relative similarity* among issues. In other words, identical motions or bills formulated in an identical way may produce different results in different legislatures or even in the same legislature at a different time. Therefore, by analogous reasoning to that given earlier, values of p_i , p_o and, if necessary, p_h may be determined empirically, and the appropriate significance tables may be calculated.

Conclusions

In order to avoid being mesmerized by figures, one must reiterate the limitations that are inherent in all methods of legislative roll-call analysis. In the first place, the data consist solely of the recorded votes. While each legislator is thus making a conscious and deliberate public commitment, his recorded vote is still only a partial manifestation of the legislator's attitude. It has already been well established that in elections the individual citizen voter casts his vote on the basis of long established party loyalties or on just the broad outlines of the issues. Similarly, legislators usually vote on the broad principles of a bill, the technicalities or legal points usually being disposed of in committees. Thus roll calls do provide a coherent set of data that indicate alignments on the main issues. Judicial decision making should perhaps be treated with more caution. The strictest interpretation of the role of a judge asserts that neither political nor moral attitudes should have any effect on judicial decisions. Although in America, at least, it is well established that the attitudes of judges are reflected in their decisions, they are certainly not the only determinant factors.

Second, while the establishment of statistical significance is of great value it is not an end in itself. It does not *eliminate* the risk of error but just gives us a *known* risk of error: for example, in the UN 20th Session there were 117 states, which gives 6786 possible pairs of states. At the $p \leq .001$ level of significance we would

expect to be wrong 7 out of 6786 times in assigning agreement or lack of agreement to a pair of States and at the $p \leq 0.05$ level we would expect to be wrong with as many as 34 pairs. It may be stating the obvious, but it is necessary to stress that any voting pattern discovered on the basis of statistical significance is not necessarily one that is *substantively* significant.

Thirdly, cluster-bloc analysis is a purely descriptive technique. It does not in itself provide any explanation of political processes. Very often we may wish to use it for simple exploration, asking such questions as, What blocs exist? How stable are they over time? The results may then suggest theories or may be used to support theories, but are not a substitute for theory. Indeed with the average legislature so much data will be generated that the researcher will be lost unless he knows beforehand exactly what he is looking for. There will probably be several hundred issue similarity scores and several thousand legislator agreement scores. Even very simple propositions will help in handling this amount of data.

Cluster-bloc analysis has long been established as one of the main methods available for legislative roll-call analysis. It can now become *the* main method, especially since the widespread use of computers in the social sciences means that the vast amount of labor formerly required should no longer be a hindrance to its use. Now that the method can be shown to have a sound statistical base, there is no longer any reason for the researcher to be forced to take arbitrary decisions that may greatly affect the results. The methodology is much strengthened. The use of summary indices that give limited information about predetermined groups is no longer necessary. The data itself can be used to discover empirically what are the voting blocs, but they can also be used to study any specified group. Factor analysis need no longer be considered a superior methodology, since it requires some substantial manipulation of the data and dubious assumptions about the level of measurement and normality.³¹ Cluster-bloc analysis does not require this, and it allows the researcher to remain closely in touch with his data. Useful indices of cohesion within a group and adhesion between groups may be derived from the agreement scores.

³¹ See for example Alker and Russett, "World Politics . . .," p. 30, which uses a complicated process of "standardization" of UN voting data. See also the references in note 7, for other authors that share my unease with factor analysis.

Special relationships within the assembly may be examined.³² Finally the methodology can also be adapted to find issue sets that indicate underlying issue dimensions. If the cluster blocs are then examined on each of the issue sets, we have a very powerful method of analysis.

³² For example, by taking agreement scores between the U.S. and all small states on East/West issues in

the UN and using the cutoff points given in Table 6, it was possible to determine for each state whether it was significantly aligned or not: H. Hveem and P. Willetts, "The Practice of Non-Alignment: on the Present and the Future of an International Movement," Department Short Study, Department of Political Science and Public Administration, Makerere University, Kampala, Uganda. With factor analysis a Western bloc would be found, but there is no way of determining whether the loading on this factor for a particular state is statistically significant or not.

COMMUNICATIONS

TO THE EDITOR:

In a recent article on "Party Effort and Its Impact on the Vote" (*American Political Science Review*, 65 [June, 1971], 439-450), William Crotty claims to show that "party effort" has a measurable impact on election outcomes. Unfortunately, the evidence Crotty displays is insufficient either to confirm or to disconfirm his hypothesis. With North Carolina counties as observations, Crotty regresses the vote divisions for five offices on five demographic variables, "party competition," and eighteen measures of "party effort" (nine per party). The main problem is that Crotty chose to report only that each entry of new variables into the equations generally increases the explained variance by a margin he regards as substantial, without revealing the strengths or even the signs of the relationships between the entered variables and the vote.

For example, Crotty shows that the entry of "party competition" into the equations pushes the multiple R's 3 to 6 points beyond what they were with only the demographic measures as independent variables. If this relationship between party competition and the vote is indeed meaningful, as Crotty suggests, it would be helpful to learn whether the sign of the relationship implies that party competition causes Democratic voting or implies that it causes Republican voting. Whichever the case, the results are at variance with current notions of the range of the explanatory power of "party competition." By extension, it is desirable to learn more about what happened when the eighteen "party effort" indicators were plugged into the equations—beyond the statement that the multiple R's increased from 2 to 12 points. Even if some of the 18 partial correlations (per equation) between effort and the vote are nonsensical, positive relationships between Republican effort and Democratic voting or between Democratic effort and Republican voting, they would—as any independent variable would—add somewhat to the explained variance.

Finally, a particularly acute problem arises with Crotty's interpretation of his finding that for competitive counties alone, the multiple R increases by 17 to 38 points when the "party effort" measures are cranked in. Crotty's conclusion is that "party effort during campaigns in competitive areas has a pronounced effect on the outcomes of the races" (pp. 446/447). But no such inference can be made from the data,

because with so many variables, restriction of the analysis to competitive counties allows only 0 degrees of freedom—a level at which all results are meaningless. As indication, the footnote to Crotty's Table 5 tells that even the mammoth .98 multiple R's so derived fail to achieve "statistical significance." Indeed, with no degrees of freedom (a completely determined system), any deviation of the R from a perfect 1.00 must of necessity be solely due to computer rounding error.

In short, Crotty has applied some interesting data to an interesting problem. But the way he chose to present the findings prevents us from learning whether or not his hypothesis is correct.

ROBERT S. ERIKSON

Florida State University

TO THE EDITOR:

Mr. Erikson exaggerates. The research intent of the article is clear, and the execution follows closely the lines of development indicated at the beginning of the piece.

A number of interesting and related lines of inquiry could have been followed. Several of the most relevant of these are developed in the Appendix—including a reference to the exploration of the type of problem he finds most intriguing.

My major concern in the article, as I stated, was to explain total results rather than to trace the specific linkages. The increases in the explained variance related to the measures of party effort introduced are, in most cases, so substantial that it was hardly conceivable to treat them as the results of random variables. They also are in accord with a substantially grounded literature as well as the theoretical guidelines that introduced the analysis.

Although the major research interest was the variance accountability rather than the direction of the contributions, Table A.2 was included to accommodate the differential impacts of the independent variables.

A close inspection of this table, the citations in the notes which refer to analyses of the problem, and the Appendix more generally should provide the basic answers to the questions raised.

WILLIAM CROTTY

Northwestern University

TO THE EDITOR:

Re: Gerald M. Kramer "Short Term Fluctuations in U.S. Voting Behavior 1896-1964" *American Political Science Review*, 65 (March, 1971), pp. 131-143.

Gerald Kramer develops a most interesting study in which he seeks to explain the federal election outcomes by economic variables such as the growth of income, inflation, and unemployment. He specifically refers to his model as being in the tradition of Anthony Downs. This is perfectly correct with respect to voters who are assumed to be rational, but due to costs of information and decision act as "satisficers."

The model is, however, at odds with Downs because of the neglect of the *interdependence* between the government and voters. According to Downs:

"Since governments plan their actions to please voters and voters decide how to vote on the basis of government actions, a circular relation of mutual interdependence underlies the functioning of government in a democracy" (p. 74 of *An Economic Theory of Democracy*, N.Y.: Harper, 1957).

This interaction is not only at the heart of the Downsian model, but an essential part of most political theories.

Thus there are two relationships to be considered:

- (1) voters' reactions to economic conditions, and
- (2) government's influence upon economic conditions in reaction to anticipated election outcome.

The fact that Kramer's model is not, as alleged, "Downsian" is of little importance, except insofar as it might lead to misunderstandings. It is important, however, that the results and conclusions reached may be doubtful if the interdependence mentioned is taken into account. The problem which arises is the classical simultaneous equation estimation problem of econometrics. There is not only the equation of the form

$$y(t) = \alpha_1 X_1(t) + \dots + \alpha_n X_n(t) + u(t),$$

where

$y(t)$ = party A's share in the vote,
 $X_i(t)$ = economic variable ($i=1, 2, \dots, n$),
 $u(t)$ = random error of the classical regression model,

as estimated by Kramer, but also an equation describing the reaction of the government upon the vote share, e.g.:

$$X_i(t) = \beta_i y(t) + \epsilon(t),$$

where

$$\epsilon(t) = \text{random error}$$

Combining the first and second equations, the expected value,

$$\begin{aligned} E\{X_i(t)u(t)\} &= E\{\dots + \beta_i u(t)u(t)\} \\ &= \dots + \beta_i \sigma_u^2, \end{aligned}$$

will be zero only under extremely restrictive assumptions about u and E .

The formula shows that when there is interdependence, the economic variables $X_i(t)$ are no longer independent of the error term $u(t)$. It is well known (see e.g., J. Johnston, *Econometric Methods*, N.Y.: McGraw Hill, 1963, p. 141) that in that case the ordinary least-square estimation procedure used by Kramer for the first equation generally leads to biased and inconsistent results.

Kramer should have paid attention to the interdependence problem; the conclusiveness of his results must be left open. His model should be extended to take into account the reaction of governments to the expected election outcomes. To do this, a theory of government behavior must be used, a possible basis of which is Downs's vote maximizing assumption. This means that empirical political science must make the step from the estimation of single equations to whole models composed of several simultaneous equations, i.e., to move towards what may be called "*polito-metrics*."

HERMANN GARBERS and
BRUNO S. FREY

University of Konstanz

EDITORIAL COMMENT

Present and Absent. There is almost certainly a relationship between the general subject matter of articles that get printed in the *Review* at one point in time, and the subject matter of the next batch of articles that are submitted for publication. Our casual observation is that scholars who have articles up their sleeves are superattentive to what seems to be getting through the editorial maze, and may attempt to infer the editor's preferences from what ends up in print. If this impedes the *Review* from having a crack at good articles which are nevertheless unlike those we have published recently, it is our loss, and consequently we want to reaffirm that we are in business to serve the scholarly needs of every branch of our discipline.

A glance at articles appearing in the last full volume of the *Review* (Vol. 65, 1971) does reveal some meaningful clusters and some notable absences. There were 46 articles printed in Volume 65. Thirty-five of them included at least one table or figure. This attests to a preoccupation in our discipline with the manipulation of large numbers of examples. It suggests a growing collective capacity to deal with frequently occurring phenomena, including such things as the opinions of mass publics, votes, and responses to large-scale surveys. An important characteristic of frequently occurring phenomena is that properly arrayed and interpreted they can be used to assign known degrees of confidence to propositions offered as explanations for their occurrence or distribution. The fact that so much of the *Review* has recently been given over to this sort of material indicates that a sizable commitment is being made in the discipline to the expansion of what we can be reasonably sure we *know*.

Nineteen articles dealt with some aspect of political psychology or mass voting behavior, value and attitude acquisition by citizens, or low- and middle-level political participation. There is no question that we are in the midst of significant intellectual progress on this cluster of problems. Diverse perspectives are being brought to bear. Scholars are offering new data and new ways of stating questions for research. Although this is not the only main current in contemporary political science, it is without question one of the main currents. The *Review* has always had a major role in encouraging conversation and research in this area, and, apparently, this role is continuing.

Twenty articles were tied fairly closely to U.S. data; 12 were explicitly about activity in

one or more foreign countries; and 14 were not classifiable on this basis. This is a somewhat less parochial showing than one might expect from an American journal.

We classified 13 articles as "formal theory." This meant that their texts contained sentences having Greek letters or algebraic expressions other than formulas for computing statistical tests. Given the spotty mathematical training of most practicing political scientists, and the consequent handicaps that they suffer in attempting to read mathematical material, this finding attests to the willingness of the *Review* to serve as a forum for small groups within the discipline and also, no doubt, to a desire to encourage the growth of new kinds of political science.

Because there is no concomitant wish to discourage more traditional kinds of political science, one deplores the weak representation in Volume 65 of contributions in public law (one American, one comparative), public administration and organization theory (one example), policy analysis (by a generous count, three examples), political philosophy (three articles), elite politics (two articles) and international politics (two articles).

How to explain these absences? We must not rule out the possibility—although it is likely no more than a possibility—that political scientists are simply becoming less interested in these subjects, and hence are writing less about them. No doubt the *Review* is seeing less than its share of articles in some subfields because these fields have first-rate journals of their own, like the *Public Administration Review* or *World Politics*, or *Ethics*, or any one of a flock of law journals. Or, conceivably, our standards are higher in old established areas of the discipline than in the new ones.

Some imbalance in the *Review* is probably inevitable over the short run and—in the short run—this is arguably even desirable. It would, however, be a matter of some concern if over a longer time span the *Review* found itself increasingly cut off from important intellectual currents in the discipline.

We have no sure way of telling whether or not the sample of articles that ultimately finds its way into the *Review* is in fact an accurate reflection of the best among the current intellectual preoccupations of political scientists. We can be reasonably confident that what we print is on the whole the best of what we see. But we should also guard against the possibility

that we are not seeing all the different kinds of articles we should be seeing. It is not entirely clear how to go about doing this, except by pointing to the light in the window, and keeping the front door ajar.

On the Cover. One of the master strokes of the new administration of the *Review* has been to shift the bulk of complaints about the magazine from its contents to its cover. The cover of the *Review*, we feel, is something to sink one's teeth into, and although we are told it leaves a gluey aftertaste, it does provide much more nourishment than the ephemera not to say trivia that we have all complained about for so long. In view of widespread interest in the cover, we are pleased to report the following:

We carefully considered, but then rejected the idea of enclosing the *Review* in a plain brown wrapper. Our grounds were that to do so might constitute fraudulent advertising and might, under the rule enunciated by the Supreme Court in *Ginzburg v. United States*, 383 US 463 (1966) seriously discommode the Managing Editor and actually impair delivery of the *Review* to its by then panting subscribers.

From the solution of putting nothing on the cover we leaped to the opposite extreme and considered putting everything there. This, we feared, would make for a cluttered, unattractive, and well-nigh unreadable cover. It also would take from the interior of the magazine a certain element of surprise. How many prospective readers, we wondered, faced with all that exoskeletal prose, would decide not to probe beneath the surface?

Moderate counsel prevailed. It was decided to put just enough on the cover to assure the timid that indeed this publication is the *American Political Science Review*, and to help them differentiate one issue from the next by means of cleverly planted clues. For the reader with an encyclopedic mind, the first page always contains a full synoptic table of contents, and ever heftier appetites can be gratified by immediately succeeding pages of abstracts, and lists of correspondents and books reviewed. By such sneaky means we thought to draw the unwary into the journal proper. Further, we hoped to unclutter the cover sufficiently to permit a little imagination and attractiveness to enter into its design.

All this, alas, has come at a price. Some authors get only their names on the cover. Others get their names and in addition the title of their article. This is invidious. By what criteria are such marks of distinction meted out? Looking

back over the past couple of issues, it seems apparent that we can pretty well rule out alphabetical order. Can it be that the editors of the *Review* play at dice in making up their table of contents? Do they follow, God forbid, some notion of individual preference? Are authors whose names appear above the title personal pals of the Managing Editor? Do they all have some secret connection with the University of Minnesota? Are they authors of the shortest contributions, or the longest? Are they the oldest contributors? Youngest? The mind reels at the possibilities. The editors of *PS* will, we know, be grateful to receive essays from Association members which in twenty-five words or less (or the equivalent in calculus) explain the handicapping system at work in designing covers for the *Review*. Entries must be dated April 1. Winning entries become eligible for inclusion in the special 1984 edition of *An End to Political Science*. Decisions of judges are final, and will be based on originality of expression and neatness. Employees of the Association, the *Review*, and holders of Yale PhD's are not eligible to compete.

Articles Accepted for Future Publication

Paul Abramson, Michigan State University, "Intergenerational Social Mobility and Partisan Choice"

Herbert B. Asher, Ohio State University, "The Learning of Legislative Norms"

David W. Brady, University of Houston, "A Research Note on the Impact of Inter-Party Competition on Congressional Voting in a Competitive Era"

Michael Brecher, McGill University, "Images, Process, and Feedback in Foreign Policy: Israel's Decisions on German Reparations"

Eric C. Browne, University of Georgia and Mark N. Franklin, University of Strathclyde, "The Perquisites of Government: Aspects of Coalition Payoffs in European Parliamentary Democracies"

Charles S. Bullock, III, University of Georgia, "Freshman Committee Assignments and Re-election in the U.S. House of Representatives" and "House Careerists: Changing Patterns of Longevity and Attrition"

Charles E. Butterworth, University of Maryland, "Averroës: Politics and Opinion"

Peter K. Eisinger, University of Wisconsin, Madison, "The Conditions of Protest Behavior in American Cities"

Robert S. Erikson, Florida State University, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections"

John Ferejohn, California Institute of Technol-

- ogy and Talbot Page, Resources for the Future, "A Note on 'Voting or a Price System in a Competitive Market Structure' "
- Alexander L. George, Stanford University, "The Case for Multiple Advocacy in Making Foreign Policy"
- Fred Kort, University of Connecticut, "Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science"
- Carl H. Lande, University of Kansas, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics"
- Wallace Mendelson, University of Texas, "From Warren to Burger: The Rise and Decline of Substantive Equal Protection"
- Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"
- Andre Modigliani, Harvard University, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy"
- Sarah McCally Morehouse, Manhattanville College, "The State Political Party and the Policy-Making Process"
- Donald G. Morrison and H. Michael Stevenson, York University, "Intergeneration and Instability: Patterns of African Political Development"
- Edward N. Muller, State University of New York, Stony Brook, "A Test of a Partial Theory of Potential for Political Violence"
- Walter Odajnyk, Columbia University, "The Political Ideas of C. G. Jung"
- Benjamin I. Page, Dartmouth College and Richard A. Brody, Stanford University, "Policy Voting and the Electoral Process: The Vietnam War Issue"
- Bruce W. Robeck, Texas A and M University, "Legislative Partisanship, Constituency and Malapportionment"
- Thomas W. Robinson, Visiting Fellow, Council on Foreign Relations, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"
- Howard Rosenthal, Carnegie-Mellon University, "Electoral Participation in the French Fifth Republic"
- John Gerard Ruggie, Dotation Carnegie Pour La Paix Internationale, "Collective Goods and International Organization"
- Lawrence A. Scaff, University of Arizona, "Max Weber's Politics and Political Education"
- Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, University of North Carolina, Chapel Hill, "Political Socialization and Political Belief Systems: An Essay on the Theoretical Relevance of Some Current Research"
- W. Phillips Shively, University of Minnesota, "Voting Stability and the Nature of Party Attachments in the Weimar Republic"
- Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich and Craig E. Schopmeyer, Ohio State University, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models"
- John L. Sullivan, Iowa State University and Robert E. O'Connor, Pennsylvania State University, "Electoral Choice and Popular Control of Public Policy: The Case of the 1966 House Elections"
- John L. Sullivan, Iowa State University, "A Note on Redistributive Politics"
- Herbert F. Weisberg, University of Michigan, "Scaling Models for Legislative Roll-Call Analysis"

From Gold to Garbage: A Bibliographical Essay on Politics and the Environment*

CHARLES O. JONES

University of Pittsburgh

Is it surprising that a system seeking to turn everything into gold ends up turning everything into garbage?¹

My grandfather, wise but unschooled, would have responded: "That ain't no question, it's an unanswer!" Well, the spate of books on the politics of environment, ecology, and pollution contains more unanswers than a Dr. Seuss en-

- *Walt Anderson, ed., *Politics and Environment: A Reader in Ecological Crisis* (Pacific Palisades, California: Goodyear, 1970. Pp. 362. \$5.50).
Lynton K. Caldwell, *Environment: A Challenge for Modern Society* (Garden City, New York: The Natural History Press, 1970. Pp. 292. \$7.95.)
Congressional Quarterly, Inc., *Man's Control of the Environment* (Washington, D.C.: Congressional Quarterly, 1970. Pp. 91. \$4.00.)
Council of Environmental Quality, *Environmental Quality: The Second Annual Report* (Washington, D.C.: Government Printing Office, 1971. Pp. 360. \$2.00.)
J. H. Dales, *Pollution, Property & Prices* (Toronto: University of Toronto Press, 1968. Pp. 111. \$2.50.)
J. Clarence Davies III, *The Politics of Pollution* (New York: Pegasus, 1970. Pp. 231. \$6.00.)
Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal, *Environmental Control: Priorities, Policies, and the Law* (New York: Columbia University Press, 1971. Pp. 311. \$9.00.)
Harold W. Helfrich, Jr., ed., *Agenda for Survival* (New Haven, Connecticut: Yale University Press, 1971. Pp. 234. \$10.00.)
Huey D. Johnson, ed., *No Deposit-No Return: Man and His Environment* (Reading, Massachusetts: Addison-Wesley, 1970. Pp. 351. \$2.95.)
Editors of *Ramparts, Eco-Catastrophe* (San Francisco: Canfield Press, 1970. Pp. 158. \$2.95.)
Roger Reville and Hans H. Landsberg, eds., *America's Changing Environment* (Boston: Houghton Mifflin, 1970. Pp. 314. \$6.95.)
Leslie L. Roos, Jr., ed., *The Politics of Ecosuicide* (New York: Holt, Rinehart and Winston, 1971. Pp. 404. \$4.95.)
Joseph L. Sax, *Defending the Environment: A Strategy for Citizen Action*, (New York: Knopf, 1971. Pp. 259. \$6.95.)
Richard Saltonstall, Jr., *Your Environment and What You Can Do About It: A Citizen's Guide* (New York: Walker, 1970. Pp. 299. \$6.95.)

This review was written during my stay at the Center for Advanced Study in the Behavioral Sciences. I gratefully acknowledge the Center's generous support.

¹ Editors of *Ramparts, Eco-Catastrophe* (San Francisco: Canfield Press, 1970), p. ix.

cyclopedia. Why review them at all? There are practical reasons for doing so. First, text selections must be made and some of these books are better than others. Second, saving a colleague's time by sorting out the literature in a field probably serves a social purpose. Third, and most significantly, reviewing a set of books on a broad issue-area permits evaluation of (1) the base of knowledge for analyzing the substance of problems and the process of policy in the given issue-area; and (2) the application of political theories, generalizations and frameworks in analyzing emerging issues.

Though major environmental problems have been apparent since the first triumphs of the industrial revolution, environmental issues have come rather recently onto the federal domestic agenda. I have not surveyed all cases of dramatic issue emergence, but I suspect the pattern of policy response in the environmental case may be typical. Political rhetoric immediately focuses on the new issue. Participation in policy making is greatly expanded, without a corresponding increase in knowledge. Laws are passed, standards established, leaders selected, organizational changes made. Since typically, inadequate funds are appropriated, and staffing for new governmental functions is a slow process, these highly publicized symbolic actions buy time for an agency while it increases its knowledge and organizational capabilities to meet increments in authority.

Outside government, scholars and publicists also adjust priorities in an attempt to marshal knowledge on the issue. The process of information exchange is, on the whole, unsystematic, without being totally chaotic. Individual scholars tend to "do their own thing" unless grants are available. And even though public and private funding influences research priorities, there is so little monitoring of research output by government agencies and private foundations that neither proposals nor results are well coordinated or integrated. All of this supports Charles E. Lindblom's characterization of the policy process as "incremental."² Surely if we do not (or cannot) assess what we know, we obviously do not have the capability

² See David Braybrooke and Charles Lindblom, *A Strategy of Decision: Policy Evaluation as a Social Process* (New York: Free Press, 1963).

of basing policy proposals entirely on knowledge.

Still other problems face political scientists in marshaling knowledge about the environment. With certain exceptions, concentrating on substantive issues has until very recently provided few professional rewards for political scientists. Indeed, the post-World War II phase in the discipline definitely de-emphasized policy study and recommendation. For research purposes, our subject matter has been organized more by political and social institution. This is not to say the resulting generalizations have no bearing on issues and how they are resolved. But we have tended not to apply them in that way. The recent demand for issue-relevant knowledge, then, requires changed emphasis in how we do our work and how we use our results. Given the obvious importance of political intelligence to environmental policy action, if political scientists do not produce this knowledge, others will try. This fact is amply illustrated in the set of books discussed here.

What may we reasonably expect of the literature on politics and the environment whether contributed by political scientists or others? The answer to this question is dictated by our knowledge needs. These are diverse but surely include description and analysis of the following:

1. The emergence of the issue.
2. Issue characteristics (e.g., problems, conflicts, organization and access of interests, public opinion) important for policy action.
3. Costs and effects of policy alternatives.
4. The institutional base for action.
5. Existing policy, its development and implementation.³

One would not expect each book to address all these topics. Rather the list directs our attention to knowledge needs, however general, that permit evaluation of the literature at hand.

Now to the books. They appear to group themselves as follows. First are those works that review the scope of the issue—presumably contributing to our understanding of the first and second categories above. The second group includes source materials useful for all categories. These books provide little or no analysis. They compile laws, review progress, seek to determine “where we are.” A third set might be labeled “how to do it” books. Though not directly relevant to the list of knowledge needs, these works implicitly contribute to our under-

standing of the costs and effects of policy alternatives. Fourth, the economists have provided some literature of direct relevance to the first three categories above. And, finally, “the politics of . . .” books may be expected to contribute to our knowledge needs under the fourth and fifth categories. I have selected examples of each group for discussion, citing other works where appropriate.

The Scope of the Issue

These books are of several types—collections of conference papers, the “eco-scare” volumes, and general and scientific analyses of environmental issues. The conference or symposium is to scholars what hearings are to legislators or study commissions to presidents. Since attendance is limited, conference papers and remarks are published for wider distribution. Typically, these are of limited value. *No Deposit-No Return*, edited by Huey D. Johnson, and *Agenda for Survival*, edited by Harold W. Helfrich, Jr., are *not* exceptions to this rule.

The Johnson volume is a potpourri of statements by 65 of the participants at the 13th National Conference of the U. S. National Commission for UNESCO. An attempt was made by conference organizers to cover as many topics as possible. Thus, a little bit (contributions range from 1 to 11 pages) is said about a lot of things. The anthology begins with Arthur Godfrey, devoted environmentalist who finds time in his busy social-action schedule to sell Plymouths on television, entreating the conference:

It's time to survive—to volunteer for survival.
Good luck! Crud Ho!

The few useful statements get lost in such flotsam and jetsam. The book ends with several “action suggestions” developed during the meetings. These range from the obvious to the naive.

Agenda for Survival is the result of a symposium at the Yale School of Forestry. Public officials and other activists in conservation were invited to present formal lectures on various aspects of ecology. The result is a literate, if very general, call to action. The remarks by Edgar Berman on population limitation, Charles F. Wurster on DDT, and Harry M. Caudill on the compatibility of conservation and capitalism, are worth reading, but over-all the book lacks organization and clear purpose.

America's Changing Environment (edited by Roger Reville and Hans H. Landsberg) stands out in this competition. Most of the papers were originally prepared for a conference in

³ Description and analysis of policy evaluation are also necessary but must follow experience in implementing recent policy and organizational breakthroughs.

1966 and were issued as the Fall, 1967 issue of *Daedalus*. Essays have been added to this thoughtful collection for publication in book form. The principal dimensions of the environmental issue are identified in this collection (though contributions are uneven in quality). Each contributor has enough space to present his argument, with supporting data; and those participating know enough to do more than "view with alarm." Political scientists will find informative the section on "Economics and Politics," though only Aaron Wildavsky's paper is strictly about political aspects of the problem.⁴

The "eco-scare" books vary from the "horror story, damn the Establishment" types to more objective efforts to outline the dimensions of the problem. *Eco-Catastrophe* stands on the far left of any such continuum. Issued by *Ramparts*, the book is slick journalism with a purpose:

... the writers in this volume do not see the ecology crisis as merely one among a number of distinct social problems, which bear only a superficial relation to one another. On the contrary, the destruction of the living environment is seen to be an integral part of the general social crisis in America: the oppression of minority peoples, the genocidal war against the Vietnamese, and the accelerating repression of all militant forces for social progress and change (p. v).

The book contributes little to fulfilling those needs listed earlier. Although these articles contain some good reporting, unfortunately, one has no way of sifting the truth as it is from the truth as the authors want it to be.⁵

The most useful books in this first category

⁴ Other recent conference-symposium format books include Richard A. Cooley and Geoffrey Wandesforde-Smith, eds., *Congress and the Environment* (Seattle: University of Washington Press, 1970) and C. L. Wilson and W. H. Matthews, eds., *Man's Impact on the Global Environment: Assessment and Recommendations for Action* (Cambridge: MIT Press, 1970). Resources for the Future publications should also be mentioned in this category. See, for example, the very useful collection *Environmental Quality In a Growing Economy*, ed. Henry Jarrett (Baltimore: The Johns Hopkins Press, 1966).

⁵ The current list of "eco-scare" books is too long to reproduce here. Some examples: Garrett De Bell, ed., *The Environmental Handbook* (New York: Ballantine Books, 1970); Jack Bregman and Sergei Lenormand, *The Pollution Paradox* (New York: Spartan Books, 1966); Editors of *The Progressive*, *The Crisis of Survival* (Glenview, N.J.: Scott, Foresman, 1970); Clifton Fadiman and Jean White, eds., *Ecocide . . . and Thoughts Toward Survival* (Palo Alto, Calif.: Freil, 1971), one of the more responsible collections—developed from conferences at the Center for the Study of Democratic Institutions; Lewis Herber, *Crisis in Our Cities* (Englewood Cliffs, N.J.: Prentice-Hall, 1965); and Ron M. Linton, *Terracide: Amer-*

ica are those seeking to provide objective descriptions and analyses of environmental issues. Understandably, most of these are written by biologists, chemists, meteorologists, ecologists.⁶ Lynton Caldwell's book *Environment: A Challenge to Modern Society*, is the only one I know about in this genre written by a political scientist. Caldwell explores environmental quality as a social issue requiring political action. He carefully reviews the dimensions of the issue that complicate policy development and administration. He forcefully outlines the imperatives of integrative planning for environmental management. The book—combining philosophy, science, technology, politics, economics and administration—is a very useful introductory essay for courses on environmental control as well as for identifying major research needs.

Source Books

Given the changing nature of government action in environmental control, the scholar needs reliable source materials on problems, policy, personnel, and organization. The *Congressional Quarterly* has been providing this type of service on major issues for some years now. Unfortunately, *Man's Control of the Environment*, is not up to the standards set by other volumes in its public affairs series. The book reviews major environmental issues—air and water pollution, coastal pollution, solid waste disposal, land use, the power crisis, noise pollution, and other conservation problems. But coverage is maddeningly uneven. Legislation is fully reviewed in some areas, not in others. Little attention is paid to the agencies and congressional committees involved in decision making. And the coverage of major political infighting among elected leaders and lobbyists is

ica's Destruction of Her Living Environment (Boston: Little, Brown, 1970). Of course, Rachel Carson's *Silent Spring* (Boston: Houghton Mifflin, 1962) remains a classic of the "eco-scare" volumes—and for good reason. See also, Frank Graham, Jr., *Since Silent Spring* (Greenwich, Connecticut: Fawcett, 1970). All of these are more balanced than *Eco-Catastrophe*—indeed, most include very useful material for understanding the scope of the issue.

⁶ Again the list is very long, with items varying greatly in usefulness for the layman. In addition to the Resources for the Future publications, the nonscientist will find the report by the American Chemical Society's Subcommittee on Environmental Improvement, entitled *Cleaning Our Environment: The Chemical Basis for Action*, 1969, most helpful as an introduction to scientific aspects of environmental problems. A more popularly written item is that by biologist Barry Commoner, *The Closing Circle: Nature, Man and Technology* (New York: Knopf, 1971). See also William H. Whyte, *The Last Landscape* (Garden City, N.Y.: Doubleday, 1968).

spotty. One expects lumpiness in a document produced by "staff." The *Congressional Quarterly* has demonstrated in other issue-areas, however, that it can do better.

Environmental Quality, the second annual report of the Council on Environmental Quality, is an excellent source book on environmental policy and organizational developments. The report summarizes all federal, state, and local activities in pollution control, waste management, and land use. Action by industry (including expenditures data on pollution control) and citizen groups is also reviewed. Special sections are devoted to the complex economic and legal aspects of pollution control and the special problems of the inner city. The organizational changes of 1969-1970 are explained, with all recent shifts charted. *Environmental Quality* is an excellent resource—highly recommended to the scholar both for research and classroom use.⁷

Another source book of value is *Environmental Control: Priorities, Policies, and the Law*, by Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal. This study was funded by the Sloan Foundation and the Council on Law-Related Studies. It consists of three papers—a superficial review of goals and priorities by Rathjens, a rather comprehensive survey of the intergovernmental aspects of environmental control by Grad, and a summary of federal enforcement and control techniques by Rosenthal. Of these, Grad's paper is the most valuable. Establishing a sharing pattern for regulatory authority within a federal system is a tediously complex matter. Though Grad barely scratches the surface of this topic, he is to be commended for this early effort to bring some order to the maze of federal, state, and local environmental statutes. This book is a resource for the scholar, bureaucrat, and legislator—not a classroom text.⁸

⁷ Congressional documents—staff reports and hearings—are also excellent source materials. Of particular note are the hearings before the Subcommittee on Conservation and Natural Resources of the House Committee on Government Operations—*The Environmental Decade*, 91st Cong., 2d sess., 1970. See also the hearings during the past ten years before the Subcommittee on Air and Water Pollution, Senate Committee on Public Works. Hearings before the Subcommittee on Public Health and Welfare, House Committee on Interstate and Foreign Commerce, *Air Pollution Control and Solid Wastes Recycling*, 91st Cong., 1st and 2d sess. 1969, 1970, thoroughly evaluate the Air Quality Act of 1967—thereby identifying many of the dimensions of administering environmental controls in a federal system. See also the extensive hearings on population growth conducted during the 1960s by the Senate Committee on Government Operations.

⁸ Other source materials of value are Sidney Edelman, *The Law of Air Pollution Control* (Stamford,

How To Do It Books

Recent public concern with the environment has spawned several books and pamphlets on how the citizen can help by collecting garbage, turning the lights out early, refusing nonreturnable bottles, and buying unleaded gasoline. Joseph L. Sax, *Defending the Environment: A Strategy for Citizen Action*, and Richard Saltonstall, Jr., *Your Environment and What You Can Do About It: A Citizen's Guide*, are somewhat more serious efforts and deserve comment here. Sax, a professor of law at the University of Michigan, argues that the administrative agency

has supplanted the citizen as a participant to such an extent that its panoply of legal strictures actually forbid members of the public from participating even in the complacent process whereby the regulators and the regulated work out the destiny of our air, water, and land resources (p. xvii).

The remedy, Sax says, is to be found in the courts.

... the court preeminently is a forum where the individual citizen or community group can obtain a hearing on equal terms with the highly organized and experienced interests that have learned so skillfully to manipulate legislative and administrative institutions. The court is attractive too because, free of the constraints which familiarity and close dealing tend to breed, it can bring fresh insights to problems of environmental management (p. xviii).

Professor Sax adds a new wrinkle to the case for participatory democracy. One does not work through elected officials, for they are all bought off. The citizen can only put his trust in the courts, which will in turn act as catalysts to action "in the public interest."

... the role of courts is not to make public policy, but to help assure that public policy is made by the appropriate entity, rationally and in accord with

Conn.: Environmental Research & Applications, 1970); Stanley E. Degler and Sandra C. Bloom, *Federal Pollution Control Programs: Water, Air, and Solid Wastes* (Washington, D.C.: Bureau of National Affairs, 1969); Stanley E. Degler, *State Air Pollution Control Laws* (Washington, D.C.: Bureau of National Affairs, 1969); the many documents and reports produced by the Public Land Law Review Commission, Department of the Interior, the Advisory Commission on Intergovernmental Relations, the American Enterprise Institute, and the Subcommittee on Intergovernmental Relations, Senate Committee on Government Operations; and, of course, the continuing coverage and special reports in the *Congressional Quarterly Weekly Report* and *The National Journal*. Finally, several of the reports by Ralph Nader's Center for Study of Responsive Law fit this category. See, for example, John C. Esposito, *Vanishing Air* (New York: Grossman, 1970) and David Zwick and Marcy Benstock, *Water Wasteland* (New York: Grossman, 1971), which combine a great deal of information on the problems and critical analysis of government action.

the aspirations of the democratic process (p. 151).

One has a right to expect some evidence from the lawyer-author—both on the allegations and the expectations. For some reason, however, Sax does not preoccupy himself with presenting evidence. For example, he has not, as we might expect given the sweep of his allegations, analyzed the administration of environmental programs. Rather, he generalizes from a case of land use in Virginia. Admittedly the case is dramatic, and most would agree that it represents a fiasco in public decision making. Still, it is hard to take Sax seriously when he generalizes from this case to say (p. 56) that “the administrative process tends to produce not the voice of the people, but the voice of the bureaucrat. . . .” The words are so vague as to be virtually meaningless in serious discourse. And even if he bothered to define them, the story he tells about Hunting Creek, Virginia, hardly prepares us to draw the conclusions he wants.

His case for participatory democracy through the courts is further undermined by the effect of public opinion on lawmaking and policy implementation in 1969–1970. There is little or no evidence to show that the courts were the catalysts either for the surge in public interest in the environment or for the outpouring of legislation during 1969–71, at federal, state, and local levels. Indeed, these events suggest an officialdom apparently responsive to expressions of public opinion. The answer to “how to do it” in 1969–1971 appeared to be the use of rather old-style democratic politics—massive pressure on elected officials.

Professor Sax does have a point, of course, in his analysis of the court’s role in environmental policy development and implementation. And it is surely legitimate to make the case for alternative methods to enforce identifiable public rights. Citizen groups may indeed find court action a productive method for enforcing environmental standards. But Sax overstates his case against the present system, seriously detracting from the very goals he seeks to achieve in the book and limiting its utility for the scholar.

Saltonstall’s book is what it purports to be—a citizen’s guide to environmental problems and suggestions for “each person doing his little bit.” His is one of the few books in this genre to discuss environmental law and to provide a list of environmental agencies, private organizations, and congressional committees. On the other hand, the legal analysis is weak and the list incomplete. Further, attempting to clean the water and clear the air by altering toilet habits

(“turn off the faucet when you are brushing your teeth”) and spare-time activities (“plant a garden or tree”) is futile. Each individual confident he is “doing his part” may in fact prevent the active citizen pressure needed to enforce environmental controls.⁹

Economic Analyses

The economists have been analyzing the costs and effects of pollution and policy alternatives for some time. Of the contemporary literature on the subject, *Pollution, Property, and Prices*, by J. H. Dales is of considerable interest to the political scientist. Professor Dales places policy choice in the context of production and consumption—identifying some of the social and economic costs of waste disposal.

The main purpose of this volume . . . has been to put pollution into the perspective of a society-wide problem. In the vast majority of pollution problems, it seems to me, we pollute each other, so that it makes little sense to classify people into polluters on the one hand and pollutees on the other (p. 101).

The only real question is how much you and I are willing to pay in order to reduce pollution (p. 100).

Dales illustrates how economic analysis helps to “decide on the best way of implementing a policy once it has been chosen.” He charges the politicians with the responsibility of deciding what the public wants. When they judge it socially desirable to control pollution, then “they must rely on advice from others if they are to come to a wise decision” on implementing policy. Dales identifies three “strategies of implementation: regulation; subsidization; and some system of charging for user rights.” To an economist, neither regulation nor subsidization can “possibly be efficient.” Rather,

It is suggested that transferable property rights be established for the disposal of wastes. Because transferable . . . property rights always command an explicit price, the establishment of such Rights makes it easy to establish a market in them . . . the market automatically ensures that the required reduction in waste discharge will be achieved at the smallest possible total cost to society (p. 107).

The principal difficulty with this solution is its impracticality for reducing multiple-source pollution. Here Dales recommends combinations of the three strategies, plus taxation.

This is one of the few books reviewed here which realistically identifies an aspect of the environmental issue, brings social science method, theory, and knowledge to bear on the problems

⁹ One of the more practical “how to do it” books is *Ecotactics* (New York: Ballantine Books, 1970).

raised. The author does not permit his own anxiety about pollution to interfere with his responsibilities as a scholar. The book might well serve as a model to be used by political scientists analyzing the environmental issue-area. Dales's conscious use of economics contrasts sharply with the style of the political scientists reviewed here (see below) for he turns first to his discipline in analyzing a new problem; our colleagues tend to start afresh with each new problem.¹⁰

The Politics of . . .

To this point we have been primarily concerned with assessing contributions to the base of knowledge for analyzing issue emergence, issue characteristics, and policy alternatives. Not unexpectedly, perhaps, few political scientists are represented in the works cited so far. We may rightly expect them to be writing "the politics of . . ." books, however.

While the other books can be assessed for what they add to our substantive knowledge; the politics books should be evaluated for what they contribute to our knowledge needs in categories four and five—description and analysis of institutional bases of action, and policy development and implementation. In that evaluation, it seems reasonable to test whether and how the authors apply accumulated political wisdom and integrate their findings, if any, into what has gone on before. Surely, if these things do not happen, we cannot carry policy analysis very far.

One of the quickest ways to hit a new market for books is to collect relevant articles in a reader. We have such an example in Walt Anderson, ed., *Politics and Environment: A Reader in Ecological Crisis*. While the readings included in this book are not limited to process and policy matters, presumably we are to learn something about politics in using these materials. The author begins, however, by saying that

¹⁰ Other recent economic treatments include the popular collection of readings edited by Marshall I. Goldman, ed., *Controlling Pollution: The Economics of a Cleaner America* (Englewood Cliffs, N.J.: Prentice-Hall, 1967); the recent reprinting of Harold Wolozin, ed., *The Economics of Air Pollution* (New York: Norton, 1966), which contains an excellent case study of policy development at the local level; and two Resources for the Future publications—Orris C. Herfindahl and Allen V. Kneese, *Quality of the Environment: An Economic Approach to Some Problems in Using Land, Water, and Air* (Baltimore: The Johns Hopkins Press, 1965); Allen V. Kneese, Robert U. Ayres, and Ralph C. d'Arge, *Economics and the Environment: A Materials Balance Approach* (Baltimore: The Johns Hopkins Press, 1971). See also specific readings in the many collections cited in other notes.

political scientists have ignored the subject of his book. They "... have somehow managed to avoid confronting as a totality the most pressing complex of political issues of our time—those which can be called environmental" (p. 1). How can one organize collected readings then? Simple enough—include works on environmental politics by journalists, public officials, and others who do write on environmental politics. Since very few people at all write on the subject, however, the next step for producing a timely collection is to expand the meaning of politics:

... we must realize that politics is the making of decisions by human beings about the manipulation and utilization of the natural environment; it is an interaction among life forms which directly or indirectly affects other life forms. Increasingly and inevitably, politics *is* ecology (p. 9).

Now it is possible to include all materials ever written (and, despite the title, the new definition makes the book little different from other "scope of the problem" books). Anderson limits himself to the familiar environmentalists—Paul Ehrlich, Rachel L. Carson, Barry Commoner, Stewart L. Udall, Lewis Mumford, etc. As expected, political scientists do not make the list.

Is political science really so barren? A response to that question depends on what one is looking for in the literature. Professor Anderson apparently searched for studies by political scientists which confronted the environment "as a totality." Not surprisingly, his search was in vain. And yet he did not insist on that standard for other works included. Practically all of his selections treat special aspects of the environment—not the totality. Lynton Caldwell, Vincent Ostrom, Grant McConnell, Dean Mann, Norman Wengert, James L. Sundquist, Philip Foss, among other political scientists, have been studying aspects of environmental politics for some time. Nothing from their many published works is included in Anderson's volume.

Or suppose in organizing a set of readings on this issue one asked, "What do political scientists know that would be helpful in understanding politics and the environment?" Suddenly many names come to mind: e.g., Murray Edelman and Emmette Redford on regulation; David Truman on group organization and action; Don Price on science and technology; Theodore Lowi and Robert Salisbury on policy effects; Morton Grodzins and Daniel Elazar on federalism; Aaron Wildavsky on planning and budgeting; or the work of political economists like Charles Lindblom, Anthony Downs, and

Mancur Olson. And it is always worth recalling that John M. Gaus's lecture, "The Ecology of Government," was delivered over 25 years ago. It may well be that these scholars do not confront the totality of the environmental issue. Many attempt to confront the totality of politics, however. I think careful selection among their works, in combination with readings from political scientists who have worked more directly on the issue and from the ecologists and other scientists who define the scope of the issue, could easily result in an impressive volume on "politics and environment."

Professor Leslie L. Roos, Jr., meets many of the criteria for a "political science" reader on the environment in *The Politics of Ecosuicide*. Making no apology for what his discipline has to offer in analyzing the environmental issue, Roos is guided in compiling selections by three sets of questions:

(1) How are decisions about environmental problems made at the national and at the state level?

(2) What is the role of analysis in selection of the "proper" environmental policies? Might different types of analysis lead to different conclusions?

(3) What are some methods for bringing about change in the ways Americans treat environmental problems? What institutional and behavioral patterns might be altered to facilitate such change? (p. 1)

He has been more successful than Anderson in finding material by political scientists. And where readings were not available, he was able to get original articles contributed to the volume.

The sections on public opinion, the role of analysis, and the environment as a political issue, contain the most useful new material. In including Allen Schick's article, "System Politics and Systems Budgeting," Roos illustrates that an article does not have to direct full attention to environmental politics to be of value in analyzing that topic. And the chapter by Robert Dorfman and Henry D. Jacoby seeks to combine political and economic factors in a benefit-cost mathematical model of public decisions (using a hypothetical water pollution case). Those stressing the economics of pollution in policy courses will find this piece a superb complement to Dales (reviewed above).

The book does have shortcomings, judged by the criteria employed here. The sections on institutions and the bureaucracy are distressingly small (together constituting only one-eighth of the book). There is little reliance on the many scholars mentioned earlier for setting a broad framework of analysis (though many of the in-

dividual pieces draw from those scholars). And the title is unfortunate, in my judgment. It is misleading, unnecessarily so given the intended audience.

But Professor Roos deserves plaudits for a really fine effort. He has selected carefully and purposefully, and his introductory comments are most helpful in integrating the selections.¹¹

The Politics of Pollution, by J. Clarence Davies, III, is an original work. Davies draws primarily on his experience as a budget examiner of federal pollution programs and, secondarily on his skills as a trained political scientist to produce a valuable survey of the political dimensions of air and water pollution problems and policies before 1970. I want to emphasize the book's qualities. It does provide description and analysis of institutions and policy—including excellent chapters on the complexities of setting standards and getting compliance. And though the book was published just before the major organizational changes and policy developments in 1970, the background material and some of his speculations on future policy are helpful to student and researcher alike.

It should also be said that Davies does not claim political science has nothing to offer the analyst of environmental issues. Indeed, it is in part political science training that permits him to explain pollution politics. Still he makes only the most limited effort to set his findings in the context of what has gone on before. It is as though his colleagues had offered him nothing to work with in analyzing this set of issues, and no place to go with his findings. I am confident Davies does not believe that to be so. But for whatever reason, he chose not to let existing theories, generalizations, and frameworks guide his analysis. Economist Dales relies primarily on his discipline, political scientist Davies relies primarily on his experience. We profit from both but Davies had the chance to combine the enormous benefits of both.¹²

¹¹ Other readers with sections or selections on politics include George E. Frakes and Curtis B. Solberg, eds., *Pollution Papers* (New York: Appleton-Century-Crofts, 1971); Fred Carvell and Max Tadlock, eds., *It's Not Too Late* (Beverly Hills, California: Glencoe Press, 1971); and several of the listings in footnotes 3 and 4.

¹² Of course the Davies volume is "super-analysis" compared to James Ridgeway's book, *The Politics of Ecology* (New York: Dutton, 1970). Advertised as a "nationwide handbook for survival," the book is actually a very superficial account of some aspects of water pollution (principally oil spills). It is one of the worst examples of "instant books" capitalizing on a new issue.

Summary

I have sought to evaluate the contributions of these 14 books to our knowledge needs for understanding environmental politics. Overall, I am not encouraged by what I have found. Though many of the books may be useful for other purposes, they fall short of meeting the needs identified here. Those seeking political knowledge on environmental issues can save time by paring this list to five books: Caldwell, Dales, Roos, Davies, and the second annual report of the Council on Environmental Quality (with the American Chemical Society's report included, if needed, for an introduction to scientific aspects of environmental problems—see footnote 6).

I have also evaluated the Anderson, Roos, and Davies volumes by a standard of cumulative political wisdom. I did not begin reading

them with that measure in mind. Rather, it developed as I puzzled about what was lacking—particularly in Anderson and Davies. Perhaps I am unduly optimistic, but I think we have a base of knowledge that can be applied to emerging policy issues. It may not be as refined as we would like, but at least something is there. Unless we first come to know what we know, however, we will continue to despair of our ignorance and confirm our irrelevance as each new issue emerges.¹³

¹³ It may well be that the environmental issue is simply too broad for policy analysis at this time. Matthew A. Crenson, *The Un-Politics of Air Pollution* (Baltimore: The Johns Hopkins Press, 1971), and George H. Hagevik, *Decision-Making in Air Pollution Control* (New York: Praeger, 1970), study one of its many aspects. Both are highly sensitive to the need for integrating their findings with those of other policy theorists and scholars.

BOOK REVIEWS

The Common Sense of Politics. By Mortimer J. Adler. (Holt, Rinehart & Winston, 1971. Pp. xiii, 265. \$6.95.)

Audacity marks much of Mortimer Adler's writing, and no book more than *The Common Sense of Politics*. He has always believed that definite and truthful things can be said in brief compass about great philosophical subjects; and that he can and does say them. His tone is usually level and patient; he treats his adversaries with respectful seriousness. But without fatigue, he takes up, one after another, questions of moral and political philosophy and the philosophy of mind, and offers *answers*. He has some of the awesomeness of his master, Aquinas. Adler describes his own discourse merely as "not embarrassed to affirm and defend the truth of moral principles" (p. xi). But there is more than a lack of embarrassment.

We must not be put off by the writer's confidence in his own powers; or by the book's surface, which is old-fashioned. To express it condescendingly (in order to reflect some surprise): Adler's work, including *The Common Sense of Politics*, is immensely instructive. Most want to brush him aside; that would be wrong.

The aim of the present book is to set down the first principles of political philosophy. "At its very best, it consists of no more than a slim body of fundamental truths" (p. 15). These truths are arrived at when philosophy refines common sense. In political matters, common sense consists in the common response to "non-investigative" experience, experience we all have "*without* asking a single question that calls for steps of observation especially contrived for the purpose" (p. 43). Ethics, the direct study of the good life for the individual person as individual person, rests on the refinement of a common sense which is essentially the same throughout time and in all places: ethics rests on the ineluctable continuities in the human condition. Political philosophy, on the other hand, changes as the political order changes: political common sense changes with political experience. But the only changes in experience that count philosophically are the ones that contribute to the emergence of the good society, to the perfection of political institutions (government) and the other institutions of civil society (the state). Adler believes that the three great changes so far are those embodied in the creation of Athenian democracy, the American Revolution, and the Russian Revolution. The

first introduced the rule of law and made citizenship possible for some, thus destroying the arbitrary rule of one man; the second led to the gift of citizenship to all; the third announced the emergence of welfare as the highest economic ideal, and classlessness as the ultimate social ideal. Perfection is defined by the existence of a worldwide federal constitutional democratic welfare state. Only this state is adequate to the task of securing the good life for every individual in the world. Adler's assumption is that every well-intentioned person who tried to think rationally about the first principles of political philosophy would have to hold the principles Adler does.

The basis of Adler's certainty about the principles of political philosophy is found in (what he takes to be) the truths of ethics. These are as follows. The good life is the life of happiness. Happiness (a condition, not a feeling or sensation) is present when "all real goods are present" (p. 24). Some goods each individual acquires as it were on his own: goods of the body, mind, character, and personal association. Others are "mainly dependent" on the "public conditions of his life" (p. 25). These latter include liberty, safety, material sufficiency, dignity. The state indirectly facilitates the first sort, directly establishes the second sort. Goods are the satisfaction of human needs. Needs, and only needs, establish rights. Institutions exist only to satisfy needs (to allow men to pursue or enjoy happiness) and to do so by guaranteeing rights. Adler thinks it not improper to say that when the worldwide federal constitutional democratic welfare state is reached, the limits of institutional perfection will have been reached: everything required for the good life for all will be on hand. Progress thereafter would be registered only in man's "interior life" (p. 203).

The parts of Adler's system fit together in an aesthetically impressive whole (more condescension). It seems to me, however, that little in it is free of perplexity—whether it be the claim that happiness is the right name of the good life, or the derivation of rights from needs, or the presumed desirability of a single global sovereignty, or the designation of a terminus for social progress, or the trust in common sense, or the equation of philosophy with a particular philosophy. (One could quarrel with him also for what he leaves out: most importantly, the question of the *will* to live the life imagined by Adler as the good life. He

makes no reference to heroism, masochism, spite, or mysticism; all these may prey, sometimes commendably, on Adler's ideal. Does he think that humane conditions of life automatically make life endearing, even tolerable?) Quarrel one must: Adler's book deserves that response. His hard thought cannot be accepted as it is presented, as a body of self-evident truth. But it can be accepted as a serious contribution to the uncompletable work of political philosophy.

Perhaps the most striking aspect of Adler's book is the effort to refute anarchism. He says that one of his main motives is to restore belief in the present value and future perfectibility of political and social institutions. He challenges those who think people could live the good life without government (and other fixed social forms). He goes so far as to say that far from being an unnecessary or necessary evil, government is an intrinsic good. It is an intrinsic good because the greatest good—the good life for all—could not be had without it. That is because a check is needed on vice, and an agency is needed to make decisions in circumstances in which reasonable men can disagree about what to do.

I doubt that Adler has made his case. Assume government at its best, by Adler's definition: uncorrupt and perfectly accountable representative government with universal adult suffrage. It would be an intrinsic good only if we could show first that it did not have a cost, that it was a pure good; and second that though it was instrumental to important values, it was instrumental by processes peculiar to government that were themselves valuable apart from the values they served. On the first matter Adler is impatient with those who see any infringement on autonomy as evil: he rejects Mill's contention that restraint *qua* restraint is an evil because restraint is pain and pain is evil. Also, he does not think that rightly inflicted punishment presents any moral problem; but punishment is also pain and pain is evil. Government rests on restraint and punishment, and on compulsion as well. Surely we would prefer a world without their necessity. We accept them *faute de mieux*; therefore the essence of government is compromised. Government at its best would still have this cost for its achievements, and a most serious one it is. No fiction of consent can remove the curse from authority.

Suppose that matter were forgotten: are the processes peculiar to government (if any) valuable apart from the ends they serve? If not, government at its best would be at best *only* an

instrument: it would fall into the realm of the morally *indifferent*. What is needed here is a theory of the value of political action. If anything could save Adler's argument, it would be such a theory. Adler does not even hint at such a theory. He exhausts citizenship in voting. Voting *is* worth something, for all the failures of democratic government to be democratic. But it is not nearly enough to permit us to say that government, even at its representative best, would be an intrinsic good—except for those few who held office. Only if everybody took his turn in office (or played some substantial role in political processes) could we say that maybe government is an intrinsic good. But then we go beyond the theory of representative government.

GEORGE KATEB

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A Primer on Social Dynamics: History as Dialectics and Development. By Kenneth E. Boulding. (New York: The Free Press, 1970. Pp. 153. \$5.95.)

"Primer" is perhaps not the exact word for a book as subtle in its argument, as allusive, and as complex in the issues it raises as this one. But Professor Boulding is one of our most humanely literate minds in the social sciences today, and in this work as in others from his pen, the author's mind and style carry us, even the most unspecialized of us in these matters, a long way. He never succumbs to the simplistic or facile, but the subjects he deals with are made surprisingly lucid. Whatever objections I may have here and there to the contents of the book, the author's learning and commitment to substance are everywhere evident.

The chapter headings indicate clearly enough both the kind of book it is and the inherent excitement of its contents: History as system; organizers of social evolution; dialectical patterns in history; nondialectical patterns in history; the Marxist dialectic; revolutionism; how history might be written; and, as a brief appendix, the case study of Japan. They are not merely headings of chapters; they are, properly, road signs for a journey that has, as few books these days have, beginning, middle, and end.

The book is polemical. The best scholarship usually is, for no matter how insistently the ultimate referent—the reality out there—may beckon, scholarship at its best is engagement—parrying, thrusting, occasionally demolishing—other scholarship. There is a good deal of this in Boulding's book, which makes for a substantial part of its excellence. The one advantage we in the academy have over the bloody ama-

teurs who write ringingly (in the cash-register sense) about nonexistent things like "future shock" is our knowledge of what others have done, what they meant, and what their excellences and deficiencies were. Scholarship began with, not Plato, but Aristotle on Plato.

To a large degree Boulding is writing on Marx in this book, or rather on the whole set of principles concerning the nature and mechanisms of change we associate with the name of Marx. Specifically, it is to what Boulding calls dialectical philosophy—"ideologies which regard conflict as the essential process in development and therefore put a high intrinsic value on conflict, struggle, war, and revolution" (Preface)—that he directs his polemic. Not that dialectical emphases, in this sense, are without value at times in history. As Boulding says, in times when the prevailing dogmas are built around orderliness, agreement, courtesy, love, and rapt worship of the One, perspectives founded on perceptions of conflict are at least pragmatically useful. What availeth, though, when the dogmas are themselves ones of conflict all compact? To look at the mannered, self-conscious, ever more precious stylistic extremes to which our secularized, bush-league Augustines of the present day must reach if their shrillness is to be heard over shrillness, is a study in futility. If Marx were alive and young today, I rather imagine he would take his departure from the dogmatics of conflict, so richly if wearisomely the foundation stone of sociology and political science today, rather than from the dogmatics of Ricardian orderliness. That's only a guess, of course. One never knows for sure what a genuinely radical mind will do. What we do know is that Marx took on the academic idols of his day, which were those of equilibrium in the social process, and one likes to think he would today take on *our* reigning idols, which are those of conflict and disorder and their talismanic properties.

Boulding, in a sense, makes the idols of conflict his polemical point of departure. As I say, he does not dismiss conflict out of hand as an important factor in history. His major argument, nevertheless, is the superiority in history, on the whole, of nondialectical processes in the building of culture, social organization, and consciousness. In a stunningly good and simple chapter on revolutionism he makes clear, it seems to me for once and all, how absurdly inflated are all the conventional sentimentalities of the dogma of revolution. Revolution, in the political sense of the word, is what might be called the heroic dish of the whole conflict banquet circuit. If conflict is good, then revolution

is better: so might the folklore of the academic mentality in sociology and political science, or much of it, be put. There is, however, scarcely a shred of evidence to support the view that the major revolutions of modern history—French and Russian foremost—have done other than *set back* the processes of economic and social improvement which were well under way, powerfully under way, until the Jacobins and Bolsheviks, successors to the religious terrorists of post-medieval millennialism, got into the act.

I should be doing Boulding's book a disservice, however, if I left the impression that it is a tract against revolution as such. "The principal villain would seem to be the dialectical philosophy" (p. 92). So writes Boulding. It is the sustained, sometimes brilliant, often wise, always knowledgeable analysis of the whole dialectical consciousness, born of religion and its envisagement of the world as one of conflict between Good and Evil, that gives the book its extraordinary value.

What does Boulding offer as substitute for dialectical philosophy? In general, developmentalism, with its processes of genetic causation, cumulative continuity, and lineal directionality. Within this broad pattern, Boulding identifies growth of knowledge, social exchange, economic development, and what he calls the integrative system as key elements or mechanisms. It is here, with no diminution of respect for Boulding's learning and insight, that I would myself be obliged to raise a question or two. Boulding thinks of these, if I read him correctly, as counter-mechanisms to conflict, including revolution. Actually, they are not. They are descriptive rather than analytical terms, it seems to me. Broadly speaking, this is true of developmentalism as a whole. However false the dialectical philosophy may be—and I regard it as false—it at least seeks to provide a *causa efficiens*. The trouble with developmentalism is its metaphoric assumption of the normality of exactly what we need explained: change, mutation, transformation in social and cultural history.

Taking Western ideas of change in their aggregate, from the Greeks down to contemporary Marxists and functionalists, it is impossible to miss, if we look closely, the common *correspondence* of developmentalism and dialectical philosophy. Few developmentalists have been revolutionists, as Marx was. But a great many developmentalists, from Heraclitus to Darwin, Marx, and Spencer, have made conflict the efficient cause of the development—the cumulative, continuous, and genetically structured development—that was their largest perspective.

The same Marx who prescribed revolution could warn readers of *Capital* that no people can "clear by bold leaps . . . the obstacles offered by the successive phases of its normal development." Some of the arguments that went on among Bolsheviks in 1917-18, until Lenin put a stop to them, had to do with this point.

Boulding at one point observes, quite correctly, that developmentalism looks best as a philosophy of change when it is applied to large entities over long periods of times. Where it is least helpful, with its key elements of genetic continuity, cumulative and more or less linear change, and assumed directionality, is with respect to the smaller entities—specific communities, regions, and nations—and to the shorter periods of time. The reason for this—and I am not sure that Boulding sees it clearly—is that developmentalism is basically inseparable from the metaphor of growth; and it is only when we step back from the concrete, the empirical, and the temporal and let our eyes roam over great vistas of the past that the metaphor of growth can seem really pertinent. Even then, however, it tends to be merely descriptive, at worst sterile taxonomy, with faith in growth required to link the individual snapshots into a moving picture, into what we are prone to call "dynamics."

Biologists have been much more successful than social scientists in relating the concrete variations and mutations revealed by genetics to the study of comparative structure, to the grander vistas of evolution. Perhaps this is the consequence of what Ernst Mayr, in his classic studies of speciation, has referred to as the biologists' liberation from the typological thinking that tended to dominate biology until early in this century. The social sciences tend still to be heavily typological in their thinking. The continuing vogue of Marxism, of social systems analysis, of structural-functionalism, indicates that we are not much less typological in our approach to the problem of change than were our forefathers all the way back to Aristotle's *Politics*.

If we move from the social sciences and their typological systems, their passion for unified theory with respect to statics and dynamics, over to the writing of history, to historiography, we do not, alas, get much help. For history remains almost as dedicated to its own metaphor of genealogy as the social sciences are to the metaphor of growth. Boulding has some splendid observations on the writing of history in our time, but I think he misses the point that what is chiefly wrong with historiography is not just the technical problems of bias,

management of data, objectivity, and the like—ever the concern of courses in historical method—but is instead the nature of historiography itself. This nature, which was old when Thucydides gave it lasting sanction with his paradigm of the Peloponnesian War, is one of taking multiform, multilinear, and multidimensional reality and then forcing this reality into a uniform, unilinear, and unidimensional genealogical pattern—that is, narrative history. Events beget events, acts beget acts, and personages beget personages in the time-honored fashion of the epic. Instead of the chosen people, it is the chosen nation or civilization or economy or religion. The flow of time (Greenwich time, naturally) is made identical with the flow of events, acts, heroes, and whatever else historians interest themselves in.

All of this is unexceptionable in the novel, the epic, or the play, and as long as historiography is regarded as simply another art form (with somewhat more attention given to verisimilitude) its practitioners cannot be faulted. But if what we are interested in is a degree of knowledge of the mechanisms of social change comparable to what geneticists have revealed about biological change, historiography is of no more help to us than developmentalism. Each must have its unified structure, and whether this structure be one suggested by growth or genealogy, we are still prisoners of metaphor.

Professor Boulding concludes his book with an interesting appendix on Japan as a case history. I assume it is a case history in behalf of his plea for developmentalism, though I cannot be sure inasmuch as case histories individually presented never seem to me to tell us much except in the realm of the world of art—the painting, the sculpture, or the poem. If Professor Boulding—who is, among other things, a talented artist—wants to leave his limning of Japan at this—that is, an art work—I will not demur or cavil any more than I would at the limning of Japan by a Lafcadio Hearn. But if he is trying to tell us something about the dynamics of change, something comparable to what the geneticist tells us about the dynamics of change in the biological world, then I must urge that he get back to the drawing board.

ROBERT NISBET

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The Public Interest and Individual Interests. By Virginia Held. (New York: Basic Books, 1970. Pp. 248. \$6.95.)

This is a literate, gracefully written book, which is easy to read and to follow. The author's perspective is objective, her manner and

judgments are temperate, and her treatment of a wide variety of predecessors (writing directly or obliquely on public interest theory) is invariably civil. She has appraised—fairly and competently—writings separated by a considerable amount of time and geography, to say nothing of language and culture. After a brief introduction to the recent literature on public interest theory (produced mostly by Americans, and otherwise by Englishmen, during the period 1959–66), Dr. Held explains her approach. She empathizes best—and most often—with modern analytical legal positivism; and she focuses on the justification of claims in behalf of, and upon the reconciliation of conflict between, individual interests and the public interest.

Notwithstanding the seeming emphasis of her book's title, the value that preoccupies Dr. Held is individual interest(s); the public interest serves as a foil to make possible the validation and adjudication of conflicting claims of individual interest. Indeed, working throughout the book on the premise of methodological individualism, she achieves in her conclusion (p. 197) a position of really quite extreme individualism whereby—if I understand her correctly—a minority of (any) one can stalemate a plurality of 200 million other individuals, leaving preference between these two opposing interests to the determination of a vague (and in this book, undefined) ethical supernorm. Each individual is defined (pp. 36–37) to be a member of a set; a group is simply a plurality of individuals; while the universal set of individuals (apparently limited, but in an unspecified manner, by space and time) constitutes the "polity" (which eventually, near the book's end, becomes synonymous with "the political system"). As the author remarks, "The term 'individuals' traditionally and often refers to individual persons, but may, as indicated, refer to groups or sub-organizations of the polity." The interests of (such) individuals are considered to be clear and univalent: "I shall have to assume that 'x is in the interest of I_i ' and 'x is not in the interest of I_i ' are not being asserted simultaneously by the same person or authority of the same x's and I_i 's" (p. 33); and thereby the ambivalence and ambiguity of self-interest (to say nothing of group-interest), which so frequently can be observed to obtain empirically, are assumed away and cannot enter into the calculations of Dr. Held's theory (or analysis).

It is at least conceivable that one might endeavor to understand the meaning of concepts of public/individual interest on the basis of empirical evidence about how people act—what they do, and why, and with what consequences

—in attempts to invoke, justify, and reconcile claims related to these concepts; one might even premise recommendations about what people ought to do, in such circumstances, on the basis of some systematic knowledge concerning what they can be observed to do without the guidance of such recommendations. But in this book empiricism does not even extend to the analysis of claimant language behavior in relation to the central concepts; the author's purpose was to deal instead with the secondary evidence of statements by other writers about these concepts, plus a few (strictly logical) statements of her own. She undertakes, in her own words, to "grapple" with the term "public interest," which she purports to do by making a *verbal* analysis of the logic of linguistic relationships. Dr. Held's approach is, from a methodological point of view, quite clearly that of modern philosophy rather than that of modern political science.

The bulk of the book presents a typology of public interest conceptualization. In each of three chapters the author initially describes the views of the traditional philosophers (her phrase) whom she deems most meet for exemplification of each type; following which she describes the corresponding views of contemporary writers, similarly selected; finally, she proffers a "problem" in the form of a case study that illustrates inconsistencies and inadequacies of the viewpoint under analysis. "Preponderance" theory (from Hobbes and Bentham through neo-Bentleyans and welfare economists) describes how majorities aggregate individual interests and validate (in their own name) governmental decisions. But according to the author there is no way to demonstrate that the majority is ethically equivalent to the public; and empirically, majoritarian decisions rest upon mere superior force. The weakness of preponderance theory is that it leaves no room for any transcendent ideal of public, in the sense of *res publica*; neither can it entertain any concept of public interest that goes beyond mere identification with majority will. Common will theorists from Rousseau to Buchanan and Tullock take the opposite tack, and eliminate individual interests by having them gobbled up in an all-embracing bearhug of common interest, which brooks neither deviation nor dissent. Unitary conceptions of public interest (Plato, Aristotle, Aquinas, Hegel, Marx, and such contemporaries as Cassinelli and Flathman) are supposed to be different from those of common interest, but they seem to be equivalent in their empirical implications. Even from a logical point of view, both perspectives are said

(pp. 44–45) to rule out the validity of the assertion together of the following pair of statements: “ x is in the public interest” and “ x is not in the interest of individual I_i .” The main difference seems to be that by definition there can be no conflict between the common interest and any properly understood individual interest; while under the unitary theory, conflict is possible, but it is capable of only one kind of outcome, and that in favor of the superordinate right of the collectivity.

Dr. Held seeks to make up for the various shortcomings of the three predominant viewpoints by suggesting what she hopes may become recognized as a more adequate explication, her own alternative conceptualization of the public/private interest controversy. Thus her concluding chapter is cast in the context, and somewhat self-consciously in the lingo, of political systems theory (in the footsteps of Easton, and Almond and Powell); surely this is a most commendable endeavor to articulate her concerns from the modal theoretical standpoint of the political science profession during the ‘sixties. (Her concerns—none of which can be said to have been tributaries to the mainstream of political science theory during the past decade—include classical political philosophy, modern analytical legal positivism, and econometric decision theory.) The political system is important because only within it can the public interest be asserted “since it presupposes such a system to provide a decision procedure between rival claims of what is or isn’t in the public interest” (p. 173). The political system (e.g., the universal set of individuals) is itself anthropomorphized, indeed *individualized*, because “reasonable expectations may be that it will assert, as will any individual, first a claim to survival, later to growth, and that, with the progress of awareness and organization, it will become capable of choosing which among various possible futures to try to bring about” (p. 174). Such a personified polity validates public interest claims by adjudicating rival demands, on the basis of whichever is approved by “greater political authority” (p. 176). Although, as we have seen, no judgment concerning the public interest can be valid outside the political system whose decision procedures validate claims about it, nevertheless “judgments concerning individual interests may be independent of judgments concerning the public interest, and the political system can itself be judged in moral terms” (p. 183).

This turns the polity into a court of first instance, because its “authoritative” “validations” of the public interest are that only for consent-

ing adults; any “majority of one” (and I do not quote the *author* in my use of this phrase) can challenge the polity simply by disagreeing; and then, in the fashion of *John Doe* versus *The People*, both parties litigant have equal status in the eyes of the author, because “any dispute between them can only be settled by going beyond the systems of both, even when a preponderance of individuals may give weight to one side or the other” (and here I *do* quote Dr. Held, at p. 188). And that implies that there must be a “more inclusive system, containing both a political system and separate individual systems, [which] . . . might be, for instance, the ethical system of a society” (p. 188). One anticipates that the discussion will turn next to the decision-making processes of ethical systems and the validating propensities of ethical authorities (if I do not strain the metaphor too much, by insisting that it remain within the same realm of discourse as that for political systems). But here we run into a blank wall, as on the very next page where the author proclaims, “I shall not begin to discuss the nature of ethical systems, nor possible decision methods for rival claims between political systems and individual systems.”

So where does this leave us? With nonauthoritative authorities validating public interest claims that are binding upon no one, and without even an inkling as to how to cope with political dissent, beyond the suggestion that this raises an ethical question about which consensual judgments might possibly be reached by coteries of “independent and impartial scholars” whose opinions would be accorded the same kind of respect given (as the author thinks) to the judgments of natural scientists (within their own fields of appropriate expertise, of course). In her own words, “a conflict between a claim in behalf of the public interest and a claim in behalf of one or several individual interests would be resolvable at the ethical level, and might be voluntarily accepted by the rival claimants” (p. 191).

Apart from a few more pages of applications, and an appendix, Dr. Held quits at this point and no doubt I should do the same. I really don’t wish to say anything ungracious about a book whose author has manifested such equanimity toward others, but I must register my profound skepticism concerning the utility of any solution (to the problems of public-interest theory) which rests upon the personification of political systems (what price sovereignty?); which renders political systems so atomized that we are all cast back into the state of nature from which civilized mankind was to be protected by

Hobbes's sovereign (or, if a more empirical example is to be preferred, the chaos that lay beyond the walls of the *polis* two and a half millennia ago); and which invokes as its *deus ex machina* a community of dispassionate social scientists and humanists. If I may paraphrase a leading spokesman for the ideology of legalism that Dr. Held finds so attractive, "Some who profess belief in the [Public Interest] read literally what others read as allegory or metaphor, as they read Aesop's fables. [Political] symbolism is even used by some with the same mental reservations one has in teaching of Santa Claus or Uncle Sam or Easter bunnies or dispassionate [professors]." I wish that I could conclude otherwise, but nothing in Dr. Held's book persuades me that public-interest theory has changed much from what it appeared to be a decade ago.

GLENDON SCHUBERT

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Essential Works of Socialism. Ed. by Irving Howe (New York: Holt, Rinehart and Winston, 1970. Pp. 406. \$12.95.)

In his Introduction the editor stresses the "enormous" difficulties in compiling this anthology. Indeed, given the admitted vagueness of the concept of socialism and the myriad books, pamphlets, and articles written by "socialists" of all hues, the enterprise of culling "essential works" out of this accumulation probably was doomed in advance. Had the editor approached the material with a clear and rigorous framework—a requirement more easily suggested than accomplished—the reader would at least have been less confused and more stimulated. The criteria of selection guiding Howe were, however, too broad and indefinite: to choose "texts . . . that would be valuable both as political reflection and historical evidence . . . writings [which] . . . matter in significant ways, sometimes as guides to opinion and action, sometimes as inducements to argument and disagreement" (p. 1). He even decided to include writers "who may not quite be socialists themselves," a choice which, if pursued consistently, would have brought the huge additional literature on problems of socialism written by more or less sympathetic nonsocialists within his range of selection.

On the other hand Howe applies—though again not consistently—a principle of exclusion intended to save the concept of socialism from its detractors: "Apologists for both Stalinism and such post-Stalinist authoritarians as the Maoists and Castroists" who, in his view, have

"blemished the name of socialism" (p. 2) find no place in this anthology. In spite of this disclaimer, Howe does devote a section of his volume to "the Bolshevik tradition" and includes selections from Lenin, Trotsky, Bukharin, *et al.* He defends himself against the charge of inconsistency with the argument of "intrinsic political value," having just conceded that the excluded "authoritarians" are "obviously important and their ideas merit study;" and—more interestingly—"because there are also democratic components in their thoughts" (p. 2). Were Howe to pursue this latter point he would have to concede that the relation between socialism and democracy has never been an unequivocal one, particularly if democracy is defined in terms of parliamentarism and the existence of a multiparty system. Starting with Marx and Engels there have been hardly any major socialist thinkers—including Luxemburg, Kautsky, and Bauer—who were unambiguously committed to what Luxemburg derisively called "parliamentary cretinism." Is it not misleading to include her famous criticism of Lenin's and Trotsky's dictatorial methods but to exclude her approval of the use of force against "the class enemy?" Or to break off the selection from her reply to Bernstein (p. 120) just before her reference to the "chickencoop of bourgeois parliamentarism" which Bernstein had considered—falsely, in her view—the "appropriate organ for the most tremendous world-historical transformation?"

The recent development of an "extra"-or "anti-parliamentary" movement on the Left thus represents, in my view, the return to an old socialist tradition merely temporarily interrupted by the liberal consensus of the postwar period. By dogmatizing the relationship between socialism and parliamentary democracy instead of perceiving it as problematical, Howe fails to explore some of the most significant controversies; and he scarcely confronts the very real problem giving impetus to the socialism of the New Left: the apparently built-in incapacity of overcoming the capitalist system by means of parliamentary reforms. However open to criticism Herbert Marcuse's attack on liberal tolerance and the alleged "totalitarianism" of Western democracies may be, his writings surely are "essential" for an understanding of the present-day discussion among socialists who have moved beyond the Stalinist experience and once again question the values of parliamentary democracy which Howe postulates.

Although it is easy to quibble with the specific selections made by the editor for a volume

of this sort, I do believe that there are some objective grounds for my impression that his choices do not effectively guide the reader to the major topics which have dominated socialist controversy since the mid-'sixties. Thus the "Young Marx" of the "Paris manuscripts" with his emphasis on "alienation" is unrepresented. It would have been meritorious—and indeed essential, particularly for the American reader unfamiliar with the philosophical Marxism of Central Europe—to include passages from Lúkács' "History and Class Consciousness" as well as from the writings of the so-called "Frankfurt School" (Horkheimer, Adorno, Habermas). The attempt to fuse psychoanalysis with Marxism—prominently reflected in the work of Marcuse, Fromm, and Wilhelm Reich—surely should not have been totally neglected. Missing also are selections from the writings of Serge Mallet, Ernest Mandel, and André Gorz, all of whom have made serious and influential attempts to adapt Marxist economic and sociological categories to the analysis of contemporary capitalist systems. Also striking is the absence of the entire anarchist tradition within the socialist camp. Although Marxists' attacks on anarchist positions are cited in several instances (Plekhanov, Lenin), thinkers who articulated anarchist ideas (Proudhon, Kropotkin, Sorel, Goldman, etc.) are not represented, in spite of the significant role which anarchism has played in the theorizing of the New Left.

One additional criticism: I find it annoying that in most of the introductory notes provided by the editor neither the precise edition nor the pages of the original source from which the selection is taken are stated. The reader wishing to go to the original is thereby severely handicapped. In spite of its many very interesting selections, the question remains what utility such a big—and expensive—volume has, and for what specific group of readers it is intended.

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Political Conflict: Essays in Political Sociology.

By Morris Janowitz. (Chicago: Quadrangle Books, 1970. Pp. xii, 271. \$2.95.)

A scholar who collects in book form a series of papers written at different points in time and addressed to different audiences runs the risk that his readers will not discern a unified theme and will consider the work a mixed bag rather than an integrated contribution. On the whole Janowitz has managed to avoid this risk, although he has not been completely successful.

Despite the diversity of topics discussed, a

set of underlying ideas links the different essays. Janowitz is critical of a mode of sociological reasoning and analysis in which political phenomena are seen as being mainly epiphenomenal. Against a view that considers processes of social stratification to be the heart of the explanation of political phenomena, he urges an "institutional" approach in which formal and informal political organization, as well as political leadership, "are seen as relatively independent and voluntaristic elements of political change." Janowitz wishes to approach political phenomena with an eye to their distinctive characteristic rather than attempting to dissolve their peculiarity by way of reduction to alleged subjacent dynamics. In this view, the political sphere has at least relative autonomy, and is not simply a social byproduct.

This analytical focus on the peculiarities of politically relevant institutions informs most, though not all, of these essays. A survey of the study of "Mass Communications and the Political Process" stresses that such study cannot proceed fruitfully unless one recognizes that the media are in fact crucial instrumentalities for both social control and social change and not simple reflections of sociopolitical factors. A fine comparative study of "Social Cleavages and Party Affiliation" in Germany, Great Britain and the United States dissents from the view that continued economic expansion will eliminate political cleavages so that the politics of the middle-mass will be devoid of ideological character. By pointing to persistent and even increasing sources of cleavage in regional, linguistic, and religious factors, as well as in foreign affairs, Janowitz challenges the "end of ideology" thesis; he also identifies those institutional forces which, despite a certain leveling of the stratification pyramid, are fostering the emergence of new political issues. New types of political leaders who utilize these cleavages, even when they attempt to overcome them, will shape the polity into novel patterns.

In the chapter on "Military Elites and the Study of War," the author, who is, of course, the foremost sociological analyst of modern military institutions, is at pains to establish the distinctiveness of these institutions. Even though war making might be profitably studied as a special case for the application of a general theory of social conflict, Janowitz argues, it is still necessary not to overlook the particular features of military organizations. Even though modern military institutions must be seen as responsive to a variety of pressures coming, *inter alia*, from the technological and organizational sphere as

well as from changing modes of indoctrination and changing relations to the class structure, it behooves the analyst to consider the military establishment as a distinctive totality, not as a mere reflection of underlying forces.

Janowitz develops a suggestive typology of political-military elites. He distinguishes the *artistocratic model*, in which political and military elites are socially and functionally integrated, from the *totalitarian model*, as in Stalinist Russia and Nazi Germany, in which a revolutionary elite of relatively low status controls the military professionals and destroys their organizational independence. Janowitz also describes a *democratic model*, in which civilian control is exercised through parliamentary institutions while the military, guided solely by professional standards, is being insulated from political contentions; in a second pair of comparisons he distinguishes between this democratic model and Lasswell's *garrison state model*, in which civilian supremacy over the military has been so weakened that the military exercises effective political and administrative power. The first two types conceptualize modes of civilian-military relationships in historical experience, whereas the last two are merely goals that certain political decision makers strive to attain. It would be interesting to have the author assess how at this point in time, fifteen years after this essay was written, he assesses the chances of implementing the democratic, as against the garrison state model. He might be a bit less sanguine now than he was then.

Space does not permit more than an enumeration of the topics covered in other chapters. Janowitz discusses "Social and Political Consequences of Social Mobility," "Civil-Military Relations in the New Nations," "Patterns of Collective Racial Violence," and "Native Fascism in the 1930s." Still another set of papers deals with the methodological dimensions of political inquiry and with relations between sociological analysis and social policy. In this regard, Janowitz contrasts an enlightenment model of social science and an "engineering model," which sharply distinguishes between theoretical and applied research and views the applied scientist as the doctor who cures society or as the engineer who adjusts it. He prefers the enlightenment model, which assumes that "the sociologist recognizes that he is interacting with his subject and a variety of publics to which he must be responsible" (p. 251) and, where "his work has an impact on himself, and his findings influence his subjects and his public in an ongoing fashion" (p. 251). I have some

doubts about whether this distinction is sufficiently sharp-edged to classify unambiguously the heterogeneous tribe of contemporary sociologists, but it remains true that Janowitz's work is a major contribution to sociological enlightenment.

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The Challenge to Systems Analysis: Public Policy and Social Change. Edited by Grace J. Kelleher. (New York: John Wiley & Sons, Inc., 1970. Pp. 150. \$9.95.)

This book represents Volume 20 of the Publications in Operations Research (Operations Research Society of America), edited by David B. Hertz. Given the current intolerable confusion over labels involving the concept of "systems," this additional information is necessary to alert us to the fact that the "systems analysis" of the title is the more narrowly circumscribed, quantitative, and applied brand, and not the rather distantly related "systems approach" as found in the works, for example, of Deutsch, Easton, and most other academic social scientists.

"The Challenge" of the title refers to the attempted application of this systems analysis to major social problems and public policy. And challenge indeed it is: "... across-the-board urban and suburban problems, pollution, safety, communications, education, population, and food."

Part I consists of five introductory chapters devoted to short histories of operations research and systems analysis, and expression of high—but tempered—hopes that these tools can meet the challenge. The authors include the President of Lockheed, two members of IDA, the Director of M.I.T.'s Operations Research Center (and pioneer in that field), and a Congressman. The remaining ten chapters, making up Part II of this small book, are likewise written largely by nonacademicians, that is, by people involved in public offices, agencies, or institutes directly faced with the social problems "on a day-to-day, shirtsleeve basis"—as Mayor Henry W. Maier of Milwaukee puts it in his chapter on "The Troubled City."

It is not really possible to comment on any of these short, substantive chapters individually, in terms of the main theme of the book. In one or two cases, "systems analysis" is not discussed at all, and in most others only a gesture is made toward either the application of such an approach, or even the delineation of the social problem in such a way as to suggest seriously its applicability. It would be going too far

to say that some of these authors are grasping at any straw, or that they have been sold a bill of goods.

For whereas operations research or "systems analysis" clearly has been effective against some kinds of problems, and promising for others, it equally clearly faces difficulties when it comes to social and political problems. First, it requires a clear, precise definition of objectives or goals, against which alternative paths or solutions may be assessed and optimized. Though this precision may be possible for the goals of a military hardware system, it is all too often hard enough to agree on the mere definition of a sociopolitical problem (a more revealing term than simply "social problem"), to say nothing of what its goal solution might look like. Second, all the important components or variables and their interrelationships must be clearly specified if solutions are to be found or goals realistically attained. In other words, the relevant system, its internal structure and dynamics, and significant environmental interchanges must be ascertained. It appears doubtful whether we are yet able to specify adequately the systems relevant to most sociopolitical problems with the precision possible with the military systems that "systems analysis" evolved to handle.

Third, since this kind of systems analysis is a "cost-benefit" type of analysis, usually involving alternative mixes of different values of the relevant variables, we face the tough problem of agreeing on some standard (dollars?) against which human values and social "utilities" can be compared. (A felicific calculus of an "acceptable" level of a few million unemployed against a certain level of business investment seems only slightly less distasteful than Kahn's malefic calculus on thermonuclear war.) And last, "systems analysis" implies a degree of quantification and availability of mathematical techniques that are unrealistic, at least at present, as far as sociopolitical problems are concerned.

In general, then, we can argue that as a system becomes more "humanistic" and less material, more open and less confined, more democratic and permissive and less authoritarian and constrained, the main criteria for successful "systems analysis" become less attainable or even less pertinent. More often than not, this approach lacks two central dynamic elements. It does not systematically consider that changes in technologies, in cultural variables, in environmental constraints, and especially in the very goals of a social system, render the initial system out-of-date and vitiate the problem-

solving activities before they have concluded. This means that any effective model must embrace the morphogenic features of social systems. Second, "systems analysis," especially as it shows up in this book, operates in a political void, forgetting that sociopolitical problems involve finding solutions for at least two major dimensions: the social problem itself, and the "political problem" that may act to promote or hinder it. And more and more of us are beginning to see the political problem as much the more difficult one.

WALTER BUCKLEY

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The Legacy of Max Weber. By L. M. Lachmann. (Berkeley: The Glendessary Press, 1971. Pp. ix, 150. \$5.50.)

These sensitive essays flow from a social behaviorism akin to Max Weber's, seeing human society not as a system which acts in its own right as an autonomous entity, but as the sphere of strategies pursued by individuals, alone or in concert. Professor Lachmann's express purpose was to replace the ideal type as the central concept in Weber's theory of action, to develop a general theory of institutions from fragments in Weber's writings, and to explore alternatives to Weber's relativism in the sphere of political behavior.

All social science, but economics in particular, according to Lachmann, can benefit by dropping the view that economic behavior is a field of forces moving toward equilibrium, and by returning to the view advanced by Weber that human behavior is distinguished by the plans men pursue and the unintended consequences that may follow from their plans or from conditions outside their plans. Lachmann proposes to analyze human action in terms of plans (estimated on the basis of *verstehen*) and unintended consequences; such an analysis constitutes the praxeological method.

The praxeological method, or analysis of behavior in terms of plans and unintended consequences, poses the problem of Professor Lachmann's second essay. How is one to circumvent the rather chaotic character that human behavior displays when viewed as a field of endlessly varied, sometimes whimsical and misguided individual and collective plans and the various unintended consequences they may entail, as well as circumstances the plans fail to take into account. Such a view would seem to imply a world in which holes and gaps, strains and discrepancies were ever present. Lachmann's answer is that one must go beyond Weber to a general theory of institutions as a system of

presupposed arrangements which arise either as expediences in the course of trial and error or as a more or less enduring precipitate of past planning. The system of institutions provides the element of continuity and predictability mitigating the chaotic character of experience.

His second essay, in turn, leads Lachmann into his third, the problem of continuity and change in the sphere of political behavior. Weber's view of political life, we are told, rested on the assumption that all events in this sphere of human endeavor are determined by confrontations and encounters in which all arrangements finally reflect ratios of power. In Lachmann's view such unqualified relativism implies that even the structure of democratic institutions could erode in time. Lachmann proposes that while all political life is a matter of compromises, they vary in durability and scope and the grand compromises of interests provide long-time continuity as against the endless variations of lesser clashes of interest.

Lachmann has so sensitively appraised the contemporary relevance of some of Max Weber's notions that I find myself reluctant to criticize his formulations. He has, however, tended to "derive" Weber's position somewhat too narrowly from the economic literature of Weber's time, failing to take full account of the richness of Weber's response to historians, political scientists, linguists, students of jurisprudence, and philosophers between whom the neo-Kantian and neo-idealistic discussion was raging. Intellectually Weber was a conservative, reluctant to drop any position so long as it retained explanatory power. Hence, in the compass of his mighty personality many conflicting directions were comprised. Weber belongs to those thinkers who brood in the night as they seek resolution of the valid claims of alternative theories. For this very reason Weber would have felt, I am sure, that Lachmann has oversimplified his picture of human action and its analysis, missing some of the major problems of *verstehen* and rejecting ideal types (which are not necessarily alternative to operations of *verstehen*) which are procedures for making comparative analysis precise.

While Lachmann appears to react against Weber's intellectual conservatism, he also appears to resist his radical social realism. Weber belongs to a tradition as old as Heraclitus, a tradition that runs through such thinkers as Polybius in antiquity and Machiavelli and Hobbes in recent times. He was passionately concerned, but he always distinguished sharply between what is and what ought to be. Social forms, he believed, are cast up in the course of the ever

varying strivings of men, and no single state of affairs obtains forever. Of course, he believed that some human arrangements are more enduring than others, some compromises are fairly durable. But basically he felt that social life would always have gaps, holes, obstacles, and frustrations and that even the grand compromises only endure for a time. Weber was well aware that many men found his relativism intolerable. On occasion he observed that for those for whom the spectacle of the human condition is unendurable, the doors of the church are open.

Lachmann's sensitive probing appraisal of what is living and what is dead in the legacy of Max Weber is a welcome addition to the Max Weber library; it belongs on every student's shelf.

DON MARTINDALE

University of Minnesota

Writings on the Paris Commune. By Karl Marx and Friedrich Engels. Edited by Hal Draper. (New York, N.Y.: Monthly Review Press, 1971. Pp. 248. \$7.95.)

Birth of the Communist Manifesto. With text of the *Manifesto*, all Prefaces by Marx and Engels, early drafts by Engels, and other supplementary material. Edited, with an introduction by Dirk J. Struik. (New York: International Publishers, 1971. Pp. 224. \$7.50.)

When Marx uttered his famous "*Je ne suis pas Marxiste*," he was trying to dissociate himself from people with minds less subtle than his own who, believing they were expressing his ideas, were actually distorting them. For more than ninety years since then, the process of distortion has been going on. Many of Marx's critics are guilty of it. But in the two books reviewed here, the distortion is created by two adherents.

The Paris Commune of 1871 is generally regarded in the Marxist camp as the first example of a proletarian dictatorship, hence as the first unsuccessful attempt by the working class to carry out the historical scenario sketched by Marx and Engels. Lenin in *State and Revolution* made this interpretation of the Paris Commune and its significance as the harbinger of Bolshevik revolutions a virtual dogma in the communist world; and the hundredth anniversary of the Commune was celebrated accordingly in the spring of 1971.

The facts, carefully presented by Hal Draper in this excellent collection of relevant documents, are that Marx and Engels repeatedly warned against the uprising of the lower class

in Paris; they did so elaborately and in official documents published by the First International. While the Commune (and similar communes in other French cities) existed, and after it had been drowned in blood, they sharply criticized its leaders, its participants, and some of their policies. As leaders of the First International they adopted a highly ambivalent attitude: They sought to associate the International with the martyrs of the Commune, but at the same time they remained eager to dissociate the International from the mistakes the Commune had made. In its final form, Marx's Third Address to the members of the International Workingmen's Association (published in June, 1871) in fact says precious little about the Commune, having eliminated much analytical material contained in the first draft. Instead, it concentrates on the duplicity and cruelty of the provisional government in Versailles, reserving little more than vague words of appreciation for the men and women of the Commune. In later years both Marx and Engels alternated between approval and criticism, depending on the occasion of the remark. On the whole, an attitude of skepticism and criticism persisted.

The care with which Marx and Engels tried to express their attitude toward this event provides important keys to understanding their revolutionary scenario. There are other interesting matters in these commentaries on a convulsive current event, particularly the theory of the bourgeois state which Marx develops, both in his first Address (delivered 26 June 1870) and, later, in the first draft for the Third Address, written down between March and May 1870. (Interestingly enough, one of these pieces is almost totally an exercise in organization theory, which sounds rather un-Marxian.)

Draper has assembled this material very well. He has chosen judiciously from the countless casual references and picked significant ones. He has rendered a great service by making available to the English-language reader the hitherto untranslated early drafts by Marx. But he is so blinded by the dogma accepted in the Leninist movement that he sticks to its tired interpretation despite all the evidence to the contrary he himself has gathered. Clearly, the caution and sophistication of Marx and Engels, their ability to view their own past misjudgments in perspective, is lost when ardent followers turn their works into holy writ.

The *Manifesto of the Communist Party* surely needs no new review. It is useful to have it presented, as Dirk J. Struik has done, not only with the introduction written for various editions by Marx and Engels, but also with

first drafts, with other companion documents, and with the authors' own accounts of its history. Here the key document is Engels's well-known statement about the history of the Communist League from his *Enthüllungen über den Kommunistenprozess zu Köln*, in which he summarizes how he and Marx got acquainted and began collaborating. As usual, Engels here grossly underrates his own essential contribution, even though he does hint at the facts: namely, that he introduced Marx to the study of the working class and the classics of political economy. When this statement appears side by side with the first drafts of the *Manifesto*, which Engels wrote in the form of catechism, his importance is made even clearer. Indeed, Struik could and should have added several other pieces by Engels, such as the articles he published in *The New Moral World* between November 1843 and May 1845; the lectures he gave at Elberfeld in February of 1845; or the *Outlines of a Critique of Political Economy* published in 1844 in the *Deutsch-Französische Jahrbücher*. It would then have become even clearer that Engels may in fact have been the chief author of the *Manifesto*. But such a conclusion might be painful to a Marxist.

The introduction Struik has provided for this useful volume has virtues and faults. First, his Marxist method has led him to write a splendid survey of the historical background; but the same ideological commitment also makes his discussion of Hegel arid and abstract. Indeed, it is such mumbo-jumbo that I dare any reader, even the most intelligent, to summarize it in his own words. Yet even though Struik clearly is committed to Marxism, he is obviously not a specialist in it; and this lack of expertise shows in a host of subtle ways, any one or few of them too petty to pick out. Further, an uncritical acceptance of the *Manifesto* itself leads him to exaggerate the importance of those persons and groups described and criticized in section III of the *Manifesto*.

Finally, like many adherents, he diminishes the greatness of Marx and Engels by overpraising them or trying to explain away some of their many and inevitable mistakes in judgment. Marx and Engels have always embarrassed their followers who, when encountering statements not in line with orthodox interpretations of the ideology, are forced to make lame explanations or, worse yet, allow themselves to praise Marx and Engels for their inconsistencies by calling them flexible or able to learn. That always makes the follower look a bit silly. He looks silly, too, when he takes some random remark of no great weight made by one of the

two founding fathers and praises it for subtlety or profundity, whereas had someone else made it, he would recognize it as shallow and inconsequential.

Without their introductions, both of these books under review are valuable additions to any Marxologist's shelves.

ALFRED G. MEYER

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The Design of Discord. By Elwin H. Powell. (New York: Oxford University Press, 1970. Pp. iii, 247. \$7.50.)

The Design of Discord is a collection of the author's essays that seeks to "deepen and amplify" the meaning of the concept of anomie. This is done, quite successfully and quite indirectly, by focusing on the manifestations of anomie that occur at the individual, societal, and international level. At the individual level, anomie is displayed most dramatically in the act of suicide, and hence successive essays focus on Durkheim's work on suicide; on the relationship between occupation and suicide; on suicide notes left behind in Tulsa, Oklahoma; and on the relationship between race and suicide. At the societal level, successive essays locate anomie in the nature of cities in general and of Buffalo, New York, in particular (between the years 1810-1910); in the incidence of class conflict in Buffalo (1910-1920); and in the incidence of crime in Buffalo (1830-1970). At the international level, anomie is related to the nature of war; the relationship between internal strife and foreign conflict in Europe (1848-1918); the collective suicide of World War I; the "suicidal war cult" of Nazism; and in the integrative and disintegrative forces created by war. All the essays draw together large amounts of previously published material, some rely heavily on new data, and most have not been published previously. In other words, this book does meet its objective: to "deepen and amplify" the meaning of the concept of anomie.

This deepening and amplification, however, is a mixed blessing. For this book succeeds in elaborating the concept of anomie to the point of obfuscation, since the concept is used metaphorically for whatever images are conjured up by acts of violence and situations of conflict. For example, in writing of "the public turmoil of our time," Powell tells us that anomie is "the discord in the rhythm of social life" (p. i); for suicide, anomie is "the loss of purpose" (p. 5) . . . "a general aimlessness accompanied by feelings of emptiness" (p. 8) . . . "the experience of the unstructured" (p. 35) . . . "adolescence

. . . : a veritable status-wilderness" (p. 44); for urban conflict, anomie is "the essence of urbanism as a way of life" (p. 59) . . . "the terminal stage in the unfolding of the ethos of individualism" (p. 77) . . . "collective discord" (p. 98) . . . "institutional dislocation" (p. 107); for war, anomie is "general bewilderment" (p. 165) and "nihilism" (p. 173). These metaphoric images may create interesting and even, for some, satisfying insights into the nature of anomie. The criticism of such images is that they are not sufficiently clear to permit systematic inquiry. For this reason, as well as others discussed below, the analytic value of this work is severely limited.

An analysis of the effects of anomie at differing levels of analysis requires that the concept have a constant meaning across each level. With constancy of meaning, the nature of anomie manifested in internal strife can be the same as the nature of anomie manifested in foreign conflict and it is therefore possible to generalize about the effects of one variable—*anomie*—on different forms of behavior. In this work, however, instead of constancy, we find multiple connotations which constantly shift whenever the term is juxtaposed with a different form of conflictual behavior.

In addition, the effects of anomie on conflict can only be ascertained if "anomie" is something different from the behaviors it affects. In this work, however, this is not the case since "anomie" is the conflictual behavior itself. Here, "anomie" does not exist as a discrete variable since it is not viewed, either conceptually or operationally, as independent of the behaviors it purports to explain.

Caveats about the statistical measures also are in order. There are a number of reasons to doubt the unbiased nature of official statistics on suicide, class and racial conflict, and criminal or delinquent acts. For example, it is not reasonable to assume, as this work does, agreement among official standards for classifying such acts, especially among the official standards of different countries. In addition, even if precise standards were shared among different countries, they probably would not be applied uniformly within each country. Those trained to apply precise standards are more likely to be found in urban rather than rural settings; therefore, even without recourse to the concept of anomie, one is likely to find a higher suicide rate in urban settings.

Finally, the analytic value of this work is limited by its insistence on developing a single-factor theory of conflict. As such it either ignores, dismisses, or distorts the meaning of other fac-

tors that have been shown or may be shown to have analytic value. For example, in this work anomie is held to have identical effects on both domestic and foreign conflict even though the work of Rummel and others shows these to be independent phenomena. This insistence results in the excessive elaboration of the concept of anomie and in the postulation of a large number of questionable assumptions. In the end, we are left with a dubious metaphor firmly impaled upon Occam's razor.

PAUL A. DAWSON

Oberlin College

Party Strategies in a Multiparty System. By Gunnar Sjöblom. (Lund, Sweden: Student Litteratur, 1968. Pp. 293. 9.70 sk.)

Viewed from the perspective of its scientific aspirations, political science has often been accused of being more scientific in those aspirations than in the facts of their consequence. Typical of this comment are alleged failures to design inquiries that are replicable by other investigators and to integrate prior theory and evidence. This charge is commonly leveled against the social sciences as a whole, a product of a tension that exists between desires to broaden and expand the overall range of theoretical competence (the push for "general theory") and the desire to produce sound "knowable" social facts (the push for detailed, reliable, "behavioral" knowledge.) The social realities we seek to investigate are so complex as to render us impatient with the requirements for detailed and narrowly focused empirical work. At the same time the compulsion to expand the overall level of theoretical competence over social phenomena leads to a proliferation of "theories" which often run far in the lead of the empirical evidence available for any reasonable test of that theory. However one states the question, it would seem that theoretical integration stands as a major need in political analysis with as many partial approaches available as there are personal analytic needs to develop them.

In this volume, Professor Sjöblom has attempted an extensive, if a deliberately noncomprehensive, integration of existing theory relevant to the operation of political parties in multiparty systems. Heavily indebted to a general systems approach, Sjöblom organizes his work to treat such subjects as the nature of the polity and the status of parties within it, actors in the political arena and the goals which orient their actions. Following Easton he examines the concept "output variables" and the process of output determination. Finally, moving closer to

the professed interest in strategies he looks at party relations, and exploiting the notion of arenas, identifies three, classed as internal, electoral and parliamentary. In its format the book is a prize of careful organization. Disparate works on parties, electoral behavior, organizational theory, general systems theory, social action theory, and micro-economics are skillfully surveyed and brought into something approaching felicitous union.

Unfortunately, however, the book is more promising as form than substance. The introductory chapter, "Some Problems of Political Science Theory Formation," the best of the eleven, provides ample evidence that Sjöblom understands the task before him, is intimate with the many debates, both historical and current, that swirl about the theoretical literature of political analysis, and is rightfully chary about offering theory where it may be inappropriate. What he offers in its stead is a "scheme of analysis" for the study of party strategies.

What follows thus cannot in any reasonable sense be called a theory: it can be seen, however, as an attempt at systematic preparatory work for a theory (or theories) based on a probably essential aspect of the behavior of parties. The scheme of analysis is intended to show that a large number of activities by the parties can ultimately be linked to some of their basic aims and that therefore on a certain abstraction level it should be possible to find a high degree of regularity in party behavior" (pp. 12-13).

What does in fact follow this statement is certainly preparatory. Most of the middle chapters read exactly like collections of notes for some future endeavor. An area of interest is identified, defined, and discussed from the standpoint of existing literature. Critical comments are almost never directed toward that literature. These chapters take on the appearance of Sjöblom's running gloss on a literature appropriate to a subject of his interest.

Of what likely use is all this to a student in the field? If on the one hand the purpose in reading such a piece is to acquaint oneself with another's perspective of the subject matter, it may prove interesting. If on the other hand, one is looking for something genuinely integrative, or something "hard" enough to generate usable theory, the book will prove a disappointment. The book is totally non-evidential. The real world is not Sjöblom's concern. The abstractions of his framework do, however, concern him and within it he offers one vague assertion after another, rarely pausing to inquire whether these assertions are linked by a common level of generality. Declaratory assertions of behavioral predisposition are continually of-

ferred as if they were empirical propositions, but are revealed upon closer examination to be nothing of the kind. They are merely assertions that in situations a, b, c, . . . x, the actors in a given situation *will* do this or that. Within the context of an explicit theoretical framework such statements might be regarded as tentative propositions susceptible to eventual testing. In the form in which they are presented they are largely empty of meaning. As a whole the work takes on this cast. It is largely an exercise in empty classification.

DEANE E. NEUBAUER

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Economic and Political Peace. By Shirley Telford. (Portland, Oregon: William and Richards, 1969. Pp. xiv, 224. \$5.95.)

This is a remarkable book. It is at the same time a political treatise, a work of learning, and a hoax. I am not sure if it is not meant as a parody; the tone is dead serious but this may be part of the game. It is definitely a Marxist tract; all its scholarly quotations are from two authors, Marx and Engels. These are the only two names in the bibliography. Yet no Marxist of any denomination will probably agree to it.

Mrs. Telford, who professes to be a disciple of Eduard Heimann of the New School for Social Research, claims to have found a new interpretation of Marx's writings. Her message is simple: the new society that Marx had in mind centers around "associated production by competitive producers" (p. 1). It combines free markets with workers' ownership and self-government of the producers. This system "should come into existence by popular vote, not by a forceful overthrow of the capitalist-class" (p. 17).

This preference for a socioeconomic organization which has a large sector of workers' cooperation (similar to that of Yugoslavia) is of course not uncommon, at least among European Socialists. The unusual element in Mrs. Telford's argument is that she does not try to prove the superiority of this arrangement by reference to its merits, or by reference to known historical facts; her whole thesis rests upon extensive quotations from *Capital*, *Theories on Surplus Value*, *The Communist Manifesto* and so forth. This makes for odd reading.

The author rejects state communism, capitalism and national socialism and here again Marx and Engels are invoked, page after page. Even strict Marxists will be astonished by this slavish adherence to doctrine; they might want to throw in an occasional dictum from Lenin,

Stalin, Mao or other Great Masters. Naturally, most Marxists will also object to Mrs. Telford's idea that the new society will come into being without any kind of revolution.

In her "Introduction" the author states that the Russian reforms of 1965 are said to follow the theories of Yevsei Liberman, but she believes this to be unwarranted; according to her, the economic reforms are due to an American publication: *The Confessions of a Girl Economist* (1963) by Shirley Telford. "Even though the Russians and others have failed to acknowledge me as the author of my new interpretation of Marx, at least they acknowledged the importance of the ideas by accepting them" (page x). She hopes to convince Mao, who, she says, still believes in central planning. In fact, in China a good deal of decentralization is going on, and I would not be surprised if Mrs. Telford believed that this is the result of the present book's publication in 1969 and that the *Confessions* may well have triggered the Cultural Revolution.

If Mrs. Telford had written a study on whether Marx was in favor of workers' cooperation or of central planning, her contribution might have been interesting, though it seems to me that even in this more modest setting her conclusion is not quite convincing. But the author's ambitions make the book look like a joke. I sincerely hope that it is meant as such.

JAN PEN

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L'Idéologie Libérale. By André Vachet. (Paris: Editions anthropos, n.d. Pp. 567. Price not given.)

"Liberalism," when it was the thought structure of the *rising* bourgeoisie, was historically functional—in Marxist terms "progressive." In the works of recent authors, Mises and Hayek for example, it has become dysfunctional, historically and politico-economically. In the interim the word was transformed in the Anglo-American world, especially in the United States. Movements such as the New Deal appropriated it to mean reformism, even—indeed particularly—in the economic sphere. (To an extent this usage had already been en vogue in England in the era of Campbell-Bannerman and Asquith—Gladstone's Liberalism was still pretty close to the authentic one, though in political theory T. H. Green, the later J. S. Mill, and earlier the Benthamites had laid the groundwork for this change.) This transformation of the term "liberalism," especially since it did not remain confined to the popular language of politics, has, I think, been unfortu-

nate. A "mere" matter of terminology, of course. But one which has not facilitated the teaching and studying of modern political thought, where agreement on "received" terms is of no mean importance and where the job of terminological reorientation represents, at best, a sacrifice of valuable time.

Monsieur Vachet's book traces the development of liberal thought in the traditional and authentic sense—bourgeois-capitalist thought. Very appropriately it is subtitled "the individual and his property." Employing C. B. MacPherson's term "possessive individualism" to characterize it, Vachet defines this dominant aspect of liberalism as the "priority of the requirements of the economic process over the expression of individual independence and interpersonal and social relations" (p. 510). Thus man becomes enslaved by the products of his labor and consequently by those social forces that control the means of production. (No intimation that this may occur also under non-"liberal" systems of political economy. But then Vachet is writing about liberalism, not socialism.) Quoting from Marx's *German Ideology* on this point, Vachet takes it for granted that the bourgeoisie presents its own interest as the universal interest of all members of society. (Which dominant class or ruling group does not?) Liberalism as a thought structure is the expression of this claim to universality. It is thus an "ideology"—in the Marxist sense.

M. Vachet does not ignore the emancipatory and civil libertarian aspect of liberalism—its "juristic freedoms," as the late Professor Franz L. Neumann called it, such as equality before the law—which lends a partial truth to this claim to universality, so that liberalism has never been *entirely* "ideological." But he sees this latter aspect as ancillary to, essentially in conflict with (he places much emphasis on the antinomies of liberalism) and dominated by the former. Nevertheless, he closes on the optimistic note obligatory to socialists of every variety: "Liberalism as ideology and as social practice contains a dynamism or mechanism tending to explode its original intentions and to create the conditions that will overcome it" (p. 513). Thus liberal ideology could not but give rise to the various strands of socialist theory. And in the history of mankind, liberalism's successes and failures have caused new social groups to create new forms of power capable of transforming society by virtue of the material abundance made possible by the "liberal" forces of production. Here, even if one is prepared to credit this rather orthodox faith in the possibil-

ity of a truly free and "rational" society, to the extent of commenting that the evidence is not yet in, one must add that it has been a long time in coming.

As Henri Lefebvre points out in his preface, this work represents a synthesis of philosophy, history, and sociology. The synthesis is a happy and successful one. The syncretic approach helps in placing the theories within their historical contexts; in emphasizing the contradictions within the system of thought taken as a whole; in tracing the social origins and pointing up the social effects of the ideas discussed. For a broad work of exposition rather than of specialized scholarship, the analyses are not only impressively thoughtful but also exhaustive, the presentation is always lucid and often profound. The argumentation is convincing; the coverage of source materials, the choice of categories, and the structure and organization of the tome are impeccable—though the latter appears, to an American reader, excessively schematized. I think this is what makes parts of the book rather heavy going, even for one to whom the subject matter is not unfamiliar.

Vachet presents his subject via the following categories: (1) the "themes" of liberalism: naturalism (i.e., the liberal versions of "nature" and "natural law"), rationalism, individualism; (2) the "theses" of liberalism: liberty, equality, property, public safety. Within this framework, he properly places his main emphasis on the thinkers of the late seventeenth and eighteenth centuries—the Physiocrats, Locke, Adam Smith, the Encyclopedists. But he does not exclude other themes. He traces the origins of the liberal and secular ethic to the declining Middle Ages in the work of Duns Scotus and William of Occam. In his fine discussion of Hobbes he stresses, as too many others have failed to do, the role of Hobbesian "absolutism" in the making of the bourgeois-individualist tradition, and he stresses as well Hobbes's affinities with Locke, which are at least as important as the frequently overstated differences between them.

I would fault the book on only two related points: Though all the terrain Vachet takes us over is relevant, some of it has already been very well charted, so that one feels the volume could have been more compact, particularly as there is a bit too much paraphrasing and quoting from secondary works. For example, the often debated relationship between Calvinism-Puritanism and the capitalist spirit is rather too much belabored by leaning on Tawney (Weber is neglected). Laski's *tour de force*, *The Rise of*

European Liberalism, also receives more attention as a source than necessary. Other examples could be given.

Even if the work as a whole cannot be called an original contribution to scholarship, parts of it are indeed of that sort, and others excel in their analysis and formulation. I think a skillful translation of this book—not an easy task!—would be a real boon to American students in the field

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New Perspectives on Organization Theory: An Empirical Reconsideration of the Marxian and Classical Analyses. By William A. Zwerman. (Westport, Conn.: Greenwood Publishing Corporation, 1970. Pp. XX, 219. \$11.50.)

Marxist Sociology in Action: A Sociological Critique of the Marxist Approach to Industrial Relations. By J. A. Banks. (Harrisburg, Pa.: Stackpole Books, 1970. Pp. 324. \$11.50.)

During the last hundred years Marxist theories have appeared in many roles and have been applied for various purposes. Recently some social scientists have tended to translate them into nonideological, purely scientific theoretical models and have hoped to use them as a means to overcome the parochiality and lack of coherence of empirical research; the books of Zwerman and Banks are good examples of this approach, through their methodological procedures are basically different.

Zwerman confronts his empirical findings, assembled without any theoretical presumptions, with Marxist and classical business theory; Banks has taken some of Marx's theoretical theses as his point of departure and has looked for empirical data which might verify or contradict them.

Zwerman's empirical findings are an extension and amplification, on the basis of American data, of the now famous investigations of the English sociologist, Joan Woodward. Professor Woodward had proved that in British industry technological factors determined fundamental organizational features, such as the style of management, shape and form of organization, and character and application of the labor force. Zwerman's research confirmed the validity of almost all of Joan Woodward's findings and are in this respect a tribute to the methodology employed and hypotheses put forward by this outstanding British scholar.

As far as America is concerned, Zwerman is

basically right in saying that, with few exceptions, scholars engaged in the analysis of formal organizations have typically played down the pioneering work done by Marx in this field and have almost entirely eliminated Marx's theoretical perspective; conservative business management theory and functional analysis have nearly monopolized formal organization theory. Therefore, Zwerman's research will probably contribute a great deal to the shaping of new theoretical perspectives in American organization theory.

It must be mentioned that in Europe, however, the interest in technological determinants of organizational structures is general among industrial sociologists. Europeans will primarily appreciate in Zwerman's study the introduction of new variables that are highly important in contemporary industry, namely the relationship between ownership and management, the ratio of nonmanagerial supervisors to managerial staff, and the dependence on local markets. All readers will appreciate the exactitude and rigor of Zwerman's empirical approach and the elegance of his presentation.

Banks deals in his book with much wider and more general problems than Zwerman. In his deliberations on Marxist theory he proceeds from the most general assumptions to theses referring to rather specific, middle-range problems. His opinion is that the most general premises of Marxism, such as Marx's theory of social change, were not susceptible to empirical verification. He apparently believes the same was true of Marx's less general theses, such as the theory of capitalism. The theses referring to specific problems are considered by Banks as either a conversion of the processes of social change under capitalism into recognizable relationships between social classes or as the application of the more general premises of the theory of capitalism to specific problems of industrial relations. It is on that lower level that he does regard Marx's theses as verifiable by empirical procedures.

In order to verify Marx's assumptions Banks has used statistical data, results of economic analyses, and historical-descriptive studies; thus his investigations are free from the onedimensionality of survey and questionnaire methods. He seems to be aware of the numerous gaps and shortcomings of the available sources. He has been remarkably successful in avoiding this difficulty by relating some points of his analysis to the British iron and steel industry about which an enormous wealth of primary sources, including good monographs, was available.

Banks deals in his book with problems which

are either overlooked by many non-Marxist writers on industrial relations or treated by most Marxist writers in a traditional, dogmatic way. Banks's most important contribution in this respect is his analysis of the evolution of working-class political attitudes; it is in this field that he aptly confronted Marx's conclusions with the contemporary situation in the industrialized part of the world. "... the historical trend in industrial relations in Britain," he writes (p. 136), "has been not simply towards class-consciousness which will abolish capitalism, but towards what might be called 'organizational consciousness' to create permanent machinery for dealing with grievances in the industrial context."

His analysis, presented with great knowledge and deep sociological insight, leads him to accept some of Marx's views concerning the impact of industrial development on trade-union militancy. Banks rejects the notion that direct action is the only indicator of militancy; he thinks that the deepening class consciousness expressed in growing opposition to complacency and compromise is of no lesser importance.

Dealing with other problems, such as the factors shaping working-class consciousness, the patterns of trade union activity, or the character of leadership, Banks concludes that Marx's analysis, although not falsified by recent developments, needs much amplification and clarification. Moreover, in some fundamental respects Banks completely rejects Marx's assumptions. In his view, the change in the character of industrial relations was the result of the transition from capitalism to some sort of voluntary collectivism, i.e., a new type of society with different economic foundations and a new class and power structure. "Private enterprise capitalism, so far from being converted into state, municipal and cooperative enterprise has been replaced by private enterprise collectivism involving welfare state politics on a pluralistic basis" (p. 198). New functions of trade unionism, collective bargaining, programs of extending state control over industry, possibilities of industrial democracy—all these issues are discussed by Banks in the context of voluntary collectivism.

But in view of these deliberations and conclusions the problem of verifying Marx's long-term prognoses seems to be insoluble. Marx's predictions became a "self-destroying prophecy" of sorts. As far as they proved true at the turn of the nineteenth and twentieth centuries, they became codeterminants of fundamental social changes which transformed the whole

fabric of industrial societies and made many of Marx's concepts unapplicable to the new social order.

Banks argues that "collectivism" is a new type of class society in Marxist terms, but it is difficult to believe that what he calls "collectivist exploitation of surplus labor" in organizational societies may be analyzed by making use of Marx's theses on free enterprise capitalism. This means in turn that the assessment of "the possible contribution of Marxist thinking to the development of truly scientific theory of sociology" (p. 18) cannot be founded on middle range theories, that is, on Marx's views on particular and transitional structures of free competitive capitalism as Marx knew it. What seems to be particularly relevant in Marxism nowadays are not so much the answers and solutions offered by Marx, but the questions that he asked, the factors and variables he regarded as essential, and the analytical procedures he used. It is exactly this general aspect of Banks's interest in Marxism that makes his study so valuable to students of industrial relations.

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Ombudsman Papers: American Experience and Proposals. By Stanley V. Anderson. (University of California, Berkeley: Institute of Governmental Studies, 1969. pp. 407. \$3.75.)

If we are indeed "on the threshold of an Ombudsman explosion," as Professor Anderson wrote in 1969 in his Introduction to his *Ombudsman Papers: American Experience and Proposals* (p. 1), it is in no small measure due to his enthusiasm, perseverance, salesmanship, and productivity. For a dozen years or so he has been one of the institution's most impressive and consistent advocates, and he was one of that very small group of American advocates—among them Kenneth Culp Davis, Brian Chapman, Congressman Henry S. Reuss (D. Wisc.), and myself—who wrote in the late 1950s and early 1960s to try to convince America of the ombudsman's virtues long before Walter Gellhorn's well-supported and publicized works began to appear in print.

It is thus hardly astonishing that this Anderson tome—published in a somewhat odd format and type by the Institute of Governmental Studies, is—as the author frankly acknowledges—"a plea for adapting the ombudsman idea to American institutions, a plea based on the conviction that ombudsmen are humanizers" (p. 72). I wholeheartedly support his conviction as well as his plea as it applies to state and local levels as well as to such non or quasigovern-

mental sectors as the campus, corporations, and consumers. But it is especially at the bureaucratic level that the ombudsman's role is at once most needed and most promising, for given the proper climate and tools, he (or she—*bien entendu!*) can most effectively address himself to society's grievances as these are presented to him in form of complaints.

Yet, as Anderson wisely points out, unrealistic expectations must be eschewed. Governmental ombudsmen cannot create better transportation systems; build low-cost housing; eradicate unemployment; or contain "long hot summer violence." Campus ombudsmen cannot prevent the physical seizure of buildings (e.g., C.C.N.Y.); but they can—and do, as Anderson documents—serve effectively as people's watchdogs for the publicity and redress of grievances (e.g., Nassau County); for the rectification of procedural (and sometimes substantive) inequities, aberrations, and injustices (e.g., Hawaii); as fertile sources of positive, non-axe-grinding recommendations (e.g., Buffalo); and as important tone setters and morale builders (e.g., Denmark).

Of crucial importance to the institution's success, however, is not only the community's unstinting support but also the identity and personality of the ombudsman. Professor Stephan Hurwitz's unqualified, total acceptance and success as Denmark's ombudsman for fifteen years—not one of his recommendations or findings was rejected by the agency or individual concerned—was due to his consummate ability, tact, poise, sagacity, perceptiveness, and diplomatic *savoir faire*. Moreover, his unanimous selection by Parliament had been widely applauded by the public. It is difficult, of course, to emulate that example, yet we, too, ought to be capable of broadly accepted choices—we shall I hope, soon once again resolve to reduce the contemporary emphasis on a veritable Noah's Ark concept of representativeness! Without support and confidence the ombudsman cannot function in America or anywhere else; nor can he function without at least a willingness to understand the purpose and operation of the institution.

Professor Anderson has compiled a valuable collection of analytical and expository materials on both the legislative history of the ombudsman and its *modus operandi* on the American scene. He defines the institution's essential characteristics and its application; he evaluates and traces sundry proposals and implementations; and he reports specifically on its operation at state, local, and campus levels. A sensible conclusion is followed by several helpful charts,

tables, and appendices, augmented by a superb (chart-format) comparative analysis of ten civil ombudsmen offices by Professor Kent M. Weeks of the College of Wooster.

HENRY J. ABRAHAM

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The Search for an American Indian Identity: Modern Pan-Indian Movements. By Hazel W. Hertzberg. (Syracuse, N.Y.: Syracuse University Press, 1971. Pp. ix, 362. \$12.00.)

American Indians and Federal Aid. By Alan L. Sorkin. (Washington, D.C.: Brookings Institution, 1971. Pp. viii, 231. \$7.50.)

The two books under review differ in both subject matter and approach. They are united only in their significance and utility to the student eager to understand the situation of the American Indian today. Sorkin's book, a typical Brookings Institution study, will probably be of greater interest to most readers of the *American Political Science Review* than Hertzberg's book, although the latter in many ways has more permanent value. Sorkin's volume, the eighth of the Brookings Studies in Social Economics, describes and evaluates the various programs of assistance to the American Indian carried out by the Federal Government. These programs fall in the areas of education, health, agricultural development, industrial development, manpower development, property and income management, and welfare services. Each category is dealt with in a separate chapter supported by numerous statistical tables. Data are frequently drawn from unpublished sources and from interviews with officials dealt with by Mr. Sorkin in the course of his research. A preliminary chapter provides a general look at the reservation Indian (the subject of Bureau of Indian Affairs concern), both historically and statistically, in terms of population, income, unemployment, education, and the like. A final chapter suggests "A Program for Improvement."

In the Brookings tradition, Sorkin does not merely describe but also evaluates the effect of the programs he discusses, although his evaluation is invariably cautious and qualified by statements of the "on the one hand . . . on the other hand" type. Often his judgments are questionable. In discussing the Indian Resources Development Act, rejected by Congress in 1967, for example, Sorkin considers "unfounded" Indian criticism of the provision to allow the Secretary of the Interior to approve or disapprove loans over \$60,000 and the use of proceeds from trust property sold, and

Indian criticism of the provision to allow tribal land to be mortgaged to obtain project financing. In regard to the first point, Sorkin asserts that "it is unlikely that this power would have been used restrictively," and "the secretary has been quite flexible toward the investment and disposal of Indian trust funds and tribal income totaling several hundred million dollars" (p. 98). In regard to the second point, Sorkin asserts that "it would appear that the advantages of being able to mortgage Indian lands would have outweighed the disadvantages," since the ability of the tribes to borrow funds would have been enhanced and "some tribes whose sole asset is land would have been able to participate in the lendable funds market" (p. 98). Sorkin makes only a perfunctory "on the other hand" concession that while the Indian objections "appear unfounded, it may be true, as some opponents charged, that the bill would have been a prelude to termination" (p. 99). Indeed it might, and Sorkin's sensitivity to Indian cultural and psychological needs is here demonstrably weaker than his perception of an Indian future cast in a white economic mold.

Sorkin's recommendations are best when they relate to details of existing programs already accepted by Indian people. His recommendations aside, which one can take or leave, his book is a compendium of up-to-date information that is indispensable for the scholar or policy maker concerned with the present economic status of the American Indian.

The Hertzberg book provides the only detailed study of modern pan-Indian movements. The author's emphasis is on the first thirty-five years of the twentieth century, but she carries the story rapidly through the second thirty years. One might talk about the rise and fall of such movements, since many have fallen victim to the bitter factionalism characteristic of Indian movements and indeed, as Dr. Hertzberg points out, of all reform movements. Hertzberg divides pan-Indian movements into three groups: reform, fraternal, and religious. The first half of the book deals with the Society of American Indians, the most important reform pan-Indian group of the twentieth century. The Society, organized in 1911 in Columbus, Ohio, under the leadership of a white sociologist, F. A. McKenzie, dissolved in factional disputes ten years later, after compiling a significant record of accomplishment in publications, lobbying, education and the like. Although organized at the suggestion of a white, the Society excluded whites from full membership and participation in its activities. In the Winter, 1970 issue of *The Indian Historian*, published by a

newly arisen pan-Indian organization, the American Indian Historical Society, I wrote a brief description of the Society and called upon an Indian to tell the fascinating account of its rise and fall. I wrote the article because I discovered that no one I asked—including distinguished scholars, Indian and white, and officials of contemporary pan-Indian organizations—knew anything about the Society or its important journal. Fortunately or unfortunately, my call proved unnecessary, because Professor Hertzberg, of Teachers College, Columbia (although not an Indian), has amply documented the history of the Society in this book. I can confirm her statement that "the history of Pan-Indianism is almost completely unknown even to contemporary Pan-Indians, despite the fact that modern Pan-Indianism has existed for two-thirds of a century" (p. 297). I hope her book will make this statement obsolete.

Prof. Hertzberg recounts the conflict between two leaders of the Society. One was Arthur C. Parker (son of a Seneca father and a white mother), an anthropologist who became editor of the SAI's magazine, its secretary, and eventually its president. The other was Carlos Montezuma, an Apache who as a child was captured by the Pimas, was sold to a white man, and later became a doctor. The story of their conflict provides a textbook example of how internecine warfare can develop from the best intentions. Montezuma, an activist, had a simple and forthright program: Abolish the Indian Bureau and "free" the Indian from its control. After several years of skirmishing with Parker and the leadership of the SAI, which sought to influence or capture rather than destroy the sources of power over the Indian, Dr. Montezuma resigned and started his own journal which he pledged to publish until the Bureau of Indian Affairs was abolished. Needless to say, the BIA has outlasted Montezuma's publication, *Wassaja*. The Parker-Montezuma conflict is only one of many stresses that eventually destroyed the society, all recounted at length.

Prof. Hertzberg's book is based primarily on manuscript sources, most notably the Arthur C. Parker papers at the New York State Museum in Albany. (An inexplicable omission is her apparent failure to consult the Fayette Avery McKenzie Collection at the Tennessee State Library and Archives in Nashville.) The author expresses the hope that her book will spur other scholars to discover other collections of pan-Indian material, particularly concerning the fraternal—and primarily urban—pan-Indian groups. She herself deals only briefly with fraternal pan-Indianism, both because of the

scarcity of materials and because her study covers only movements national in scope. Fraternal pan-Indianism often was local in character, lacking national organization. Hertzberg is similarly brief in her discussion of the peyote cult and religious pan-Indianism, because several good studies of the movement, as eventually organized in the Native American Church, already exist.

Although the author's prose is lumbering and often diffuse, her analysis is keen and perceptive. It has, nevertheless, to be dug out of the material she presents. Thus both Sorkin and Hertzberg have given us valuable books, although we need not accept either their style or their frames of reference.

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Congress: Two Decades of Analysis. By Ralph K. Huitt and Robert L. Peabody. (New York: Harper & Row, 1969. Pp. 241. \$3.95.)

This volume includes a review of research on Congress by Robert Peabody and six previously published studies of Congress by Ralph Huitt. To judge the volume is primarily to assess Huitt's major work and secondarily to assess the present state of congressional research.

The success of a political scientist, according to Peabody, is judged by his professional peers and by his contributions to the growth of a body of knowledge and a discipline. By these standards, there is no doubt of Huitt's success. Students of Congress are indebted to him; he has strongly influenced the manner in which they perceive the institution. Political science is indebted to him; by artfully combining theory, common sense, and careful observation, he has helped to shape the discipline. Huitt's success is based largely on his intuitive grasp of men and their institutions as well as his ability to concentrate on the significant, despite the lure of the periphery. These talents are personal, not methodological. They are strikingly revealed in all his work—whether drawing on documentary sources ("The Congressional Committee" and "The Morse Committee Assignment Controversy") or relying mainly on participant observation ("Democratic Party Leadership in the Senate" and "The Outsider in the Senate") or ranging broadly and speculatively ("The Internal Distribution of Influence: The Senate" and "Congress, the Durable Partner").

Talents such as Huitt's are not randomly directed; they are guided by concepts derived from role theory. The roles of members of a committee, the role of the Democratic leader,

the norms of the Senate, and the role of the "outsider" are all explored in depth. Role concepts for Huitt are orienting, as they seem also to be for Fenno; they are not binding or limiting, as they appear to be in studies by Wahlke and Eulau and by Davidson. There is nothing as categorical, as reassuring as are "delegates" and "trustees," "tribunes" and "ritualists." Instead, roles are extremely varied and often conflicting. Nor is there a simple explanation of why certain roles are adopted and not others. As far as the committee member is concerned, party, constituency, interest group, and occupational affiliations all count. As far as the outsider in the Senate is concerned, the role Senator Proxmire chose was one suited to his personality and temperament, one he had played before, one in line with his goals, one congenial to his independent Wisconsin constituency, and one recognized as legitimate, if not popular, by his colleagues.

Huitt regards the concept of role as a means, but not the end, of inquiry. By itself, the concept falls short. A senator's interpretation of information, of facts is shaped by his preconceptions, allegiances, and perceptions. This is shown in the study of the congressional committee. But a senator's behavior is also shaped by situational factors. This is shown in the studies of leadership and committee assignments. Leadership behavior depends not only on the leader's subjective view of his relationship to the presidency, but also on his objective situation vis-à-vis the White House and members of the Senate. Which party controls the Executive, which the Congress; what policies are at stake; and how are men, preferences, and resources distributed. Much depends on Senate norms, but norms rarely require particular behaviors. They allow competing claims and permit alternative actions; and outcomes vary according to how situations are structured, what leaders and members do to restructure them, and just how they go about their tasks.

The purpose of Huitt's case studies of individual senators, including their roles and the situations in which they find themselves, is to increase his own and our understanding of Congress. Some researchers study congressmen, some analyze votes, and others fasten on roles—abstracting and simplifying as is necessary. On occasion, however, they become so absorbed by the elements that they disregard the context. Huitt is different. He never loses sight of Congress as an institution, an institution that is something more than the sum of members, behaviors, or constructs. To him Congress is complex, and he is acutely aware that "some

familiarity with senators and their world brings with it a hesitancy to say very much with certainty." Certain or not, he says much that makes sense—about the institution, how it works, and why it does.

Ralph Huitt may be pre-eminent, but he is not alone. Peabody's incisive review of research from the mid-1940s to 1967 describes a number of important contributions by students of Congress. It should be noted that some of them owe much specifically to both Huitt and Peabody, who headed the American Political Science Association's Study of Congress project, begun in early 1964. Empirical research on Congress is in a healthy state. Yet no one—and not Huitt—has produced a "conceptually clear and comprehensive theory." Peabody would like a scientific breakthrough. So would most of us. In this respect, there is little reason to be optimistic. No overarching theory is on the horizon, and surely none that is likely to do a job on Congress. Yet there is little cause for distress. Much has been accomplished in the recent past, and the payoffs have been rich. New endeavors, such as the examination of "policy systems" proposed by Huitt, promise even more. I for one can curb my appetite and happily settle for the valuable fragments of knowledge served up by Huitt and other sensitive students of Congress.

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The Basic Symbols of the American Political Tradition. By Willmoore Kendall and George W. Carey. (Baton Rouge: Louisiana State University Press, 1970. Pp. 163. \$6.00.)

Ambitiously, the authors aim to reinterpret the whole American political tradition. Central to this revaluation is the proposition that Lincoln's interpretation of the Declaration of Independence is heresy (pp. 88–94). More specifically, the interpretation of "the 'all men are created equal' clause" in the years since Lincoln has caused "a derailment in our tradition" (p. 84). A century-old "derailment"—wreck—of the train of our oldest beliefs is a serious matter. Because wrecked beliefs cause wrecked policies, the authors are not content to criticize the now dominant equalitarianism in our tradition. They also wish to reappraise the older democratic republicanism of pre-Civil War and pre-Revolutionary America. Therefore, this book is radical: it brings to light the roots of our tradition in order to make questionable their present-day fruits.

Two events justifying this questionable enterprise are mentioned. First is the crisis of

American self-understanding caused by those Lincolnians, the progressivist-revisionist historians. The version of the past first elaborated by Beard and carried further by Hofstadter, Dahl, Burns (not to mention Randall), and others teaches that all political opinions are equal or that Americanism is holding that no political opinion is un-American (pp. 5–7). Because the great American democrats did hold strong opinions about what is un-American, say the authors, these historians are out of sympathy with our past. While claiming commitment to democratic "values," progressivist-revisionist historiography in fact corrupts American democracy by not appreciating the great speeches and deeds of our political heroes. Thus the authors desire to ask again, "What is traditional amongst us?" What is oldest and best for us? The recent development of Voegelinian "vocabulary and intellectual apparatus," the second event making possible this enterprise, provides the answer to these questions. Voegelin's conception of tradition as a people's means to organize itself for "action in history" by continuously representing itself to itself through a process of symbolic differentiation inspires our authors to revalue the American political tradition.

Following Voegelin's "practice," our authoritative "symbol" is discovered: rule by the deliberative sense of the community of virtuous men, men who understand themselves to be under sacred restraints. This symbol is already present in a "compressed" or "undifferentiated" form in our earliest public document, the Mayflower Compact. All major public documents through the Bill of Rights make this symbol more meaningful, differentiate it into peculiarly American institutional, political, economic, and socioreligious symbolic forms. Some time after 1789 and before 1860, derailment occurred. Americans became primarily dedicated to "equality," to "pursuing policies designed to make 'all men equal' socially, economically, and politically." Only secondarily do they cling to "self-government through deliberative processes," e.g., to legislative supremacy. Because of Lincoln's speeches and deeds, there have been "two traditions" which are "not compatible with one another" (pp. 94–5). Kendall and Carey, it seems, hope for one harmonious tradition; they hope to "derail" that tradition "which holds to a rather extreme view of equality."

Why then do they almost ignore the large and growing scholarly criticism of progressivist-revisionist historiography? Such an oversight seems puzzling because the antirevisionist his-

toriography—for example, the writings of Adair, Stourzh, Elkins and McKittrick, Diamond, Jaffa, Wood, Kenyon, Bailyn, and later Hofstadter—largely discredits the view that we have two antagonistic traditions and so produces a more harmonious view of our tradition. Why do the authors in effect accept the most fundamental presupposition of the progressivists, the opinion taught by Hofstadter to so many that there is an “antagonism . . . between the philosophy of the Constitution and the philosophy of American democracy”? Why do they only argue that—to use Hofstadter’s terms again—“the philosophy of the Constitution” is morally and politically superior to “the philosophy of American democracy”? Perhaps their acceptance is for argument’s sake. After all, the progressivist version of the tradition is still that most often taught in high schools and colleges. But does not our great tradition enjoin us to accept not its current or dominant but its true version?

Not the lowly demands of rhetoric alone, I suggest, but the authors’ or their authority’s conception of tradition necessitates the acceptance of the now traditional understanding of the tradition. According to this conception, traditions result from a processive differentiation of the compressed symbols of a natively unique experience. Compressed symbols cannot appear as such to those who undergo the originary experience; the Mayflower voyagers could know nothing of the differentiation of compressed symbols. First symbols appear “compressed, by comparison with that which they will become with the passing of time” (p. 24). What compressed symbols become is “unpredictable.” Indeed, such symbols are capable of “an infinite variety of potential developments” (p. 37). Political philosophy now seems to know about such symbols and what happened to them in time. But political philosophy, we discover, is itself a differentiation of symbols, a “critical clarification” of traditional symbols that uses “the symbols as its raw material” (p. 25). So this Voegelinian interpretation of our political tradition requires that the traditional or publicly dominant symbolization of the tradition be received as the object of critical clarification. Since that symbolization is understood as essentially accidental, this interpretation depends upon the “passing of time.” Its truth is temporal and, therefore, not truth in the traditional or philosophic sense. But perhaps Voegelin’s representation of symbolization will be shown to be the compressed symbol of the absolute experience. Until it is, *Basic Symbols* will stand as

an attempt to criticize the times from the point of view of the times.

In spite of or because of this root difficulty, the book has pedagogic value. To accomplish their questionable aim the authors must state the dominant view of the tradition and its characteristic presuppositions. This they do with clarity and force. With equal force, they show this view’s theoretical and historical difficulties. If they have not succeeded in understanding our political experience better than our great political heroes did, they certainly understand it better than the now dominant scholarship does.

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Power and Community: Dissenting Essays in Political Science. Edited by Philip Green and Sanford Levinson. (New York: Pantheon Books, 1970. Pp. ix, 396. \$10.00, \$2.75 paper.)

This collection of essays is an outgrowth of the Caucus for a New Political Science. It more or less accurately reflects that group’s unhappiness with so-called mainstream political science. In a lead-off essay, Euben ascribes many of the problems of the discipline to the trauma of Nazism and McCarthy. One who remembers the way in which populist became a pejorative term can find some merit in the history. Not only did the leaders of the discipline develop a fear of the masses and old fashioned democracy, they began to rationalize a pluralistic group competition with an apathetic citizenry as a reasonable facsimile of all that democracy really need be. Political science had become part of the establishment and unconsciously began to pay its way by rationalizing the status quo in true Panglossian style. Beyond the discipline’s love affair with the Land of the Free, it suffered a further astigmatism by one-sided attachment to the national cross-sectional sample survey, the endless and empty though honorific voting studies, and mindless statistical exercises in correlation analysis.

Were this the whole of Euben’s complaint, many would agree with him. However, he embarks on an application of Kuhn’s *Structure of Scientific Revolutions* to the discipline in apparent innocence of Kuhn’s subsequent abandonment of the position taken in that book and Stephen Toulmin’s powerful criticism of it. Following Kuhn, he comes precious close to making political science a mere procession of momentary fads without independent empirical warrant, corrigibility, or possibility of cumulative improvement. This may be right, but one

must hope, if the discipline is to have value as other than rhetoric, that there is a better possibility.

Levinson, I would like to believe, is disturbed by Euben's extreme posture especially since he seems to be honestly and honorably concerned with whether or not he has something of value to teach his students about useful modes of explanation and reasoned evaluation. His own experience at the feet of Kissinger must render him chary not only of the mandarin but of others whose warrant to teach is even less susceptible of rational criticism. But he is surely right that asking how the discipline might provide its students with really useful tools would be refreshing in its impact.

Dolbeare makes the point, with which more and more are coming to agree, that the discipline has been too wrapped up in institutions and institutional inputs to the almost complete neglect of outputs, particularly as these are observable in the lives of the underlying population. "Who Governs?" needs to be supplemented by the question what difference does it make to whom who governs and the mode of their governing. The burden of many of the essays is that a favorable verdict has been bestowed without inquiry.

Rogin attacks what he claims to be Truman's failure to deal with the political process of groups in the same manner as with states. National interest is treated as a "spook" but group interest is treated in a different fashion. Rogin sees the conflict between leaders and rank and file in labor as an important reason why interest group liberalism fails to yield the democratic fruits claimed for it. One may wonder whether Rogin thinks Lenin wrong that the working class by itself doesn't rise above trade union consciousness or holds Truman at fault for not making Lenin's point.

Lipsitz finds Lane and Converse to consign "the poor," whatever this term may mean, to a satisfied conservatism or a confused inarticulateness. His *n*, several times that of Lane but still scarcely significant, shows that the "poor" have grievances. Presumably, a truly democratic system would achieve their articulation. Moynihan, if he had taken off his technician's blinders, might have had more tolerance of the O.E.O. experiment.

Parenti finds the classical image of ethnic integration misleading. Irish, Poles, Italians, and others have been invited to assume American burdens but not join the country club. The condition of Negroes is far worse. From the most rabidly assimilationist of the ethnics, they have

been forced to the perspectives of black nationalism on the one hand and class struggle on the other. While Parenti sympathizes with the class struggle position of the Panthers, he finds value in the nationalist posture as well. He sees the Negro future as bleak, depending on a social revolution that only whites can make. It would be interesting to examine what data and what numbers make this perspective persuasive.

Wolfe recounts his experience of the attempt to create a community, with the creation of community as the purpose at Old Westbury. Not surprisingly he finds this was the wrong way round. You have to have purposes in common that make a community a useful enterprise.

Walzer raises one of the most engaging ideas in the volume. Taking off from the great sit-down strikes of the automobile industry and implicitly raising questions of university governance, he examines the possibility of the civil disobedience of corporations without utter incivility or a confrontation with the state. This essay has the great merit of being grounded in history and addressing concrete continuing problems. One would like to see it carried further.

Green discusses the revolutionary implications of Ocean Hill-Brownsville but mainly in terms of a polemic with Mayer. He assumes that the possession of a school board creates a black community in the district. Would that that would do it. Those blacks and whites trying to develop leadership and organization in the ghetto would have a ready made nostrum. It would also have helped if Green had been able to tell in what other than an intuitive way he measures the good done the people of the district and the children of Ocean Hill-Brownsville. These need more than taking on faith.

Schaar, speaking in a voice oddly reminiscent of William Y. Elliott attests the decline of authority destroyed by the workings of a rational-legal order and the abundance of the bored middle class. There is a sense in which we all would like the poetry of religion and tradition like the mullioned windows of a Gothic Cathedral, to darkly illuminate what reason will not warrant. James the First attacked those who would wade into the mystery of kings. But as Schaar himself with Weber recognizes we can't go back. Nor is the case except for the bored unemployed academic so hopeless. Human misery and vast scarcities abound. The bored vapid vanity of affluence is the occupational disease of a well paid academia and its middle-class pupils.

Tracy Strong writes an essay "Hold On to Your Brains." It's a hard ride. He cites with approval Nietzsche. "As I understand Nietzsche, he is writing in such a manner that everyone who reads him is forced or drawn to read *himself* into what Nietzsche wrote." This new epistemology is apparently unburdened by the law of the excluded middle and offers limitless possibilities for a new political science.

McWilliams winds up the volume with an essay of which only a very fastidious Platonist would disapprove. On balance, with exceptions, these essays are not so much an attack on mainstream political science as a plea for a return to an older genre of the discipline, belletristic political theorizing in the mode of the *New York Review of Books*. It is difficult to answer the vulgar question who would do what differently and better as a result of reading many but not all of these essays. Somewhere Marx writes, up to now the philosophers have only interpreted reality; the important thing is to change it. These are for the most part philosophers. What and how and if they seriously want to change remains obscure.

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Toward a New Public Administration: The Minnowbrook Perspective. Frank Marini, ed. (Scranton: Chandler Publishing Company, 1971. Pp. 372. No price listed.)

Dwight Waldo, feeling that the times were out of joint, and startled by a young professor's assertion that a special issue of the *Public Administration Review* constituted "old men talking to old men about irrelevancies," took the initiative in organizing a conference of "young Public Administrationists" at Syracuse University's retreat at Minnowbrook in New York in September, 1968. The conferees were "to discuss whatever seemed important to them." The papers and commentary were assembled and printed in book form, *Toward a New Public Administration*, subtitled *The Minnowbrook Perspective*. Frank Marini of Syracuse edited the book.

There are twelve papers, most followed by comments, often short papers themselves. In addition, short portions of the tapes of the meetings are frequently inserted to further illustrate their tenor. All but one of the twenty-two contributors are academics, most, assistant professors (as of 1970); all are "young," but the criterion of youth is not disclosed. Given a conference so organized, readers will probably expect to meet a predictable syndrome of ideas, assertions, criticisms, and emotions. They will

not be disappointed. This syndrome "came to be called the Minnowbrook perspective" (p. xv).

Although the tone of the conference, as revealed in papers, commentary, and edited tapes was unmistakable, not all participants shared the "Perspective" or bought it one-hundred per cent. Many arguments, therefore, were at least easy for me to understand. Since we know that administration, being government, is inevitably political, a more self-conscious recognition of the administrator's values should contribute to responsibility. In a world of rapid change, the administrative objective might well be development—an evolving, adaptive, problem-solving structure rather than classical rationality. Recognizing new needs for change, innovation, and clientele involvement, and recognizing also the inevitable value commitments of administrators, might indeed produce a more realistic public administration theory. We are told that social, technical and cultural changes may require modification of Weberian bureaucracy in the direction of short-term, situationally determined, project organization. An orthodox assessment of the scientific validity of administrative theory is combined in one paper with a criticism of the moral basis of such theory, deemed to be poor "if our assessment is sensitive to the preferences of minority groups or underprivileged participants in a political system" (Kronenberg, p. 215).

A few papers could have been presented comfortably to a Conference of Older Public Administrationists—for example, Henderson's adequate paper on comparative administration and Sharkansky's paper on policy making, which comes the closest to an empirical study in the classical scientific style. And a few people (for example, Howard) even expressed doubts about that prestigious new governing device—PPB—in line with Wildavsky's warning that government is inescapably political, not economic.

The last three papers are by Syracuse faculty members. Frederickson attempts to foresee the future of a public administration with a "social equity" basis. Lambright tells us that "Public Administration is Public-Policy Making"; and Marini, expressing some "Objections to Religious Crusades," attempts to summarize the "Minnowbrook Perspective."

Perhaps more interesting, if not more enlightening, than these digestable proposals is the mixture of notions and emotions which constitute the "Perspective." (Remember, as far as this book is concerned, the "Perspective," alternately the "New Public Ad-

ministration," is what we, the readers, have been expressly offered.)

A dominant theme is the search for "relevance," a term never really defined. Generally accepted is the notion that this is a period of revolutionary ferment and change, but no indices of scope or rate of change are produced. There is even an occasional reference to this "repressive" society. ("Perhaps more attention should be devoted to exploring the options available to administrators in a society bent on repression" [Friedland, p. 54]). In such a conference one is not surprised to find commitment to "the Politics of Love" (White, p. 69), defined as consensus behind absolute ethical norms and a political process of confrontation rather than negotiation and compromise by representatives. Apparently, history is or ought to be a progression from status to contract to confrontation. However, the exaggerated claims for youth ("... the modern young person is undeniably more mature than his counterpart of even a generation ago" (White, p. 61) might indicate a movement from contract (achievement) back to status.

Many of the contributors accept the notion that the American system is coming apart. "It has now become painfully visible to our society that it is the owner of decaying cities, racial inequities, civil strife, . . . inadequacies in the quality and responsiveness of a large array of governmental services, and so on" (Biller, p. 96). Dogmatic terms like "painfully obvious" abound, and one suspects that, as usual, they indicate a paucity of data.

The contributors, for the most part, reject the notion of an objective social science. Knowledge is subjective, value-laden, and culture-bound, a result of social brainwashing supporting the *status quo*. Since it must always project value preferences, they urge that social science, and especially public administration, adopt an explicit value orientation. Roughly, this orientation should be the promotion of equity in income and power—a strong identification with the interests of fairly powerless minorities. To bolster this position, they reject "positivism" and hopefully call upon phenomenology and existentialism. The structure of consciousness itself qualifies the purity of our knowledge. Hence, self-analysis, self-revelation, empathy are all parts of knowledge gathering. One must "expand consciousness." (See especially the paper by Kirkhart and following commentary.)

These views lead to an attack on democracy as we experience it. "... the procedures of representative democracy presently operate in a

way that either fails or only very gradually attempts to reverse systematic discrimination against disadvantaged minorities" (Frederickson, p. 311). This criticism also applies to public administration, which is too majoritarian. It does not reflect "the preferences of minority groups or underprivileged participants in a political system" (Kronenberg, p. 215).

Consequently, "the new Public Administration should be concerned with making the public bureaucracy an instrument for achieving social justice and equality. This concern is very different from that of simply making the government responsive to the wishes of a majority of the people" (Zimring, p. 231). Public administration should be oriented toward the clientele, provided they are underprivileged, not the popular sovereign whose servants we thought administrators to be. There is too *much* distinction between policy and administration. Administrators should be political.

But how can public administration thumb its nose at majorities, or at the actual holders of power? Why should the establishment support such an administration? How do you transfer power from those who have it to those who do not? They have no solutions and barely discuss this problem. The only suggestion is an ominous one. "... it may be necessary for the new Public Administration to develop outside of the existing institutional framework and thinking of the university and government" (Zimring, p. 233).

This group was little concerned with techniques to improve administration. Why increase the effectiveness of a bad institution? For many of them, however, PPB is different. It is related to the concern for social justice. Perhaps this method will work where pluralistic democracy, with its incrementalism, failed. "PPB can be used as a basic device for change . . ." (Frederickson, p. 313). Experts in the science of decision, given a vision of equity for the downtrodden, need little help from democracy. An elite of the good and the competent will be a better form of government. The role of the university in all of this is to produce such an administrative elite. Academic public administration should seek to attract and produce change agents or activists bent on securing greater equality regardless of congressional or presidential mandate.

This book is not concerned with empirical studies or explanations of administration (with the exception of the paper by Sharkansky). It is concerned with conceptual frameworks and might, therefore, have greater value to academicians than practitioners. It is, however, mostly

concerned with philosophy and criticism of our governmental and, to a lesser extent, academic institutions. Consequently, for the most part, if it tells us anything it tells us about the emotional-intellectual state of several young teachers of public administration rather than about administration. Since they are teachers of administration, the book is best regarded as containing *facts in* administration rather than *facts about* administration.

It is customary for a book reviewer to give his personal evaluation of a book. While I can accept much of the analysis, the ideas involved in the "Minnowbrook Perspective" are difficult to evaluate. The positions in this "Perspective" are so fundamental that they are really beyond proof or disproof. Such fundamental commitments are acquired by an act of will. These positions are so contrary to my own as to render attempts at communication all but useless. I am committed to the belief in objective reality; the belief in the possibility of objective truth, hence knowledge; the belief in pluralistic democracy; the belief that administrators should be responsible to Congress, the President, and the Courts. I believe in tolerance, in negotiation and compromise, in contract, in incremental policy making. I do not understand why it has not occurred to the "New Public Administration" group that the governing instrument they wish to create might turn against them and their values and promote a set of values of an entirely different kind.

VICTOR A THOMPSON

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The Political Persuaders: The Techniques of Modern Election Campaigns: By Dan Nimmo. (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1970. Pp. 214. \$2.45.)

"Can the 'scientific politician' do better than his 'natural' opponent?" Robert Lane and David Sears promised we would have the answer to that question by 1974 (see their *Public Opinion*, p. 56).

Dan Nimmo, writing in 1970, and thus with four years, if he wants it, to change or firm up his mind, says the answer is becoming clear. The answer is yes and no, but mainly yes.

The scientific politician, Nimmo says, "does better" with the uninvolved citizen. He knows that knowledgeable citizens, politically interested citizens, and partisan, committed citizens (who are all probably the same people) cannot be converted, and can at best only be reinforced. The scientific politician knows that the citizens worth attending to are those who are less knowledgeable, less interested, and less

committed. Because, as Converse very effectively demonstrated ten years ago ("Information Flow and the Stability of Partisan Attitudes," *POQ*, Winter, 1962), the least political are the most manipulable. They are, in addition, the hardest to get to, because they will not initiate a search for political opinions. To generate political opinions in them, whether they will or no, the scientific politician uses the techniques of the mass marketer—impulse politics in place of impulse buying.

The principal virtue of Nimmo's book is that it is an up-to-date survey of techniques for getting to the least political. The central part of the book—chapters on "Campaign Management," "Profiling the Electorate," and "The Tactics of Mass Persuasion"—count the ways the least political are wooed and won by scientific politicians.

Around those central chapters Nimmo weaves a theory of "attitudinal and perceptual effects." The scientific politician, he says, finds partial and often nonpolitical linkages to voters, then minimally and temporarily politicizes them in the context of the campaign—just long enough and strong enough to secure the vote. The result is a discrete, unselfconscious, and impermanent, symbiosis of candidate and apoliticals in which the candidate gets the winning margin (he hopes) and the apolitical citizen keeps his self-respect (and obtains relief until the next time around). The mass man appears once more as one who, in political things, moves through perceptions to images to behaviors with little if any of the psychosocial integration that characterizes rational man. Thus:

Mass Man: Perceptions → Images → Behaviors
Rational Man: Beliefs ↔ Attitudes ↔ Behaviors

Once more social science seems to have shown the improbability of reasoned politics. Nimmo appears to believe that things are getting worse: campaigns are "increasingly" pseudo events (p. 26); there is an "increasingly growing" group of voters who rely on TV and "usually isolate themselves from all other media" (p. 118).

But *are* things getting worse? Campaigns were always pseudo events, were they not? The case could be made—convincingly, I think—that elections have always been unsubstantive and unthoughtful in direct proportion to the size of the electorate. If the American electorate now is becoming more knowledgeable and politically conscious (all the correlates of education, age, and class tell us that it is), then things may not be getting worse. We don't

know. The trends are not clear yet in the contest between manipulative technology and campaigns in which there is a shared and genuine public discourse. I think we're getting more of both.

An end note. With a minimum of editing, this could have been a trade book. It may not be too late even now. It might sell five thousand copies a year in airport bookstalls.

BERNARD HENNESSY

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The Politics of American Foreign Aid. By Michael Kent O'Leary. (New York: Atherton Press, 1967. Pp. 135. \$6.95.)

Winston Churchill once said that democracy is the worst form of government "except for all the others." Professor O'Leary says that the man on the street evaluates foreign aid in somewhat the same way: ". . . foreign aid is a good policy except for all the forms in which it has been applied" (p. 113). This aphorism epitomizes the ambiguity surrounding foreign aid and which affects every level of U.S. policy making and administration. But Professor O'Leary has achieved an academic tour de force by writing a different book on a well-worn subject. This one concerns the *domestic politics* of foreign aid.

The author notes that experts disagree about where trade ends and aid begins. He compounds this confusion by introducing a new term, "ideological aid." Harking back to Tom Paine, he writes that "in [this] sense, America has been in the business of foreign aid for its entire history" (p. 5). Moreover, "much of the malaise about foreign aid can be attributed to this ambiguity" about its nature and purpose.

The foreign aid dilemma is not just a case of ambiguity however, not just the well-known American desire to be compassionate and hard-headed at the same time. Rather, and this is the central message, our political system has a capacity for producing a continuing stalemate on this question.

The book is devoted to an analysis of the workings of the entire political system, but to the reviewer the most striking evidence is contained in Chapter 3, "The Political Distribution of Opinion." Here the author states that "relevant opinion is distributed throughout the population in such a way that almost equal weight is given to the proponents and opponents of American foreign aid policy . . . neither . . . enjoy[s] significant political advantages" (pp. 29-30).

The geographical distribution of opinion shows a high incidence in all regions of "inter-

nationalism." This finding supports the national average—51 per cent—in favor of foreign aid (AIPO poll, March 1958). However, the South in the same poll registered only 38 per cent in favor. The South favors military aid for its direct opposition to communism; it also favors aid of a low-cost and clear-cut "missionary" character. Nevertheless, Southerners also see aid as a big spending program launched by the distrusted central authority that is forcing integration upon them. This program is carried out in favor of foreign peoples and leaders who happen to be colored. The intensity of negative feelings about the latter clearly override the favorable feelings toward the former.

The real possibilities for stalemate are seen in the fact that those states having the lowest approval of foreign aid (44 per cent in 1958) sent to Congress 57 per cent of Representatives and 52 per cent of Senators. In this case a nationwide minority opposing aid is represented by a majority in each house of Congress.

Favorable opinion within socioeconomic groupings is strongly related, not surprisingly, to high levels of education and income. Favorable opinions exceeding 60 per cent were found only in groups who had at least a partial college education (17 per cent of the population) and within professional, executive, and sales classifications (29 per cent of the work force).

Further possibilities for stalemate also exist in socioeconomic groups: Some anti-aid opinions are held with greater intensity than pro-aid beliefs. Furthermore, the author points out that "a little learning is a dangerous thing for foreign aid" (p. 35). More people in a middle position in education, having partially completed high school, considered foreign aid expendable—in response to a question about which items of government expenditure they would most like to see reduced—than did those with either more, or less, schooling. As indicated, one of the contributions of the study is its emphasis not just on the existence of opposing opinions but on an analysis of the intensity with which opposing opinions are held.

When the results of poll questions on foreign aid are tabulated according to party allegiance in presidential elections, no significant differences are found (page 42). This observation is supported by polls going back to 1948. Reasons: first, on foreign aid, only one respondent in five had simultaneously (1) an opinion, (2) any knowledge of government policy, and (3) an accurate perception of the policy positions of the two parties. Second, higher levels of voter knowledge and interest tend to be associated with "bread and butter" issues. (But the

author notes that the single issue in 1960 on which the greatest number of respondents showed knowledge and perception of party positions was the question of taking a firm stand against China and the Soviet Union.) Third, foreign aid offers no opportunity for candidates to tap a consistent set of opinions.

The attitudes necessary for support of foreign aid are found to include: internationalism, support for long-term involvements abroad, a belief in the "exportability" of the "American way of life," tolerance of "deviant" international behavior by aid recipients, and a certain optimism about government spending (p. 43). These values are diffused throughout American society and are "not concentrated in a politically effective manner." This makes foreign aid a very tricky issue for presidential candidates.

For much the same reason, pressure groups have not been very effective either in marshaling their publics in support of foreign aid or in providing alternatives. Foreign aid is thus decided largely by the interplay between the Executive and Congress. In his treatment of Congress, O'Leary finds that there *are* party differences between Democrats and Republicans. In the 86th and 87th Congresses, in the House the average Democrat voted supporting foreign aid 68 per cent of the time; for Republicans it was 54 per cent. In the Senate, the average figures for Democrats and Republicans were 53 and 48 per cent, respectively. Republicans on the whole have been more critical of foreign aid and more willing to restrict its operations and reduce its size.

Congressional voting is, however, much affected by which party has the Presidency. Senators and Representatives shift their votes on foreign aid significantly between one Congress and the next; both parties exhibit strong patterns of consensus when voting with a President of their own party; out-party votes in both houses exhibit bimodal distributions associated with well known patterns of conflict.

The Presidency is thus the hub of public-opinion formation on foreign aid. It is here that the major policies have been established—the Marshall Plan, Point IV, the Alliance for Progress, the Peace Corps. Yet the tools available to the Presidency in the continuing contest for support are only partially effective. The frequent resort to "reorganization," the shuffling in and out of new administrators, the appointment of citizen and expert study commissions, while all intended to impress the public with presidential concern and hard-headedness, have confused the public and have reduced AID administration to musical chairs. The result is a

weakening of presidential direction with Congress moving into the vacuum. At times, a confusing role reversal occurs with the Presidency emerging as important in the setting of policies, and the Congress becoming primarily embroiled with details of administration. A sort of political Gresham's law seems to be at work here.

O'Leary has an interesting appendix on the "ranking of values" with respect to foreign aid. By comparing the poll results over a fifteen-year period, with respect to two or more questions having one element in common, the author was able to rank the strengths of the positive and negative factors in public opinion relative to foreign aid. The methodology utilized here is inventive and deserves further experimentation; it tends to overcome some of the weaknesses of research based on public opinion polls. Namely, it deals with the continuities in public opinion over a relatively long period and allows for qualitative and comparative statements about the apparent underlying values involved. In spite of this the present reader found the specific results, in this case, rather unconvincing.

Professor O'Leary has done us all a favor in his well-informed, data-packed, and occasionally witty book. His book has "added to knowledge" and while it has ended on the same note of perplexity with which it began, that, after all, is typical of work on foreign aid.

CLARENCE E. THURBER

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The Politics of Fiscal Policy Formation. By Lawrence C. Pierce. (Pacific Palisades: Goodyear Publishing Co., Inc., 1971. Pp. 224. \$8.00.)

Lawrence C. Pierce has written a good book on *The Politics of Fiscal Policy Formation*. Anyone who is interested in how fiscal policy is made in America should begin with this volume. I have a few qualms about it; the first half, in my opinion, is considerably better than the second. Taken as a whole, however, the book represents an admirable achievement from which I learned a good deal.

Pierce seeks to connect the application of economic theory and data to the organization of advice on fiscal policy in a political context. Through extensive interviews with participants, he describes in operational terms how economic forecasts are made, how problems are determined to exist, and how the various presidential and congressional actors go about resolving them. He is particularly good at revealing the relationships between the fiscal policy

instruments—taxing, borrowing, spending, lending, etc.—and the political costs involved in using them to help secure such policy goals as price stability, full employment, and economic growth. Since the executive has certain discretionary powers in the area of debt management, for instance, it may use them, when a change in disposable income through taxes would be preferable, because the more desirable alternatives require cooperation from others who are not always willing to give it.

The admixture of political and economic considerations leads to the adoption of informal norms that may be more important than any declared policy. Social security taxes are often favored for adjustments, for instance, because people do not seem to react strongly to speed-ups or postponements of increased benefits or levels of taxes. Tax increases should not be sought, another norm states, unless the budget adjustment is at least \$2 billion because the hassle is not worth it. Old taxes are apparently better than new ones because people are used to them. So if a tax is scheduled for removal, the first alternative would be to consider continuing it. I would have liked to see more done with the practical impact of these norms under different conditions.

The third and fourth chapters on economic forecasting are the best in the book. Pierce describes how the Troika (The Council of Economic Advisors, the Treasury, and the Bureau of the Budget) goes about making economic forecasts. Each major participant has its own biases. The Treasury, which is always worried about having to raise taxes, tends to underestimate actual revenue. The Bureau of the Budget, which owes its main allegiance to the President but must also get something for the various spending departments, tends to underestimate expenditures. At once, this permits the BOB to give the President what he wants, which is the appearance of less of a deficit than supposed, and to enable a few more expenditures to appear in the budget than would otherwise have been the case. The Council, composed of academic economists, shares their preference for greater concern over employment rather than inflation and towards greater rather than lesser economic growth.

Members of all three organizations must get together, agree on a central economic story, resolve on a step-by-step basis the numerous uncertain aspects of each forecast, and arrive at a series of recommendations to the President with which all of them can live. Disagreement may be found in qualifiers to various sentences or in the shadings of words. When greater un-

certainty than usual is felt to be present, or when the President reaches out more widely for conflicting opinions, advice may be sought from academic economists around the country.

Throughout the book, Pierce provides a description and running commentary on President Johnson's tax surcharge of 1968. By December 1965, it had become apparent to the Council of Economic Advisors that Vietnam war expenditures, in the context of an economy operating at near full employment, would increase inflationary pressures unless there was a significant increase in taxes. President Johnson did not want to cut down on his domestic programs, and he found support in Congress for an increase virtually nonexistent. At one point he asked members of the Council whether he should, nevertheless, still ask for a big tax increase. Pierce reports the following exchange:

'Should I be on the right side for the good of future policy even if I would lose the fight?' After much deliberation, we told him that it would probably be better not to ask for it, that from the psychological standpoint, the costs of a loss of faith in the 'new economics' would be less than the cost of a loss of confidence in the financial markets at home and abroad arising out of the president's inability to get such a request enacted (p. 122).

Pierce does not ask what business "expert" economists have in making recommendations to presidents "from the psychological standpoint," but it would have been a good question.

The second half of the book is devoted to chapters on the Presidency and Congress, ending with a series of propositions purporting to describe fiscal policy formation. Somehow, there is a loss of direction. Little new is added, and much old is recast in unsatisfactory form. What is one to make of propositions like "The greater the degree of technical complexity, the more difficult it is to predict policy outcomes" (p. 183), or "The greater the expected impact, the larger the number of issues and goals likely to be evoked" (p. 187)? Similarly, in the chapter on the Presidency, Pierce's taxonomy of role concepts in decision making (general leadership roles, special staff roles, critical problem roles, etc.) is not at all illuminating. By the end of the book the author is concerned with all sorts of questions—whether policy making will be more or less comprehensive or incremental, whether a smaller or greater number of alternatives will be considered, whether the decisions will be made more slowly or rapidly—that neither he nor anyone else can answer. He would have been better off following through on his earlier efforts to show how the standard operating procedures, which evolve from the interaction of economic

analysis, political requirements and organizational needs, affect a variety of fiscal policy decisions.

Nevertheless, I recommend the book highly to students of national politics and public policy. It is the best we have on the political organization of fiscal policy.

AARON WILDAVSKY

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Aussenpolitik in Adenauer's Kanzlerdemokratie.

By Arnulf Baring. (Munich and Vienna: R. Oldenbourg Verlag, 1969. Pp. 492. Brosch. DM 28, Leinen DM 38.)

Although Baring's work is subtitled "Bonn's Contribution to the European Defense Community," neither the title nor subtitle reveals that the book is actually an exhaustive case study of the beginning of Adenauer's efforts to rearm West Germany and to integrate her into the Western camp. Baring records the relevant history of the period 1949-1953 using an explicitly German-centered, decision-reporting format. Within the limits he has set for himself, he has done a thorough, scholarly piece of work that deepens our understanding of Adenauer's decision-making process, the nature of West German federal-state relations in the early 1950s, and, most persuasively of all, the extremely narrow and illiberal conception of democracy held by Adenauer and most of his chief assistants.

While he has written a good case study, I would criticize Baring for not having written a book to go with the title. For example, there are very few generalizations about foreign policy that transcend the first four years of Adenauer's rule. Baring offers no proof that decision-making characteristics he discerned at an early stage in Adenauer's rule were continued past the first four years. Complicating these omissions is the fact that the thesis of the book is first stated in an appendix. Before one reaches that appendix the reader must guess at exactly what it is that Baring is trying to prove.

His thesis is, first, that the particular contribution of West Germany as an actor in post-World War II European politics has not been duly recognized by Western writers (he singles out as an example Hanrieder's *West German Foreign Policy, 1949-1963*), who overemphasize West German passivity and acquiescence. Second, he argues that Adenauer's success in achieving rearmament (despite majority opposition) and integration with the West (despite warnings that this would result in permanently dividing Germany) occurred because there was no effective domestic political power opposing Adenauer.

In trying to prove his twofold thesis, Baring reaches a number of interesting conclusions about German political actors of that period. Theodor Heuss, for example, who has been depicted frequently as a forthright democrat setting examples of proper behavior for more authoritarian Germans, appears in Baring's study as an acquiescent follower of the Steel Chancellor. The present President of the Federal Republic, Gustav Heinemann, on the other hand, receives favorable notice as one of the very few principled opponents of Adenauer who resigned a ministerial post because of opposition to governing policies.

The focus of the book is on Adenauer as policy maker: his conceptions, strategy, and tactics which dominated the West German political scene. Baring uses a set of themes familiar to readers of Adenauer's biographies in explaining Adenauer's very early decision to anchor West Germany in the West. Baring's explanation is more persuasive to this reader than those offered by Hiscocks and Wighton, for example, because of his more scholarly documentation and the use of deathbed confessions that occurred after the Hiscocks and Wighton books had been written. Using these more persuasive data carefully, Baring links factors such as the Rhineland's ties with the West and Adenauer's Catholicism, his anti-Prussian and his anti-Communist beliefs in a way that convincingly supports even the sweeping statement that "National Socialism, the war, and defeat could not change the foreign policy conception of Adenauer" (p. 56). Baring's study furthermore reveals that Adenauer's early proposals for Western European unification (Fall, 1945) were designed to make impossible a future war between Germany and the West by fusing the economic power of what was to become the ECSC several years later.

Baring claims that there was a "vacuum of domestic political power" (p. 339) when Adenauer's foreign policy course was revealed. There were those who sought to change the Western integration policy but they had no political power with which to challenge Adenauer. Opponents also are shown to have forgotten Adenauer's dictum that "policy must be simple" and thus to have failed to produce popularly acceptable alternatives to Adenauer's policy. Adenauer monopolized the foreign policy-making process by his recruitment of experienced men like Globke and Lenz who led a "political General Staff" which in turn created a foreign policy bureaucracy insulated from the government, the parliamentary majority, the opposition, and public opinion.

This reviewer was particularly struck by the

importance of authoritarian expectations in Adenauer's method of governance. Adenauer and the majority of his top-level assistants appear to have been truly ignorant of the nature of an open, liberal democracy. Adenauer's near total control over subordinates in his own Chancellor's Office was apparently the way he thought publishers and radio station heads, for example, controlled their subordinates. Adenauer acknowledged no spontaneous, independent criticism: all was opposition-inspired, politically motivated. Hence in the field of foreign affairs, anyone in West Germany who advocated German neutralization and demilitarization in the early 1950s was regarded by Adenauer as "either an idiot of the highest order or a traitor" (p. 107). Adenauer and his chief supporters in the government publicly questioned the motives and integrity of the judges on the federal Constitutional Court when that court acted within its competence but in a way that threatened to defeat Adenauer's treaty proposal. Similar charges were hurled at the Bundesrat when it sought to review the treaties involved in the European Defense Community negotiations.

Baring supplies additional evidence to that given in earlier sources on Adenauer's actual reasons for maintaining the position that West Germany and the West should only negotiate with the Soviets "from a position of strength." He offers considerable evidence that Adenauer had actually abandoned the eastern part of Germany to Soviet hegemony on grounds of *Realpolitik* as well as from the fear that the West would be made uneasy by any serious attempts at reunification.

Baring's study, which includes a useful annotated bibliography, is, in conclusion, a well-documented case study of a most important and formative set of negotiations in the beginnings of Adenauer's chancellorship. I hope that Baring will return at some future time to write the more general work this book's title suggests.

PAUL J. CASSIDY

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Attitudes et comportements politiques à Boulogne-Billancourt: enquête par panel 1965-1967. By Frédéric Bon and Guy Michelat. (Paris: A. Colin, 1971. Fondation Nationale des Sciences Politiques: Travaux et recherches de science politique, No. 12. Pp. 129. No price indicated.)

Boulogne-Billancourt, a residential and industrial suburb of about 110,000 inhabitants on the southwest corner of Paris, features the immense government-owned Renault automobile factory along with luxury apartments on the

edge of the Bois de Boulogne. The Fondation Nationale des Sciences Politiques has devoted considerable study to this microcosm of political and social attitudes (cf. E. Findberg, *Évolution politique et sociale de Boulogne-Billancourt depuis 1945*, Paris: F.N.S.P., 1968).

The present work is the result of an extensive questionnaire (10 pages of close type) given to 593 respondents in 1967 after the elections to the National Assembly. All the subjects had also been similarly questioned in 1965 on the occasion of the presidential election (de Gaulle won, remember?). On the basis of these two sets of responses, Messrs. Bon and Michelat draw the most extensive and tenuous conclusions regarding consistency and change in voting opinions and patterns with reference to: sex (not very significant), age (younger voters change more often and prefer presidential system over parliamentary), education (pattern more consistent among people who went beyond primary schooling), occupation (voting stability among upper cadres), income (change is more frequent if real income is either rising or falling), television in home (no significant effect on voting patterns—N.B., French TV is state-owned and -controlled), interest of respondent (greater consistency of voting patterns among those with greater political interest, as measured by their reading of political news items, listening to political broadcasts, etc.), and so forth, with some very nice charts and tables.

A major portion of the analysis is devoted to the question of the Gaullist presidential system versus the parliamentary type: More men than women favor the latter (49 per cent of men respondents as against 38 per cent of women), except that this variation disappears when the political-interest factor is held "high" for both men and women, in which case 57 and 56 per cent, respectively, favor the parliamentary form. Support of the Gaullist system varies with income level and with education (i.e., lower classes like strong rule). Among respondents aged 50-64, 61 per cent prefer the presidential system, while those aged between 21-49 are almost evenly divided between the two systems in their preferential opinions.

In a final chapter the authors attempt to assess the problem of attitude stability, in general, among voters. Conclusion: The greater the integration of the voter into the political system, either because he is a member of a political party or because he is part of the educated elite, the greater will be the consistency of his political attitudes and behavior. Conversely, the more disaffected and ignorant the voter is, the more variable and apathetic will be his voting

behavior. (The argument is really more sophisticated than this, but I am not sure it is more significant.)

For those readers interested in knowing the breakdown of both the 1965 and 1967 samples, the figures are given by sex, age, level of education, income (five categories), occupation of respondent (eight categories), and occupation of head of household. The structure of the two samples varied at most by 5 per cent (in sex differential and in income levels), with a mean variation of only 1 per cent. How the two samples compare with the occupational structure of Boulogne-Billancourt is not indicated, but one notes that 25 per cent of the respondents belonged to the professional and middle-to-upper cadres, whereas only 12 per cent of the active population of France as a whole belonged to this category in the 1962 census; conversely, 35 per cent of the respondents were in the employee-worker-service category, whereas 55 per cent of the active population of France, and 75 per cent of the active population of the Parisian suburbs, were in this category.

To draw from this study any conclusions other than those concerning voter change and stability, mentioned above, would therefore be unwise. But, although not earth-shaking, those data will be of interest to all students of voting behavior and opinion.

WESLEY D. CAMP

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Lenin, Gosudarstvo, Politika. By F. M. Burlatskii. (Moscow: Izdatel'stvo "Nauka," 1970. Pp. 498. 2 rubles, 40 kopecks.)

It is extremely difficult for a Westerner to review any Soviet publication. One can never be sure of the author's views: while a writer's remarks sometimes are his own, often they simply reflect the current party line. As the political atmosphere in the USSR changes, scholars (and other writers) are allowed more or less latitude to express their own convictions. Moreover, Western analysts often are too strongly inclined to look for hidden meanings or subtle protests against the regime or its leaders. Even those who are unenthusiastic about Kremlinology sometimes see Aesopian significance in perfectly innocuous statements.

The present book shows how relevant these problems are. Professor Burlatskii, trained in philosophy, is best known to American political scientists as a vigorous and articulate advocate of the scientific study of politics. In a major *Pravda* article in 1965, Burlatskii advanced the radical notion that political analysis in the USSR involved little more than rhetoric and

doctrinal exegesis. A genuinely scientific approach, he argued, involves "the asking of questions whose answers are not known in advance." Unfortunately, events have shown that the Brezhnev-Kosygin regime is extremely hostile toward freedom of scientific inquiry, and the promise of Burlatskii's article has not been fulfilled. Advocates of political science in the USSR are stifled by powerful conservative forces—academic, ideological, and political.

Burlatskii's book shows only too clearly the terrible constraints under which Soviet social scientists labor. The most interesting and innovative section of the book, "Society and Its Political Structure," is essentially a theoretical and methodological essay. It sets forth the rationale underlying a systems approach to the study of politics and argues for empirical research efforts combining structural and functional analysis. Burlatskii's presentation is almost entirely straightforward and undogmatic. He is clearly critical of the ossified state of Soviet social science. To bring it up to date, Burlatskii recommends the "critical assimilation" of "bourgeois" research techniques, especially modeling and statistical methods (p. 186). He explicitly urges Soviet sociologists to expand their research into political matters and criticizes official agencies for their failure to make available certain of their data.

Burlatskii emphasizes the instrumental role political science could play in the USSR. To bolster his argument, he cites the experience of East European social scientists in developing an independent discipline of political science, e.g., in Poland and Yugoslavia. Moreover, he sets forth some rather unorthodox proposals. He supports the practice, adopted by a number of East European states, of allowing contested elections for legislative seats. He even repeats the extraordinary suggestion for court reform he first put forth in 1958, to increase the number of people's assessors from two to ten and thereby introduce a form of trial by jury.

The bulk of the book, however, is tendentious and polemical, hardly conforming to the innovative suggestions sketched out in the methodological section. Burlatskii's views on most subjects are predictable and dogmatic. His criticism of "bourgeois" (primarily American) political science is sweeping, sometimes to the point of being doctrinaire. According to Burlatskii, political science fulfills very explicit functions in bourgeois society. It rationalizes the existing system of property and power relations and contributes to the political socialization of Western youth. This conception of political science is important and merits careful analysis;

indeed, some Westerners hold the same view. But Burlatskii makes no attempt to prove his point; he advances certain conclusions without providing any evidence whatsoever. Thus, he says, American political science developed "under the influence . . . of state monopoly capitalism" as a response to Marxism and to help the "enormous state machine" function more efficiently. Even now there is a "direct link" between the discipline and state-monopoly capitalism. And political scientists are responsible for the "ideological and political training of students"—all of whom, he says, must attend a series of lectures on political science. The purpose is to train citizens "in a spirit of defending bourgeois ideology and combatting communism and socialism" (p. 185).

But if Burlatskii is no radical, he certainly is no hack. His writing is far superior to most Soviet writing on politics. His defense of the Marxist approach to the study of politics and his criticism of Western behavioralism (especially the group study of politics) are intelligent and articulate. But the reader is left frustrated. Burlatskii is unquestionably an able and informed scholar. His enthusiasm for methodological rigor, for empirical research, for a comparative approach, is clear. He is manifestly impatient with the need to conform to an official line; on the basis of his earlier writings, it is clear that he is a progressive thinker. Unfortunately, this book does not meet his own high standards. It is a casualty of political change in the USSR, of a regime which is increasingly unenthusiastic about academic freedom. Perhaps if he had published it earlier (it was ten years in the writing), Burlatskii would have produced a very different manuscript. But Soviet censors today are even more vigilant in preventing the publication of anything resembling value-free social science. Though not without considerable merit, the book ultimately is disappointing.

DAVID E. POWELL

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Memoiren 1918–1934. By Heinrich Brüning. (Stuttgart: Deutsche Verlagsanstalt, 1970. Pp. 721. Price not listed.)

While Heinrich Brüning's memoirs cover the years 1918 to 1934, three-fourths of the book is devoted to his work as *Reich* Chancellor, 1930 to 1932, and as Foreign Minister, 1931 to 1932. Some seventy pages deal with the two years preceding his flight to Switzerland in 1934. His years as a youth are not mentioned. A few pages are given over to his experiences as a front line soldier.

The memoirs were written in several parts, in different years, beginning 1934 to 1935 in Switzerland and ending in the 'fifties when he wrote in the United States about the period from 1919 to 1929. His book is based on a wartime diary, newspaper articles published by him and some friends in 1923, and, for the crucial years of the Chancellorship, on the detailed daily appointment calendar notes (*Tageszettel*) kept by State Secretary H. Pünder, Chief of the Chancellery, and supplemented by Brüning's personal stenographic notes. The *Tageszettel* and a number of important documents were smuggled out of Germany in 1934.

The memoirs provide only a few attempts at an overview of the historical events of the two-year period shortly before Hitler's rise to power. They cover in some detail a large number of grave economic and political problems, domestic and foreign. Among them were reparations, military and naval questions, the *Osthilfe*, and the Austrian customs union; financial crises in industry, agriculture, banks, newspapers, and religious and other organizations; widespread corruption and fraud in public and private life (pp. 406–409, 449–50); the waste of public funds, unemployment, elections, and relations between the *Reich* and the *Länder*. The fateful emergency decrees are described and discussed. Brüning had in readiness drafts of additional emergency decrees which he was secretly prepared to use for the sake of "*Ruhe und Ordnung*" in the event that coalition negotiations in the *Länder* ended in failure. These drafts provided for the *Reich* to take over from the *Länder* control of the police and administration of justice (p. 570).

Unfortunately, Brüning offers no philosophical discussion of the use of emergency decrees, no coherent examination of the role of Hindenburg or of Brüning's attitude toward him, and no questioning of whether the system of proportional representation contributed heavily to the German political party mess. He does, however, provide a number of acute observations on German political life of the time.

Brüning had some trouble keeping his Foreign Office in line (pp. 295–97). His official telephones were bugged by *Reichswehr* officers and secret information was passed on to both General Schleicher and the Nazis. Secret Chancellery documents were made available to Berlin newspapers (pp. 395–98).

Brüning was a man of strong convictions, and of an independent mind. He was a convinced monarchist who favored and planned for the restoration of the monarchy (pp. 373, 378, 418, 453, 456, 462–63, 579), but he of-

ten praises Social Democratic leaders for their courage and sense of civic responsibility (pp. 110, 132, 214, 218, 260). Although he was a strong conservative, he frequently castigates the parties of the right, the *Reichswehr*, big industrialists, bankers, and land owners. A devoted Centrist, he had bad personal and political relations with Prelate Ludwig Kaas, the national leader of his party. He writes bitterly about the willingness of many Centrists to play ball with the Nazis, some, he claims, to save their cabinet jobs in the *Länder*. He even charges prominent pro-Nazi Centrists with "treason" against the party (pp. 668-69). On an official visit to Rome, he tells us that he felt close to Mussolini and had sharp differences with Cardinal Pacelli, Papal Secretary of State. He told Pacelli that he "would have to resist any attempt on the part of the Vatican to exercise influence on his political decisions or the attitude of the Center Party," but he felt free to advise the Pope that "National Socialism and Fascism had no spiritual bases in common" and that he, therefore, "considered it necessary that the Vatican come to a conciliation with Mussolini at a time when Bolshevik ideas in red or another color were spreading throughout the world" (pp. 354-60).

Brüning was bitterly disappointed in the weak and changeable attitude of most German bishops and many higher clergy toward the Nazis (pp. 653, 664-65, 673). He expresses horror at the willingness of the Vatican to conclude an unfavorable concordat with the Third Reich and at its unwillingness to form a common front with Protestant believers in defense of basic Christian teachings (pp. 666, 670-73, 677).

The memoirs abound in detail. Curiously, there are many references to meetings taking place on "Saturday" or "Wednesday at 9:30 A.M." but few indications of the day of the month, or year. There is an overabundance of last names, often without indication of positions held or other background information. A valuable but incomplete Index of Persons helps to identify many people of importance but falls short of clarifying Brüning's references to "a high official," "his uncle," or "the mother-in-law" of an ambassador.

For the student of German government and politics there is plenty of "pay dirt" in Chancellor Brüning's memoirs, but the individual veins vary greatly in worth. The prospective publication of a number of Brüning's university lectures and letters may be expected to add to the

value of these memoirs by clarifying a number of questions.

JOHN BROWN MASON

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The British General Election of 1970. By David Butler and Michael Pinto-Duschinsky. (London and New York: Macmillan, St. Martin's Press, 1971. Pp. xvi, 493. \$15.00.)

This treatise, which is the eighth in the series of Nuffield College election studies, follows the main contours of the three preceding volumes, but with a few beneficial variations. In place of the sketches of the campaign in selected constituencies, the authors offer a more general and better-ordered treatment of electoral politics in the localities. Interviews over a wider range have yielded more information about developments in the interim between elections and about strategies in the national campaign, as well as political activities at the local level. Included for the first time are valuable census data arranged on a constituency basis.

The authors set for themselves three "central tasks": (1) to see why the fortunes of the Labour Party dwindled so rapidly between 1966-1969; (2) to show how it staged a comeback in 1969-1970; and (3) to "examine the way in which the Conservatives won after all" (p. ix). These questions, though oriented more toward description than theoretical analysis, are nevertheless clear-cut. The study, however, is not explicitly organized around the questions, and at times the reader has to pull together factors that are buried at various places in the narrative. Collecting a vast amount of useful factual material, the authors intend their study to be primarily descriptive: "We hope we have made a substantial contribution to the argument and provided evidence which proponents of any point of view can cite" (p. xiv).

After wrapping up the voting charts which recorded their 1966 victory, the members of the Labour Government turned to the economic graphs which indicated nothing but trouble. To counteract inflationary pressures and chronic runs on the pound, they instituted wage and price controls. Eventually they had to resort to devaluation, but were slow to take advantage of it. As unemployment spread, deflationary measures grew more unpopular. Strikes in 1968 prompted the Government to introduce an Industrial Relations Bill which generated so much hostility within the Party that the minister was forced to withdraw it. Dissatisfaction with the Government's performance in

1966–1969 was reflected in the opinion polls (its support dropped to 26 per cent at one point), heavy losses in by-elections and local elections, and a decline in Party membership. The wounds inflicted by Labour's economic policies, the authors conclude, were not sufficiently healed in time for a June election.

In the autumn of 1969, however, Labour's position began to improve. The balance of payments turned in Britain's favor, and the Government scored points in its handling of the violence in Northern Ireland. Encouraged by these developments, Labour strategists launched a campaign to rally the Party activists, and the Government relaxed some economic controls. After spotty improvement in by-elections and county elections, Labour registered dramatic gains in contests for borough council seats in May 1970, an increase in popular support that was confirmed by the polls. At this point the Prime Minister opted for a June election rather than waiting for October. As the campaign advanced, the odds were strongly in Labour's favor.

In light of these developments, why did the Conservatives win? The authors find part of the answer by comparing the two party organizations. After 1966, the Labour organization suffered from a further loss of agents, a rift between Transport House and Downing Street, and low morale among the activists owing to defeats in local elections and hostility encountered on the doorsteps. Labour's late recovery and an early election did not allow sufficient time for the Party managers to get their machines in good working order.

In the period between the elections, the Tories also faced difficult problems: internal disagreements, a narrowed range of maneuver in attacking Labour policies, and a leader who did not inspire confidence inside or outside the Party. But soon after the 1966 defeat, Conservative leaders set about to build up and streamline their organization. Increased financial resources enabled them to upgrade the agency service and to undertake research programs as a basis for policy discussions. They also tried to recruit members and candidates from a wider social base, and they designated certain marginal seats for special effort. While most of these projects fell short of the targets, the Tory organization at least did not deteriorate, and the Party's managers were better prepared for an early election than were their competitors.

In the authors' view, the outcome—for the first time in many years—was probably decided

by the campaign, even though it did not generate much enthusiasm. Issues were not conspicuous, and media coverage focused upon Wilson and Heath. Recognizing increased volatility of public opinion in recent years, the authors suggest the possibility of a swing to the Tories in the campaign's closing days. But what caused it is still a mystery (pp. 186–187). They see the volatility as a long-term trend, with voters being swayed by income boosts timed so that an election can be held before prices catch up.

For the first time in a general election, eighteen-year-olds voted, and candidates were permitted to list their party identification on the ballots. The voter turnout, however, was the lowest since 1935, partly as a result of holidays. The authors found no relationship between diminished turnout and swings to the Tories—an analysis confirmed by survey research. Abstention rates were especially high among Liberal voters. The evidence suggests a higher turnout among colored immigrants who may have been activated by "Powellism," and the Conservatives derived little gain from local feeling against immigrants. According to the analysis, the nationalist parties in Scotland and Wales drew support from people who are ordinarily nonvoters.

The swing—the biggest in the postwar era—was more uniform than usual. In analyzing the swing patterns, the authors detect mostly cumulative trends rather than pendulum movements, with Labour exhibiting strength in Wales and Scotland, the large cities, and parts of the North, and the Conservatives drawing their support from rural and suburban areas, parts of the Midlands, and the South.

Timeliness dictated that the study be published as soon as possible, and, despite this pressure, the authors have done a fine job of reporting events and compiling statistical information. Understandably, there are occasional slip-ups, as when they report that the April local elections both "offered little comfort to Labour" and showed a "marked improvement for Labour" (pp. 131, 172). Scholars who are looking for a chronicle of developments before and events during the campaign (including an interesting treatment of Scottish nationalism) will find this a valuable book written in an engaging style. Researchers, too, will find the statistical data a very useful source; especially important are the census data on the constituencies.

Scholars with a thirst for hypothesis testing and theory building will want different ques-

tions to be posed within a conceptual framework and will urge a refinement of the rich data. The variables employed are usually constituency and party, and the aggregate data provide only crude measures of individual voting behavior. In the absence of survey research, for example, we cannot be sure that it was the Liberal vote that switched to the nationalists. By the same token, the "swing" is a measure of net voting change, but it can only suggest clues as to which voters shifted position to produce that change. Appendix III goes a step further by introducing a socioeconomic dimension, and it lays down the challenge for future researchers to combine the new census materials with available survey research data.

Specialists in British politics and research workers in the expanding vineyard of comparative electoral behavior will want to have this volume on their shelves as a reference and source book. Indeed, this reviewer purchased it and enjoyed reading it even before he was awarded a free copy for writing this critique.

JOHN E. TURNER

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The Elites of Barotseland 1878-1969: A Political History of Zambia's Western Province.

By Gerald L. Caplan. (Berkeley and Los Angeles: University of California Press, 1970. Pp. 270. \$8.50.)

The key figure in the first—and perhaps the best—part of this study is Lewanika, the King of Barotseland. Lewanika's cooperation with the whites led to his kingdom acquiring a privileged status in Northern Rhodesia, of which account had to be taken in the negotiations which preceded Zambian independence in 1964. Dr. Caplan argues persuasively that Lewanika followed two overriding policies. The first was to consolidate his own position and to do this he turned for support to the British South Africa Company. Second, he sought with the help of the whites to secure the safety and modernization of his kingdom. His success in both directions, however, was equivocal.

Yeta, who succeeded Lewanika in 1916, belonged to the new, educated elite which had emerged in Barotseland and led its members in an alliance with the traditional ruling class against white rule. In acting in this way rather than turning to, say, Native Welfare Associations "based on a wider nationalism," Dr. Caplan argues that "Lozi nationalism was . . . essentially reactionary, an attempt to restore the lost privileges of the past" (p. 122). But this is to argue with the benefit of hindsight. In 1916 it was hardly "reactionary" to seek to preserve

the Lozi nation against further white encroachment or to fail to see that it was in Barotseland's best interest to become one day a province of an independent Zambia. Obviously, it is important to define the term "nationalism." According to Ginsberg, the "nation" is "a group whose members are held together by a sense of community, induced and sustained by objective factors which, however, vary from case to case;" these objective factors may include language, religion, race, and common traditions and customs. On this definition, it is clear that Barotseland qualified as a nation in the earlier part of this century no less well than the artificial unit of Northern Rhodesia, which was still in the process of creation.

The story unfolds chronologically. In the second half of Yeta's reign, after the Crown had succeeded the Company in 1924, the Lozi ruling class realized that it would not recover its former powers and authority. It decided instead to be satisfied with material manifestations of its status, with conspicuous consumption compensating for the absence of real power. And so Barotseland became a "living museum," a political and economic backwater, cut off from the current of African politics in the rest of Northern Rhodesia. This remained the situation under Yeta's successor, Mwana-wina (1948-68), the overriding purpose of whose reign was to preserve those few privileges which remained to the traditional ruling class. In these circumstances, the torchbearers of the progressive interests of Barotseland became in the 1930s a handful of young men on the Copperbelt and along the rail line, and subsequently the Barotse National Association and the Lozi nationalists within UNIP.

The author shows, however, that the determination to preserve the status quo and to maintain the special position of Barotseland within Northern Rhodesia was not the preoccupation of the traditional elite alone. For even UNIP in Barotseland comprised men "who intended to be both Lozi patriots—tribalists—and nationalists" (pp. 203-4). But so, too (and this significant fact the author does not mention) did UNIP's Lozi national leaders. The latter, according to Mulford, threatened in January 1964 "to withdraw from the party altogether, unless they were granted what they regarded as adequate representation in the new Cabinet." (*Zambia: The Politics of Independence*, pp. 329-30.) Mulford's example underlines again the difficulty which arises from Dr. Caplan's failure to define one of his key terms—in this case "tribalism." He equates parochialism with tribalism (p. 134), thus ignor-

ing the fact that a people may react in a parochial way without necessarily being tribalist. Following James S. Coleman and Carl G. Rosberg, Jr., (*Political Parties and National Integration in Tropical Africa* [Berkeley and Los Angeles: University of California Press, 1964], p. 687), we may take tribalism to entail "the persistence—indeed, the paramountcy—of 'primordial' attachments or ties" (p. 687). Are we therefore to conclude that in the period immediately before and after independence the Lozi were stirred solely by an emotional attachment to the past? The author himself shows that this was not the case in pointing, rightly, to the strong sense of economic deprivation among the Barotse people.

Political behavior in an emergent or new African state must be seen in multi-variable terms rather than in terms of a single variable—here, the factor of tradition. It is for this reason that recent writers on Zambia, such as Rotberg and Molteno, prefer the term "sectionalism" to "tribalism." They argue that sectionalism is a political phenomenon, arising from the competition for power among leaders who make use of linguistic, regional, and other social distinctions to create sectionalist identifications as the basis for recruiting political support among people anxious to secure access to economic resources. If this interpretation is accepted, we can conclude that in the modern period, the Lozi, like others of Zambia's sectional groups, were competing for the economic resources made available through the sale of copper, and that this competition was often played out within the idiom of tribalism.

Dr. Caplan has produced a useful and readable book, for which he has drawn on Lozi oral tradition as well as European documentary sources. But his failure to define his terms, notably nationalism and tribalism, leads him sometimes to draw rather doubtful conclusions from the facts which he marshals with considerable skill.

WILLIAM TORDOFF

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Technology and the Economic Development of the Tropical African Frontier. By Thomas R. DeGregori. (Cleveland and London: The Press of Case Western Reserve University, 1969. Pp. 539. \$15.00.)

Technology, Tradition, and the State in Africa. By Jack Goody. (London, Ibadan, Accra: Oxford University Press on behalf of the International African Institute, 1971. Pp. 88. £1.75.)

These two books together take a historical perspective on a relatively neglected field—the social functions and political implications of technology in Africa. Jack Goody's book is a short but important work that relates political anthropology to technological change in Africa; Thomas DeGregori's book is a much longer exercise in political economy, again in the context of the history of technology.

Both books take us through early phases of political and economic organization in Africa's history, the growth of statehood, a growing awareness of the potentialities of certain resources, and the consequences of discovering new means of exploiting those resources. DeGregori is particularly concerned with development and the sophistication of the means of production. The distinctiveness of Goody's approach is that he sees technology in Africa's history not only in terms of its effect on the means of production but also in terms of its implications for the means of *destruction*. Even the emergence of the metal spear was a significant variable in the process of consolidating monarchical systems of government in Africa. The use of metal was a leap towards statehood to some extent, at least as compared with a combat culture based on the bow and arrow. In the words of Jack Goody:

"The bow and arrow is essentially a democratic weapon; every man knows how to construct one; the materials are readily available, the techniques uncomplicated, the missiles easy to replace. . . . With the technologies of the bow and stone-tipped arrow, any kind of centralization is almost impossible. But with the introduction of metals, kingdoms are on the cards" (pp. 43–6).

According to Goody, the uneven distribution of raw materials involved systems of exchange and opened up long-distance trade, which in turn necessitated systems of control and security. Furthermore, the manufacture of metal weapons became complicated and specialized, and those equipped with the necessary skills acquired a new level of access to power. Thus, we are told, in some parts of West Africa, special kin-guilds of blacksmiths transmit their traditions among their members; and in centralized groups like the Mossi "the guilds often have a special relationship with royalty, who are often their major patrons" (p. 46).

With the subsequent coming of the rifle in colonial Africa and finally the tank in independent Africa, military skills became more specialized and hence more confined to elites. The old days of military democracy—when everyone passed through the warrior stage and the simple weapons could be made by the warrior

himself—were now replaced by the era of professional military specialists, with weapons requiring high technological skill to manufacture and some specialized training to use. The effect of this new military technology on domestic power relations in Africa has been enormous. The history of the military coups on the continent in the last seven years is in part a history of the impact of military technology on the power structure of the new African states.

DeGregori's book, on the whole less original than Goody's, is packed with much more information and bibliographical guidance. DeGregori's book is perhaps longer than it should have been, but it has definite advantages in the immense accumulation of both data and suggestive insights concerning this particular aspect of the political and economic consequences of technological change in Africa. DeGregori is especially good in discussing the relation of *values* to technological response. He takes us through the defiance of the Négritude school of African political thought—the sort of defiance which made Aimé Césaire cry out:

Hurray for those who never invented anything
for those who never explored anything
for those who never conquered anything
hurray for joy
hurray for love
hurray for the pain of incarnate tears!

.....
My Négritude is no tower and no cathedral
It dives into the red flesh of the soil.

(This translation from the French is borrowed from Gerald Moore, *Seven African Writers*, 1962, p. viii.)

There is in Négritude an element of romantic primitivism. DeGregori does not hesitate to preach to Africans. And why should he hesitate? He tells us that:

Africans need not consider borrowing technology a mark of inferiority. All cultures throughout their development borrow from other cultures unless they are isolated. It is precisely this isolation, or the lack of opportunity to borrow from other cultures, that is the major cause of technological backwardness, not race or personality. African cultures have shown themselves to be adaptive in borrowing technology from others (pp. 65–6).

DeGregori is aware, however, that technology is not a value-free phenomenon, but carries within itself a set of normative presuppositions which the author calls "technological values." He refers, for example, to the simple value of punctuality.

Less explicitly DeGregori talks to us about the effect of urbanization in Africa upon the speed of technological acculturation. In En-

gland in the eighteenth century, the migration from the countryside to the cities turned out to be irreversible. The enclosure movement of the previous four decades at that time helped to sever the link which many people might have retained with their ancestral countryside. But in Africa the persistence of migration of nomadic laborers—men who were peasants at one time of the year and industrial workers at another—has had its own consequences for technological change. To some extent technological acculturation may have been slowed down by the persistent ruralism of African population working in cities. On the other hand, the movement from urban to rural may be a slow but significant force in the diffusion of technological values from the industrial plant in the mine of Lusaka or the factory of Kinshasa to the villages to which these urban workers periodically return. DeGregori relates a frontier theory to expanding technological horizons and economic goals. Again, his notion of a moving cultural frontier is the most stimulating in this section of the book.

Goody is less concerned with applying old concepts to new situations, and more concerned with evaluating the relevance of systemic models derived from Europe for African history and political evolution. He pays particular attention to the concept of "feudalism" as abstracted from European experience, and as applied to oriental and later African state structures.

Both Goody and DeGregori are prone to moralize; they address their preaching to scholars generally and to those who live in Africa. DeGregori advises Africans not to be ashamed of borrowing new technology from others (e.g., p. 66); Goody advises contemporary scholars not to be ashamed of learning "from the studies of medieval historians" (p. 15). Goody is the more subtle and probably more disciplined; DeGregori has obviously acquired massive scholarly material and seems keen to share as much of it as possible with other scholars. Such a predisposition is a value, but it can lead—as often in this case—to a clutter of facts and quotations from others, inadequately integrated. DeGregori's book would have benefited from the application of such "technological values" as organization, economy, and discipline.

But having said all that, we are still left with a persistent admiration for the work both Goody and DeGregori have done as pioneers in the new subfield of "the politics of technological change."

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Catholic Radicals in Brazil. By Emanuel de Kadt. (London: Oxford University Press, 1970. Pp. 304. \$9.75.)

This is a case study of the Brazilian organization MEB (Movement For Basic Education), a church-sponsored, government-financed program which since early 1961 has been active in rural areas of Brazil's less developed states. Operating both through radio transmission, assisted by community monitors, and training sessions conducted by visiting teams, MEB has emphasized literacy and other basic education, self or community help training, and what might be called political socialization and mobilization—all at different times in the organization's history and in different parts of the country.

The book's title excites the reader's curiosity about the nature of radicalism and its connection with the church in Brazil. The answers lie in the origins of several organizations. As de Kadt recounts in Chapters 4 and 5, one in particular—University Catholic Youth (JUC)—underwent a period of great internal strife during the mid-1950s. The conflict focused on the desire by militants for greater activism and posturing on sociopolitical issues versus the more conservative position espoused by some members of the church hierarchy. Opposition by that hierarchy and the intellectual preoccupation of the main body of JUC with other issues caused activists to seek another vehicle for social action. AP (People's Action) was founded by the most militant JUC's along with others, outside of Catholic student circles; ultimately, MEB's leadership cadres were drawn from AP.

The *weltanschauung* of AP (chapter 5), de Kadt tells us, emphasized full human development, first through the elimination of the structures of capitalism, but more important, through individual and group self-awareness, understanding, and choice. The author considers this movement as basically populist, and he contrasts it with "massification," which literally means being politically manipulated as part of a mass. AP's analysis of Brazilian society employed the conceptual pair of the dominated pole and the dominant pole, derived dialectically from the history of domination, as per Marx and Hegel, applied to Brazilian social history. Industrialists, financiers, and land owners, with overlapping interests were believed to make up the dominant pole; communism, the AP declared, had merely substituted the state as the dominant pole and had failed to allow plural participation. This epistemology created a problem for MEB's cadres—namely, how do those of the dominated pole develop awareness

of their condition and thereby the ability to seek their own interests without manipulation by MEB change agents? The idea of *Conscientização* (consciousness raising) as the process of developing awareness of social and economic conditions emerged (Chapter 6). *Conscientização* was conducted as part of literacy training using lessons which describe the existential situation of the poor such as their economic condition and treatment by land owners. Also, gradually, through discussions, students were to become aware of their environment's potentialities.

As de Kadt makes clear, the Catholic church in Brazil by no means takes monolithic stands on sociopolitical issues, and so long as the activists of MEB did not penetrate the zone of indifference of conservative bishops or threaten the security of the church, the program was able to develop independently, though not without controversy. But several events which culminated in the 1964 military takeover of Brazil drastically affected MEB. One of these events was the issuance in 1964 of the book *Viver é Lutar* (*To Live Means to Struggle*) as the basic textbook of literacy training and *Conscientização*. The ensuing criticism of the book and the interposition of the coup caused the bishops to replace the primer with a book less radical in format, and to exercise greater control over MEB. Emphasis also shifted to literacy and religious training.

The author presents some vignettes of MEB operations in several regions of Brazil (Chapters 9, 12, 13) which demonstrate the variety of conditions among the rural poor. These differences, he says, caused MEB to respond differently to local conditions, in some cases, for example, advocating rural syndicalism, as contrasted with an emphasis on individual and community self-help. Frustration often occurred when the commitment to nondirective self-determination obliged MEB cadres to accept fully what was decided by the people. In many cases no decisions were made.

The author concludes his analysis with three general hypotheses about populism (page 267 ff.): (1) "populism is unlikely to survive when it is harnessed to political mobilization"; (2) there is an inverse relationship between the effective purity of a populist movement and the presence of other movements which are not inhibited about manipulating people for their own ends; and (3) the maintenance of populist purity is inversely related to the breadth of scale of the movement. These generalizations are important contributions to the literature on political movements. The book also provides

information on Brazilian politics during the Goulart regime, with insights on the radical student movement and attitudes of the Catholic church particularly toward improvement of the life of Brazil's rural poor.

In concluding, I have two critical comments, neither distracting from my overall positive evaluation. First, the author uses the term "populism" in its generic sense to mean a grass-roots movement arising outside of established political channels. In Brazil, however, the concept has been associated more narrowly with what he identifies as "massification," especially with reference to mobilization of urban labor under Vargas and Goulart and its direct control through the Labor Ministry. The author does not reconcile this inconsistency explicitly, but implies that the two concepts are connected because they both produce political socialization (page 36). While this is perhaps true, the theoretical inconsistency remains between the connotation of mass manipulation and that of self-determination.

The second criticism concerns *concientização*. The peasant who has no subtle understanding of his social reality does not suddenly develop this awareness without some relevant experience. In the MEB case, the experience was indoctrination, but one must infer this fact from the author's analysis, since he does not acknowledge it explicitly. This is in spite of the ample evidence he presents in case studies and quotations from *Viver é Lutar*. Perhaps de Kadt's personal identification with MEB caused him to overlook this point. In any case, the issue is an important one, for again it makes explicit the inconsistency between manipulation and self-determination.

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Ulster at the Crossroads. By Terence O'Neill. (New York: Humanities Press, 1969. Pp. 201. \$4.50.)

The Price of My Soul. By Bernadette Devlin. (New York: Random House, 1970. Pp. 224. \$1.95.)

Ireland Her Own. By T. A. Jackson. (New York: International Publishers, 1971. Pp. 514. \$1.95.)

Few participants in the affairs of Northern Ireland are likely to write books, and the field is left largely to the journalists. So the books by Bernadette Devlin, anti-Unionist leader, and Terence O'Neill, former Unionist premier at Stormont, provide useful insights of those en-

gaged in the action. Miss Devlin's book is a mixture of autobiography and personal accounts of her role in the conflict. O'Neill's book is a collection of his speeches with an introduction and running comment by John Cole.

Jackson's *Ireland Her Own*, first printed in 1947 and now reissued with an epilogue by C. D. Greaves, is a conventional recital of nationalist Irish history with an occasional Communist twist. Jackson argues that the Irish nationalists and the English proletariat have a common cause against the English financial power and together can end partition. But the weight of evidence is that political power mobilized in England solves English problems and that Irish alliances with English power have proved expensive. Perhaps he relies upon international socialism to assure a just outcome. He says, "... one looks with confidence for an Irish enthusiasm for the Soviet Union parallel with that of Tone for the revolutionary people of France" (p. 19).

Socialism and Irish Nationalism are equally obnoxious to the Unionist leaders, but they themselves have no adequate answer to the plight of the country. The grace, moderation, and understatement of Terence O'Neill's language in *Ulster at the Crossroads* seem out of place in Belfast, as the premier discovered when he was ousted from office by the local leaders of his own party. For the most part the language was gentle and even timid when he advocated reform, especially before mixed audiences. But the majority of his speeches appear to have been directed to Unionist audiences, and tucked in among the admonitions to reform are echoes of the traditional Orange slogans of "Not an Inch" and "No Surrender." Speaking of reforms he said, "I would suggest . . ." Speaking of the maintenance of the British connection he insists, "We must . . ." Perhaps, had O'Neill been able to develop a program of social justice ending the old establishments, and had he found words to transcend the local history and sectarianism, he might have rallied the overwhelming majority necessary for reform. But Devlin and Jackson agree that this would have been the path to socialism, a path O'Neill could never take.

Nor have the Unionists appreciated the urgency for drastic change in the pattern of segregation. O'Neill, though dedicated to ending religious strife, saw nothing wrong with denominational organization of social institutions. As a remedy for the difficulties in a dual society he suggested that Catholic and Protestant organizations should associate themselves as "twins" as do the Chambers of Commerce in the paired

cities of two foreign lands. Replying to the criticism of gerrymandering he claimed the Stormont Parliament was democratically elected and made no mention of the local councils where the problem was acute.

O'Neill's essential goal was economic progress coupled with toleration. But as Devlin says, he was unwilling to pay the price in surrendered privilege. For economic progress he urged hard work and loyalty on the part of the employees, and attractive living conditions for employers so that skilled and imaginative managers might be lured from Britain. For toleration he urged that Nationalists give allegiance to the constitution and at the same time he asked that Northern Ireland's "peculiar institutions" and folk festivals of parades and vengeful challenges be cherished as a heritage. Bernadette Devlin correctly describes his program as enough to frighten Orangemen and not enough to satisfy their opponents.

It is evident from the events which have transpired since O'Neill's departure from office that no premier of Northern Ireland can bring peace to the province, and the situation is instructive for those interested in federalism and devolution. A local legislature at Stormont allows the Westminster leaders to escape responsibility, and for fifty years the Westminster Parliament has generally ignored the province except to provide a subsidy. Yet the Northern Ireland economy is dependent upon the decisions made in London. Since Stormont deals with few significant issues other than the defense of the regime, which the Unionists insist is not debatable, the authority of the Stormont ministers in the Unionist Party is slight. O'Neill ruefully recognized in one speech Madison's maxim that in such small political communities special interests may exercise great power. The real political power rested with business leaders, local party leaders, and the Orange Order. O'Neill's speeches reveal the weakness of his position. It is not surprising that O'Neill was overthrown by the local party bosses after being hounded by Ian Paisley and thrown off course by the Queens University students. It is no wonder that Nationalist representatives find attendance in the Parliament pointless and that Bernadette Devlin treats Stormont with contempt.

Indeed, Bernadette Devlin speaks in no muted voice. She said she needed no notes for her maiden speech in Parliament when all she had to do was look into the face of a Unionist member and find in it reminders of all she wanted to say about the tragedy of her country. Her origins were humble and her childhood trying, so this youngest member of Parliament

speaks with the voice of experience on at least those matters that concern her. Her socialist direction follows no ideological lines but seeks relief from poverty and unemployment and from bigotry and parochialism. Somewhere beyond, perhaps in the socialist nationalism of James Connolly, she hopes to find peace and economic well-being.

WARNER MOSS

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Apathy and Participation: Mass Politics in Western Societies. By Giuseppe Di Palma. (New York: The Free Press, 1970. Pp. xix, 263. \$6.95.)

The nicest thing about this book is that the author is willing to speculate—to risk venturing where strict attention to presumably "hard" empirical data would not permit. About a third of the study is relatively "soft"; it involves the author's judicious and inventive use of historical and contemporary information to explore problems of political participation in modern democracies and complex societies. It includes too a most interesting comparison of structural factors that impinge on participation in the United States and Italy. Without these chapters, the book might well be cited as a limiting case of methodological/empirical overkill. The good that will perhaps derive from the methodologically exhaustive analysis is that the book may become a capstone in the use of data gathered thirteen years ago by Almond and Verba.

In addition to intellectually engaging speculation, the author provides a clear and thoughtful introductory essay on the relationship between nation building, modernization, and participation. Many readers will want to challenge some of his claims about the relationship between class, ideology, and participation. But the introduction is quite important in that it sets the premises and delineates the conceptual bases for the hypotheses to be explored later.

My deepest reservations about Di Palma's study involve some of his conceptual distinctions and the use of indicators derived from the Almond-Verba data for central concepts and variables. For example, I found the basis for distinguishing dissatisfaction from disaffection to be very weak. Equally questionable are the indexes from which degrees of dissatisfaction or disaffection or both are derived. Whatever may be the quality of the data gathered by Almond and Verba, it is probably dubious procedure to base major indexes (that then become independent variables) on as few as two or three items in the Almond-Verba questionnaire. If my reservations here make sense, the prob-

lem is quite serious, since one of Di Palma's major efforts is to show that it is disaffection and not merely (and *never* directly) dissatisfaction that leads to withdrawal and nonparticipation.

I was also struck by how difficult it is for writers to adhere rigorously to one and only one empirical referent for a key concept. In this case the concept is participation, the major dependent variable in the study. It is clear enough that when the author arrays data and comments on statistical relationships he means by "participation" all of the items that go into his "political participation scale." It is less clear, however, whether he means more than voting when he discusses other writers who have dealt with participation or when he is writing more generally about the impact of, say, education or social status on participation.

It is possible that the cursory reader of the analytical chapters will conclude that many of Di Palma's findings are already well known, given *The Civic Culture* and, say, the astronomical number of seminar papers produced by graduate students with access to the Almond-Verba data set. We surely already knew that Italy is less participant than Germany, the U.S., and the U.K.; that participation increases as one's sense of efficacy increases; that the more "pride-in-nation" one expresses, the higher his participation score; that education is positively related to participation, and so on. What Di Palma does, however, is considerable analysis in depth to give us much more refined propositions in this area. Thus, to cite one example, the strongest statement to make about sense of efficacy and participation is that a lack of the former results in almost total actual nonparticipation. Or, to take variables like income and education, Di Palma persuasively shows that income is stable across the four nations as an independent variable and further that its impact on participation is negligible when education and occupation are controlled. Furthermore, he demonstrates very well indeed the extent to which factors like education and occupation weigh *differently* on participation as one moves from one country to another.

This latter finding provides an important bridge or transition from the more aridly empirical to the more speculatively empirical and normative concerns of this volume. For one of the most arresting suggestions Di Palma offers is that a cumulation of structural conditions, including life chances, not only influences political participation but creates highly stable, self-perpetuating subcultures of nonparticipation. Thus, in the cases of both Italy and the United

States, poverty alone will not account for disaffection from and nonparticipation in the polity. Historically rooted cognitive systems and a middle-class definition and style of politics may very well contribute to deeply institutionalized patterns of nonparticipation. Similarly, political processes dominated by middle-class definitions, styles, and values will tend to diminish certain kinds of political opposition and to underscore the meaninglessness of certain forms of participation such as voting. Although Di Palma may not intend it, his study provides considerable reinforcement for the critics of democratic pluralist theory, for members of the New Left and the so-called "counter-culture," and for all of those, from Peter Drucker to Herbert Marcuse, who have underscored the essentially spurious quality of voting as a genuinely efficacious political act in a highly industrialized society.

Without providing any detailed guide to what might be, Di Palma stresses the need to search out new forms of participation. He joins a growing number of writers who lean in the direction of both decentralized and corporativistic modes of participation. He is concerned that contemporary complex societies cannot meet conditions of democracy and of democratic opposition delineated by generations of democratic theorists. He is equally concerned that archaic forms of participation when insistently propounded and demanded in the name of the collectivity may enslave rather than liberate the individual. Evoking a value that can easily be obscured in our race for scientific elegance, Di Palma says, "The ultimate aim is not the polity; it is man and his ability to develop fully his best qualities." There is much in this book that demonstrates both the limits and the promise of political science in helping societies to realize that ideal.

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Resolving Conflict in Africa: The Fermeda Workshop. Edited by Leonard W. Doob. (New Haven: Yale University Press, 1970. Pp. 209. \$7.50.)

The study of conflict and conflict resolution is the concern of numerous social scientists. Surprisingly few, however, have dared to subject their proposals to the acid test by applying them to an intense dispute such as can occur between nations. The present volume reports such an attempt and must be regarded as an important landmark in social science intervention in public policy. Doob and his colleagues describe a workshop which they conducted at a

South Tyrolean hotel during August, 1969, in hopes of finding a solution to the border disputes affecting Ethiopia, Kenya, and Somalia. At the heart of the conflict are pieces of land occupied primarily by Somalis, and thus claimed by Somalia, but comprising parts of the nations of Kenya and Ethiopia. For a decade there have been intermittent clashes—and at times armed conflict—between the Somalis residing in these areas and the police or soldiers of Kenya and Ethiopia.

The organizers of this unique encounter were interested in whether sensitivity training could be used as a method for facilitating the resolution of international conflict. Could participants "in the detached and permissive atmosphere of a workshop . . . express and then modify some of their attitudes and values, improve their communication skills, reveal the deeper psychological or emotional issues in the disputes, and thus move closer to an innovative resolution" (p. ix)? Although the African participants were not policy makers of the three countries involved in the disputes and did not officially represent their governments, they were thought to have some informal influence within their governments or at least some access to the men in power. The eighteen African participants (six from each of the three countries) were divided into two training groups (T-groups). The T-groups spent the first week of the workshop learning how to function in an unstructured environment without formal leadership or agenda. During the second week the T-groups discussed the border disputes within the same unstructured environment. Mixed with the meetings of the T-groups were meetings of all participants in a General Assembly.

In an attempt to get as accurate a record of the workshop as possible without using recording equipment, Doob has collected a series of statements from all the types of persons involved in the workshop. There are chapters by three African participants (one from each country), several chapters by the trainers (leaders in the sensitivity training), and several chapters by the organizers (Doob and his associates—William J. Foltz, a political scientist, and Robert B. Stevens, a lawyer). In his preface Doob notes that each contributor was told to write about the workshop as he experienced it. Thus, the reader learns about the Fermeda Workshop—both its successes and failures—from multiple perspectives. To Doob's credit as editor, little redundancy occurs among the contributions. In many respects this book serves as a manual for others wishing to experiment with sensitivity training as a means of resolving con-

flict in an international setting. Particularly salient to any future attempts is the chapter by Richard Walton, one of the trainers, who thoroughly discusses the basic issues in designing mediation efforts such as those used in the Fermeda experiment.

Based on the written report of the Fermeda Workshop, does sensitivity training appear useful as a means of resolving international conflicts? The answer is equivocal. The participants failed to agree on a proposal for resolving the conflicts among the three countries. Although each of the two T-groups, which included representatives of all parties, arrived at a possible solution, the participants were unable to unify the two proposals. Thus, the workshop ended on a note of dissent. The participant-contributors to the book (an Ethiopian philosopher, a Kenyan political scientist, and a Somalian lawyer and journalist) suggest that sensitivity training is more useful for becoming personally acquainted with the so-called enemy and learning about his attitudes and feelings than for resolving conflict. As the Somalian contributor states: "The personal respect and trust established in the workshop may eventually have direct political consequences. The participants were men of some standing in their countries and could conceivably come to the top. . . . The personal links of comprehension could then be invaluable" (pp. 55-56).

The trainers and organizers remain ambivalent about the success of the workshop. In the last chapter, the organizers indicate their conviction that the conflict could be resolved through the establishment of a Joint Administration Area for the regions under dispute. Both trainers and organizers, however, like the African participants, view their primary success as the substantive interaction of individuals closely identified with governments manifesting intense hostility toward one another—the dialogue among these individuals occurring in a permissive enough environment to promote the probing of an issue important to the conduct of their nations. In actuality the ultimate success of the workshop in resolving the conflict remains uncertain. Several questions require answers. Have the participants discussed the workshop with those in their governments responsible for foreign policy? Have the foreign policy organizations in the three countries considered the alternative solutions proposed in the workshop? Are the governments now in power disposed to settle the conflict—in other words, how salient is the issue to them?

Perhaps instead of perceiving the technique used at the Fermeda Workshop as a means of

resolving international conflict, one should view it as an adjunct to more formal types of negotiation. Once parties commit themselves to seeking a solution to a conflict, sensitivity training could be used to provide the environment conducive to creative solutions. An experience of the Fermeda type could occur during that phase of negotiation when possible solutions were actively being sought.

This book serves as a useful companion volume to John W. Burton's *Conflict and Communication: The Use of Controlled Communication in International Relations* (London: Macmillan, 1969) which describes the work of the Centre for the Analysis of Conflict in the University of London. The Centre has employed "controlled communication" in two workshops that have had official government representatives as participants and that have focused on international disputes involving military operations (e.g., Cyprus). Both of these volumes chronicle the beginning of the application of sociopsychological techniques to international conflict. Whether or not the conflicts have yet been resolved, these first halting attempts to apply such techniques to foreign policy disputes add a new tool to man's inventory for conflict management.

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History in Geographic Perspective: The Other France. By Edward W. Fox. (New York: W. W. Norton, 1971. Pp. 190. \$6.95.)

The central thesis of this book is that in medieval and early modern France up to the middle of the nineteenth century, two different types of economy and social structure were produced, one in areas that were accessible only to land transportation and one in those that had access to water. The high cost of land transportation meant that although the financial accounts of an area might be balanced in Paris by money taxes and money public expenditure, the physical accounts, the supply of goods in trade, had to be balanced in a small area around a small market town; Fox calls this "subsistence" agriculture. The physical accounts of seaport cities (Bordeaux, Marseille, Nantes) and major river ports (Paris, Lyon, and Rhine cities) could be balanced from distant plantations of the Atlantic, North, and Baltic coasts.

This implied that in the interior there were basically two possible forms of economy—a feudal economy in which the local financial, taxation, and rental accounts were balanced locally under the control of the local nobility or

provincial bourgeoisie, and a royal or imperial economy in which taxes and rents went to Paris and were redistributed by the civil service back to the areas where the physical goods were available. The conflict between these two alternatives for the territorial state shaped the political conflicts of the central government with the nobility or the local rich of provincial cities for rural and small-town France. But in contrast in port cities, the central economic relations are free bargains with people elsewhere under a different government, in an Atlantic community, rather than—as in rural areas—authoritative exactions of rents, seigneurial dues, and royal taxes. Therefore the natural government of such cities is a negotiating oligarchy of merchants, of Whig parliamentarians. The feudal versus monarchical conflict of the interior, and the natural alliance of the king or Bonapartist emperor with the peasantry against the exactions of the nobility, do not touch their interests. They go their own Atlantic way except for resistance to mercantilism and an occasional binge of constitution writing.

There are two questions about this: whether the geographical variation is right, and whether it is convincingly connected to varying political behavior. Geologically, Northwestern Europe is divided into three main base rock formations. The limestone bottoms of ancient flat shallow seas occur in two subvarieties, the heavy clayey plains with fertile basic soils of the Seine, Loire, and Aquitaine basins in France, the Southeast of England, the Low Countries, and Denmark, and second the folded limestone hills pushed up around the Alps and the Central Massif, wet and infertile in the North (Jura) and dry and barren on the Mediterranean coasts of Provence. The clayey ancient sea bottom plains are laced with slow meandering navigable rivers; canals and roads are easy to build; the high fertility means that each mile of waterway or road gives access to a lot of surplus food. By the eighteenth century the quantity of food accessible was increasing by the spread of intensive Flemish farming with much use of manure from stall-fed animals, mineral fertilizer, and crop rotation, giving a yield of 10 or 12 bushels of harvest per bushel of seed, in contrast to 4 or 5 to 1 for the three field fallow system. This new system was conquering the plain by about 10 miles a decade as it progressed from Flanders toward Paris.

The second geological formation type is the ancient crystalline rock massif, of Brittany, the Massif Central, the Vosges-Schwarzwald cut by the Rhine, and Wales and Scotland. These are much eroded, with the cultivable alluvial soils

being relatively sandy, acid, and infertile. These soils are found more frequently as the rivers that rise in the massif flatten out and slow down to enter the plains around the Massif Central and the Vosges, or the sea around Brittany. Throughout these areas cultivation has apparently always been much more intensive in the river valleys such as the Rhine and the Rhone, and on small delta areas where non-navigable rivers come down to the sea.

The third geological formation is new alpine mountains, wet on the Northern side of the Alps and the Atlantic side of the Pyrenees, and dry and barren on the Mediterranean side of both, except for small pockets of alluvial soils at river mouths. These areas were not very fertile nor heavily populated, and were mostly administered lightly and feudally by the crown as more or less autonomous *pays d'état*.

From this geographical situation it appears that most of the fertile land, most of the intensive agriculture, and most of the people must have been concentrated on plains laced with navigable rivers or along the coasts. I would guess that between half and two thirds of the denser plain populations of the Seine, Loire, and Aquitaine basins and in the Rhineland must have lived within three miles of navigable water. In the Massif Central, Brittany, the Alps, and Provence-Languedoc, I would guess that between a third and a half were in river valleys and deltas or along the coast. We find that even in Limousin in the central massif, Turgot as Intendant arranged to have grain bought in the Low Countries and shipped up the river to relieve a famine. The foreign trade of France in the eighteenth century seems to have increased about ninefold; it is very unlikely that this could have taken place without increasing supply of goods from the hinterlands along the river courses.

This means that the French peasantry mostly *could* participate in commerce. That is, the geographical situation of most of the French peasantry, when closely analyzed, does not support Fox's allegation that rural physical accounts had to be balanced in a small area. Maggiolo's data on literacy in the eighteenth century shows 60 per cent or more with literacy enough to sign the marriage register among males all across the Seine and Loire basins, and more than 35 per cent down the Rhine, near Marseille, and in the Aquitaine basin near Bordeaux. If literacy reflects involvement with urban culture, this supports the idea that peasants in the rich and populous North were extensively involved in their daily life with the city. The density of the network of royal roads in

the Northern plain, with sparse roads in the Massif Central, Brittany, and the Midi, supports the same notion. The canal building of the seventeenth and eighteenth centuries suggests that somebody was moving things around in, and between, the Seine and Loire basins.

In sum, Fox's assertion about the subsistence, noncommercial character of most French agriculture by the 18th century is at least not proved, and it looks unlikely for those areas with the most peasants and on which the core of royal administration was based, the jurisdiction of the Paris *parlement* in the Seine and Loire basins. If I am right that there was much commercialization, there should be high correlations between grain prices at various points throughout the Seine and Loire basins with grain prices in the great international markets in the Low Countries, but little correlation of all these with grain prices at, say, Grenoble. If Fox is right, there should be marked regional variation in the direction of price movements even on the plains. Louise Tilly's analysis of the correlations of price movements (*Journal of Interdisciplinary History*, 2, No. 1 [Summer, 1971], pp. 35-45) supports the idea of extensive development of the national market by the eighteenth century.

Now to the politics. The major specific connection between geography and politics offered seems to be that the constitutionalists in the Revolution were called Girondins after the estuary of the Garonne serving Bordeaux. The rest of the analysis is pretty straightforward elaboration of the Tocqueville-de Jouvenel argument of the growth of the state putting the nobles first out of work, and eventually out of power, with the added Marxist speculation that the peasants liked strong kings and loved Bonaparte. It was a brilliant speculation 120 years ago; the fact that it is still a speculation says bad things about the historian's craft. The analysis is a modified version of that form of Marxist history which narrates political events and actions of governments and factions, but simply calls the actors by the names of social classes. Fox calls the actors by the names of social classes and geographical types of economy.

What is needed to demonstrate the connection between geography and politics is the extension backward of Andre Siegfried's brilliant, detailed studies of how political behavior varies with geographical situation, carried out in the Brittany massif in contrast to the surrounding plain, and in the Ardeche river valley versus mountain. We have a first-rate example of what is needed in Charles Tilly's *The Vendée*. Gilbert Shapiro and his co-workers have promised

us for many years an analysis of the grievances of the *cahiers de doléances* by the social and economic character of the areas from which they come—an analysis which should settle the question for the last years of the old regime. Fox's general postulate of the conservative character of the peasantry seems to me not proved by election studies for the modern period; I imagine there was a lot of variation in the old regime as well. All of these examples show that connecting geography to politics by way of geographically determined economic and social structure is a lot harder than Fox thinks it is.

My overall evaluation is that Fox has set himself a fascinating task. But he has failed both halves of it, by not showing that the peasantry of France was mostly commercially isolated, and by not showing that people from different geographically determined economies behaved differently in politics.

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Guerrillas in History. By Lewis Gann (Stanford: Hoover Institute Press, 1971. Pp. vii, 99. \$3.95.)

There is little evidence that there has been any slackening of academic interest in the guerrilla or his opponents. Historians, participants, organizational theorists, political scientists, journalists, all sorts and conditions continue to add to the literature each in his own way and each apparently without at all diminishing the challenge of the guerrilla. The guerrilla has been approached by a variety of academic means from systems analysis to hagiographic description, for a variety of purposes, many beyond academic confines, and still there is not even general agreement on definitions. Guerrilla theories and insurrection models abound, often convincing none but the converted. And over the whole investigation of the guerrilla hangs the threat of contamination—research bought or encouraged for immediate application, research to patina over a revolutionary commitment, research that is, in effect, part of the guerrilla complex rather than comment on it.

Lewis H. Gann, well known for his work on imperialism and Central Africa, in his brief *Guerrillas in History* is almost completely divorced not only from the potential application of his investigation but also from most of the analytical arguments. He has expanded an article in the *Military Review* into a brief, chronological survey of guerrilla tactics from the Maccabees to Indo-China. There are only the most

limited conclusions and those hedged with qualification—no theory, no new methodology, no novel definitions nor analytical insights. For those interested in lessons to apply in the field or political ideas concerning the nature of guerrilla-insurrection—or simply novelty—*Guerrillas in History* will offer little. Yet the monography, a sip of cool water in a flood of vitriolic analysis, has certain simple virtues. In less than a hundred pages, Gann has given a balanced survey of the long history of guerrilla tactics. In any such undertaking, the odd quibble of detail will arise; but Gann's book is sound, touches most of the crucial areas with a sure hand, displays no quirks or hobby-horses, and includes some novel examples—Matabele tactics, or the Anglo-Norman campaign against the Welsh. His few, tentative generalizations are difficult to dispute: partisan operations have severe limitations without contact with a regular force; partisan war exacts a grim price. Ever mindful of the particular, Gann is scrupulously careful to limit his conclusions, "Mass support is vital; yet masses on their own can never win" (p. 87). The burden of the book, however, is briefly detailed in his preface.

Guerrilla warfare has acquired an aura of romance that once used to be reserved for the exploits of pioneers in the Wild West. The very fashions of many urban intellectuals—berets, boots, and all—have been influenced by the real or supposed examples of Cuban partisans. Yet guerrilla warfare is neither romantic nor new, nor has it always served a revolutionary cause. Neither is it a certain road to victory (p. vii).

Gann's conclusions, or assumptions, are by no means accepted as truisms by many guerrilla theorists and most guerrillas in the field. For them Gann is writing about tactics divorced from strategy, about limited reverses in powerful and certain historical directions. For the more academic and theoretical of mind, Gann's book may in fact be true but on such a particularistic, episodic level that his contribution to the understanding of the guerrilla is negligible. Be that as it may, Gann has written a tidy pocket history of guerrilla tactics, suggested a few conclusions of limited scope and, as a historian, eschewed theory and polemic. Few within the academic guerrilla-establishment can claim as much.

J. BOWYER BELL

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Les socialistes et la guerre: La S.F.I.O. et la politique étrangère française entre les deux guerres mondiales. By Richard Gombin. (The Hague: Mouton & Co., 1970. Pp. 271. D.G. 25.)

Contrary to the popular assumption that an outside threat compels cohesion, Richard Gombin demonstrates that in the late 1930s external dangers generated by the fascist dictators accentuated differences within the French Socialist Party (then known as the S.F.I.O.) instead of inducing unity. General agreement on foreign policy issues in 1920 permitted the leadership to synthesize differences, but twenty years later the S.F.I.O. was in a state of virtual schism, split between the advocates of concessions (the *pacifistes*) and those of firmness (the *bellicistes*).

From its origins, according to Gombin, the S.F.I.O. "carried the germ of its future decomposition" (p. 256) in the incompatibility of doctrinal pacifism with national defense. Although the Socialist Party was able to maintain a facade of unity during most of the interwar years, the fundamental questions of national defense in an increasingly threatening international environment finally disclosed irreconcilable positions. Therefore, "it is points of doctrine which explain the final collapse" (p. 256) of the S.F.I.O., the differing interpretations and policy preferences in the face of war emanating from ambiguities inherent in the Party's ideological foundations.

The author's analysis is primarily based upon the writings and speeches of Party leaders, gleaned from Party newspapers and records of the Chamber of Deputies and Party Congresses. As Gombin notes, many documents concerning the S.F.I.O. disappeared during World War II, and little has been done with what remains. Consequently, Gombin has made a significant contribution to our knowledge of the S.F.I.O.'s intraparty disputes over foreign policy in the late 'thirties by bringing this material together.

Gombin organizes his study of the foreign policy of the S.F.I.O. around three themes and deals with each chronologically: the problem of war debts, involving a quarrel with the government over means not ends, for Party and government agreed about the need for reparations but not about how reparations should be made; the organization of national security, considered by the S.F.I.O. to necessitate support of the League of Nations and other international peacekeeping agreements; and the march toward war, which marched the S.F.I.O. toward collapse.

During the decade of the 'twenties, the façade of party unity cloaked party differences, but in the 'thirties successive League failures and growing pressures from the fascist states increasingly augmented divergencies within the

S.F.I.O., and its followers aligned with either Party spokesmen such as Léon Blum now calling for firmness or those urging concessions to counter the fascist threat. In detailing each disruptive juncture of these troubled years, Gombin objectively presents the arguments of the contesting Party factions. Ethiopia, the remilitarization of the Rhine, the Spanish Civil War—to name a few of the events discussed—all resulted in doctrinal disputes which led the S.F.I.O. to the 1940 rupture. Should Socialists cooperate with a bourgeois government in order to defend France? Should democracy be made safe in France and the revolution left to an unspecified future? Should Socialists fight their own bourgeoisie and take advantage of the situation to take power by direct action in collaboration with the Communists? Should there be a general strike to immobilize the war machine? Both *belliciste* and *pacifiste* found ideological justification in their commitment to peace and socialism, but their antagonistic interpretations ultimately sundered the Party.

A general familiarity with the S.F.I.O.'s organization, history, and domestic doctrines are assumed by the author, and the subtitle well describes the book's subject matter: the S.F.I.O. and French foreign policy between the two world wars. By concentrating on factional differences as revealed by his sources, Gombin generally neglects considerations of domestic political pressures on foreign policy and ignores personalities. Would it not help us better to understand factionalism in the S.F.I.O. if we knew something more about Paul Faure, Jean Zyromski, and Marceau Pivert? Indeed, the reader gains the impression of an intellectual contest somehow removed from men and the strife of French political reality (with the exception of the discussion concerning the intervention-nonintervention dispute during the Spanish Civil War). In short, politics is markedly absent in this study of a political party.

It must be said, however, that Gombin's three aims largely divorce him from considerations other than the purely doctrinal: to set forth and analyze the official party positions on foreign policy; to define the attitudes and preferences of the factions within the party in order to present the entire "physiognomy" of the party; and to explain how positions taken on foreign policy were motivated by ideological factors. The author himself observes that "Especially valuable to us would be a study which would attempt to explain how and why different tendencies evolved" (p. 151). Gombin admittedly does not provide the "how" or the "why," but he has presented a solid view of the

"what" of S.F.I.O. factionalism in foreign policy within the framework of his preoccupations. Although frequently commented upon, these divisions have seldom been scrutinized. This descriptive study, however, makes no effort to go beyond the framework in terms of analysis or generalization, as concerns either national or comparative political behavior.

ORVILLE D. MENARD

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Guerrilla Movements in Latin America. By

Richard Gott. (New York: Doubleday and Company, Inc., 1971. Pp. 626. \$10.00.)

In 1967, when Richard Gott was asked to review an English edition of Regis Debray's *Revolution in the Revolution?*, he wrote, "such is our ignorance of Latin America, the Englishmen in a position to query the factual basis upon which (Debray's) theories are erected can be counted on the fingers of one hand." To remedy this situation, Gott wrote *Guerrilla Movements in Latin America*, a 626-page volume, most of which consists of detailed historical, chronologically organized accounts of the five major rural guerrilla movements inspired by the Cuban Revolution—those of Guatemala, Venezuela, Colombia, Peru and Bolivia.

The information Gott presents, which was gathered "mostly from . . . obscure left-wing periodicals, from newspapers (and) occasionally from people and books . . ." (p. 8) does provide the reader with a basis for evaluating several of Debray's major theses. Whereas Debray stressed "the essential unity of the Latin American Revolution" (p. 11), Gott's narrative emphasizes the disunity and incomparability of the various peasant uprisings, officers' revolts, labor strikes and student riots. The failure to date of all attempts to operationalize the *foco* theory of rural guerrilla warfare also raises doubts as to the relevance of the Cuban-inspired model to other Latin American countries. On the other hand, the ephemeral commitment of most of the Latin American Communist Parties to the various guerrilla movements seems to support Debray's critique of these parties.

Data, however, rarely lead inexorably to a given conclusion, and so the conclusions which Gott arrived at on the basis of the information he presents are somewhat different from those noted above. Despite the failure of the attempted alliances between the Latin American Communist Parties and the guerrilla movements, Gott believes that the guerrillas need the Communists ". . . for where else will they find their manpower—and the Communists need

the guerrillas—for how else can they maintain their claim to be revolutionaries?" (p. 36). He also believes that the defeat of the guerrilla movements ". . . does not necessarily mean that in other, often-ignored countries" such as Nicaragua, Uruguay or Mexico, "the *foco* theory or a variant of it might not meet with more immediate successes" (p. 484).

Clearly, Gott is more optimistic than I am. He feels that the various reasons he offers for the failure of the guerrilla movements in the five countries he treats—the lack of unity and commitment on the Left, the United States' anti-guerrilla programs, the guerrillas' ignorance of local conditions and the nonrevolutionary nature of most Latin American peasants, all *can* be overcome in the future. The basis for this belief seems to be his conviction that they *should* be overcome. Gott's confusion of fact and value is one of the weaknesses of *Guerrilla Movements in Latin America*.

The book also suffers from an overabundance of detail and insufficient attempts to organize material into general categories of information. As a result, it is difficult to compare the experiences of the five countries discussed. The conclusions presented in the Introduction and the Epilogue, some of which have been mentioned above, are not well integrated with the chronological narratives of the country studies.

Despite its weaknesses, *Guerrilla Movements in Latin America* is superior in some ways to Luis Mercier Vega's *Guerrillas in Latin America* (New York: Frederick A. Praeger, 1969), the other general introduction to the topic available in English. The Mercier Vega book, although more readable and interpretive, provides extremely superficial accounts of guerrilla activities in eight Latin American countries. Gott's effort is more ambitious, thorough, and scholarly. His volume contains much information on topics such as the origins of the guerrilla movements, the background of their leaders, the changing strategies of the various groups, their relationship with the Latin American Communist Parties, the guerrillas' programs and their involvement (or noninvolvement) with the peasants. The eleven appendices of documents, the fifteen-page bibliography listing primary and secondary source material, in addition to the already noted wealth of information, make *Guerrilla Movements in Latin America* a significant contribution which, perhaps, will prove more useful to specialists than to the novices for whom it apparently was written.

SUSAN KAUFMAN PURCELL

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The Emergence of Modern Afghanistan: Politics of Reform and Modernization. By Vartan Gregorian. (Stanford University Press, 1969. Pp. 586. \$17.50.)

This book—the best of its kind on Afghanistan—will be a basic source for years to come, particularly since almost 200 of its 586 pages consist of extremely valuable appendixes, notes, bibliography (including pertinent Russian and Persian references), and, finally, an excellent index. The author's competence in all the languages involved in his bibliography is not just impressive, it is probably unique among Afghanists and Iranicists, as well as other specialists interested in Central and South Asian studies and their interrelationships. (Parenthetically, among other things, Gregorian taught me a new English word: *eleemosynary*, p. 337.)

I shall leave the nit-picking to those who have already nit-picked in other journal review articles, and limit myself to a brief discussion of a few of the book's salient points, taking issue (usually mild to the point of innocuousness) with those interpretations with which, as an anthropologist, I find some latitude for disagreement. For example, the undefined term "feudalism" (which many scholars seem to equate uncritically with the medieval European variety) used throughout troubles me, and I refer the reader to my article (and then the reader will have the privilege of disagreeing with me): "Medieval European Feudalism and the Contemporary Middle East," *Report on Current Research, Spring 1957*, Middle East Institute, Washington, pp. 47–55.

An interesting theme in the book is the implicit equating by most Afghan "modernists" of the introduction of secular *Western* socioeconomic and political models with modernization and progress. Particularly in the light of rekindled and, in my opinion, often positive, ethnocentrism, tribalism, regionalism—or whatever one wishes to call it—in the West, this evaluation may be open to question. Witness the Black Power, Chicano Power, Indian Power movements which, at least partly, arose because the reciprocal, functioning sets of rights and obligations between the government and the governed have broken down—or, in reality, never existed. Crushing the tribal systems *prematurely* can be more disruptive to modernization (however defined) than trying to work within existing regional units until a government can create some sort of reciprocal (and the key word is reciprocal) sets of rights and obligations with the governed.

I believe Gregorian is fundamentally sympathetic with this point of view, but feel that he may have overstated the case against the tribes

as being antireform, antiprogressive, and anti-modernization. It also seems to me that he plays down the role of those among the urban power elites who have used the modernization processes in attempts to perpetuate that illusory, impossible political institution: the status quo. Gregorian rightly points out that the reform processes have primarily benefited the urban groups. Again, rightly so, because the urban commercial community, easily accessible to revenue officers, has had more than its share extracted from it to support reform and pseudoreform schemes.

The author satisfactorily acquaints the reader with the various social, cultural, economic, and political forces, both cohesive and divisive, which led to the beginnings of modern Afghanistan. He easily handles sources from a variety of disciplines (history, political science, economics, anthropology, sociology, etc.) and knits them together into a meaningful whole.

Having established the background for the action, Gregorian divides the history of modern Afghanistan into seven periods: 1832–1839 (incipient nationalism begins under Dost Mohammad Khan); 1839–1880 (European imperialism replaces Asian imperialism as a major force in the area); 1880–1901 (Amir Abdur Rahman Khan attempts to create a modern nation-state); 1901–1919 (Amir Habibullah continues the processes begun by his father); 1919–1929 (King Amanullah gains political independence, and unsuccessfully attempts to accelerate modernization); 1930–1946 (King Mohammad Nadir Shah and his brothers and son continue modernization at a slower pace); 1946–present ("positive neutralism" and the acceleration of social, economic and political development continues under the leadership of King Mohammad Zahir and his close advisors; the constitutional-parliamentary period begins in 1964).

Gregorian orchestrates all these periods with a masterly touch, ending each chapter with a brief discussion of the achievements and failures of the ruler involved. Throughout runs the leitmotif of the necessity for creating a military base of power to back up reform programs internally and to protect the nation's integrity externally. How effective the military have been in helping bring about sociopolitical reform remains for the future, and I for one, hope there will be a Gregorian in our future to do the analysis.

In the past, all books written about Afghanistan had to be measured alongside Elphinstone's 1815 classic. We now have another yardstick: Gregorian, 1969. Although no one should question Gregorian's scholarship, some of the

younger generation of Afghan scholars may begin to question some of his interpretations, particularly in the arena of sociopolitical reforms. But, then, I am sure that is one reason Gregorian wrote the book.

LOUIS DUPREE

American Universities Field Staff

Neighbors and Networks: The Idiom of Kinship in Social Action among the Ndendeuli of Tanzania. By P. H. Gulliver. (Berkeley, University of California Press, 1971. Pp. xii, 366. \$10.00.)

Professor Gulliver's new book is an important addition to the distinguished list of studies in African kinship systems, and is at the same time representative of some recent theoretical developments of profound interest to anthropologists, sociologists, and other social scientists as well.

It concerns the Ndendeuli of Southern Tanzania, numbering somewhat more than 20,000 persons when Gulliver did his fieldwork among them between 1952 and 1954. The Ndendeuli are culturally and historically linked with several of the other Bantu-speaking peoples of this part of the East Africa, and have a relatively simple agricultural economy based upon shifting cultivation, in an area of low overall population density. In an excellently concise historical section, Gulliver traces the recent history of the Ndendeuli through their involvement with the Ngoni and German intruders, the Maji Maji "rebellion," and their subjection to British colonial rule. Their relatively high spatial mobility and obvious hostility to all these "external" influences form an important background to the detailed analysis of their kinship and local organization presented in this book.

As in many other African societies, all important social relationships among the Ndendeuli were expressed "in the idiom of kinship." But, and this lies at the heart of Gulliver's analysis, the Ndendeuli kinship system is emphatically not based upon the principles of unilineal descent so common in much of Africa and the study of which has until recent years been such a major aspect of African sociology. The present study is essentially one of a bilateral kinship system, in which kindreds, kin-sets, action-sets, networks, and clusters provide the elements of the analytical model. In the excellent central chapters on the founding, development, and structure of the local community, the process of dispute settlement, economic cooperation, and intercommunity relationships, a major contribution is made to the study of nonunilineal kinship. The author provides an extremely de-

tailed account of the "closed network" as it is manifested in the social process in three inter-related local communities. The large number of "cases" show the same individuals involved in the shifting alliances of ephemeral action-sets, more stable kin "clusters," and cooperative endeavors, and give a brilliantly clear picture of how this kinship system "actually works."

As far as it goes, his analysis is entirely successful, and amply justifies his claim that the entire range of Ndendeuli economic, political, religious, and neighborhood relations are expressed in the idiom of kinship and encompassed in their bilateral kinship system. "Kinship was essentially an idiom in which relationships were expressed—an idiom easily understood by the people, with an immediacy of its own, and one that had both moral strength and practical utility. Kinship provided a language of communication in agreed terms" (p. 351). And, taking up an issue which has been the focus of long controversy, Gulliver makes clear his conception of the nature of kinship relationships: "Except perhaps in some philosophical sense, kinship does not exist in its own right, but only as a vehicle for other things" (p. 352).

But however important the idiom of kinship and the "kinship ethic," Gulliver found that they were not sufficient in themselves as a basis for action. The two other principles of reciprocity and locality were equally emphasized, and, combined with kinship, produced cooperation. Within the overall "dogma" of kinship, which served "as a simplified symbol for other factors," the principle of reciprocity "was not only a moral principle, though that it definitely was; it was essentially a practical mode of conducting social relations" (pp. 225, 227).

Almost in passing, the book tells us a good deal about a wide range of basic issues illustrated by the Ndendeuli material, such as the critical importance of ecological factors in the analysis of social relations, the role of mediators in the settlement of disputes in systems without courts, the nature of negotiation rather than adjudication in such legal systems, and the related politico-jural basis for dispute settlement, rather than the "judicial." The diligent reader also picks up information on the structure of "quasi groups" and the utility of this concept in the analysis of social action, as well as the manipulative efficacy of influence rather than power wielded by the "big men" or notables in the community. Ndendeuli leaders must avoid even the slightest hint of compulsion and have to rely entirely upon inducing their kin neighbours to support them (p. 251).

It is in the discussion of political relations that some weaknesses appear in Gulliver's analysis. He does cite a case of one notable's enhancing his status in the local community by involvement in wider political issues and economic activities of an entrepreneurial kind (pp. 103-104). But we get no more than a hint of what are obviously hostile attitudes held by Ndendeuli toward the then colonial government, and how these attitudes affected the people's acceptance of "progressive" economic innovations foisted upon them either by the representatives of alien rule or by over-zealous members of their own society whose motives were often obviously suspect (p. 262). It would have been fascinating and theoretically important to have had a systematic analysis of how such a noncentralized, nonsegmentary society adjusted to the problems of external domination and the introduction of alien political roles and structures, and "bureaucratic-type" norms. What does emerge, however, is the intrinsic weakness that a bilateral kinship system such as this one—lacking both politically differentiated roles and corporate groups of a segmentary nature—experiences in handling political relations of an intercommunity, or "external kind" (e.g., pp. 324-327). Further discussion and elaboration of this point would have enhanced the overall analysis.

There is, to my mind, one major flaw which mars this otherwise outstanding book. There is absolutely no systematic discussion of Ndendeuli kinship terminology, nor of the "conventional attitudes" associated with kinship categories. English terms such as "brothers" or "cousins" are frequently used in contexts where their meaning is far from clear, and the cultural norms and attitudes that Ndendeuli must have about at least some of their kinsmen are largely ignored or dismissed as unimportant. Hence, although the "actuality" of the kinship relations "underlying the ideology of kinship" is beautifully demonstrated, the ideology itself is hardly discussed. Gulliver seriously underplays the sociological significance of values, norms, and even emotions about kinship relationships in any society, and too frequently dismisses them as "vague" or "irrelevant." This omission is even more peculiar since the author is obviously aware of the problem, as demonstrated in his by no means short discussion of how he tried to assess Ndendeuli attitudes and motivations in making the choices they did in situations otherwise so well described.

This analysis of Ndendeuli kinship lacks, therefore, a whole dimension of meaning, a dimension which would have told us much more

about what Ndendeuli culture and society are really like. My contention is not merely that Gulliver's account lacks "cultural clothing" or Ndendeuli "atmosphere"; it goes to a much deeper, theoretical level. Apart from the development of a network model of social relations, to which this book is obviously an outstanding contribution, it does not sufficiently treat the interaction between culture and society, currently a most significant growing-point in anthropological theory. Where there is some discussion of Ndendeuli concepts, such as that of "the owner of the land" (pp. 63-65) or the very short chapter on "Kinship and Cooperation" (pp. 216-239), the presentation suddenly takes on an added luster and depth, and I cannot help wishing more material of this kind were included throughout the book.

I have emphasized these relatively minor criticisms not to detract in any way from the importance of this outstanding book, but to illustrate the wide range of fundamental issues which it touches upon. It is a fine example of an up-to-date analysis of kinship and a major contribution to East African ethnography in that the analytical framework is thoroughly abreast of some recent developments in theory, even if the data are nearly twenty years old. Professor Gulliver is to be congratulated upon adding yet another volume to his already long list of distinguished monographs in the social anthropology of Eastern Africa.

PETER RIGBY

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Les Communistes en Espagne. By Guy Hermet. (Paris: Armand Colin. 1971. Pp. 216. 34,000 F.)

This highly informative book is by no means easy to read, but it constitutes a mine of reliable information. Despite its brevity, it deals in extraordinary detail with the origins, organization, functions, programs, personalities, and schisms of the Spanish Communist Party since its foundation in 1919 by dissidents from three existing militant organizations: the anarcho-syndicalist CNT, the Socialist Party, and the Socialist Youth, who had organized themselves separately from the official party and were more militant in regard to political tactics. The Communists had to remain clandestine until 1931. They were perpetually plagued by factional struggles, the most significant of which was the refusal of Moscow to permit Andrés Nin to become general secretary in 1927. The party had to accept the Moscow-determined tutelage of Jacques Humbert-Droz during the 1920s and of Palmiro Togliatti in the 1930s.

From 1939 to the present it has again been a clandestine, faction-ridden party.

Hermet gives perhaps the clearest account available anywhere of the tapestry of ideological and leadership conflicts whose most consistent thread is the eventual domination of the party by Dolores Ibarruri (*La Pasionaria*) and Santiago Carrillo. In its clandestine and factional experience the Spanish Communist party is comparable to the Communist parties of other underdeveloped and dictatorially ruled countries. But during the years 1936–1939 it played a major role in the Spanish Republican government and army, thereby acquiring a degree of power and prestige never attained by any other central or western European party. During the Civil War era it was more a party of bureaucrats, soldiers, and prosperous peasants than it was a party of industrial workers and poor peasants. It publicly defended the bourgeois state and private property against the left Socialist and anarchist collectivist experiments of 1936. It held two cabinet posts, produced some of the most important commanders of the Republican army, and largely controlled the organization and supply of that army.

It should not be surprising then to learn that that party, condemned once again to clandestine operation since 1939, has preached a relatively moderate line. Thus, Hermet tells us, its program for the post-Franco years includes “bourgeois” political liberty, explicit recognition of the role of non-Marxist parties, land reform (but not wholesale collectivization), nationalization of banks, natural resources, and “monopolistic” industries (but maintenance of a large private, non-monopolistic sector), and separation of Church and state (for the mutual benefit of both, but with the retention of Catholic schools and without any anticlerical orchestration). Also, having suffered constant tutelage from Moscow both in the ‘twenties and during the Civil War, and having collaborated with bourgeois parties in the government, the Spanish party was not surprisingly more critical than all other European Parties of the Soviet invasion of Czechoslovakia; it is also not surprising that Moscow has retaliated by supporting a splinter faction under the Civil War general Enrique Lister against the official leadership of Santiago Carrillo.

There is of course absolutely no way of knowing how large or how influential within Spain is the present Communist party. Hermet evidently feels that it has very little chance of becoming a legal party again despite its moderate program. Whether Franco is succeeded by a technocratic, conservative monarchy or by an-

other military dictatorship, the Communists will not be invited to participate in political life. And the author believes that the chances for another democratic republic in the proximate future are very slender. The Franquist regime has evolved successful techniques for allowing *de facto* representation to most of the social elites and class interests of Spain—with the significant exception of the industrial working class. Hermet therefore sees the main function of the Communist party—and also of the left Catholic worker groups—to be the formation of worker elites and the maintenance of proletarian class consciousness and morale.

The author writes with full and sure knowledge of the many ideological cross-currents within the party. For structural and functional analysis he depends heavily on the work of Juan Linz. Only one aspect of the book seems to me unsatisfactory. A reader must surely suspect that behind a great many apparently doctrinal and tactical quarrels lie deep personal power conflicts. Only indirectly can one get a sense of these conflicts, and it is impossible even to estimate their relative importance in the life of an organization whose whole history has been characterized by polemics, schisms, and purges.

GABRIEL JACKSON

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The Economy of Cities. By Jane Jacobs. (Vintage Press, 1969. Pp. 268. \$1.95.)

Green-Belt Cities. By Frederic J. Osborn. (Schocken Books, 1969. Pp. 203. \$2.45.)

Neither of these paperbacks deals directly with politics, but both have implications for public policy. Mrs. Jacobs, always an interesting writer, likes to think of herself as an iconoclast. Here she seeks to put to rest the “dogma” of agricultural supremacy, or, as she writes (pp. 3–4): “Current theory in many fields . . . assumes that cities are built upon a rural economic base. If my observations and reasoning are correct, the reverse is true: that is, rural economies, including agricultural work, are directly built upon city economies and city work.” If this idol has not already been smashed, it certainly has been badly chipped. As the social scientists tell us, a city cannot grow unless its agricultural base can produce a surplus of food and fiber for use by the city folk; but as historians have long argued, human innovations have been primarily urban ever since cities first appeared. And Lewis Mumford has argued convincingly that cities predated agriculture in the development of man. Indeed,

the theory developed in this book owes a good deal to the earlier work of Mumford, whose name does not even appear in the index. (See his article, "City," in the *International Encyclopedia of the Social Sciences*.)

Mrs. Jacobs develops a detailed theory about how settlements become cities through innovation, with new work spinning off from old. It is a theory of the biological or evolutionary type, cities being alleged to have a period of youth, of maturity, and—in cities like New York today, which have ceased to be centers of discovery and invention—a period of decline. She also explains that "full" employment (or its approximation) can be maintained only if there is a furious rate of innovation, for this is what creates new work. Incidentally, the theory explains poverty as evidence of inadequate innovation. Poverty itself "has no causes. Only prosperity has causes" (p. 121). Mrs. Jacobs discusses why some trading centers become manufacturing centers while others do not, and under what conditions a city which performed one type of function during one period of its existence may perform another major function at another period. And she points out the pattern in the way cities free themselves from dependence upon imports from other cities and develop markets for their own exportation. Like the chain letter, success depends on continuous expansion into new urban markets and into their hinterlands. This is no steady-state theory. (Reading it reinforced my conviction that Japan is the Land of the Immediate Future, for Japan follows the theory in its trade agreements, while the party of the second part often does not.) According to this theory, one should think not of national economies, for there is no such thing, but of agglomerations of urban economies.

The book is written in a pleasantly informal style, with a great wealth of historical illustrations which are by no means limited to Western civilization. And for city-savers in a hurry, the entire theory is diagrammed in a nine-page appendix. The book should be of interest not only to those concerned about poverty and "decay" in our cities, but also to those interested in the economies of the "underdeveloped" (i.e., poor) nations. The theory contains important implications concerning the wisdom of much of our foreign economic aid. For politics generally, the most important problem raised is this: Those who control developed economies under any form of government attempt to suppress innovations that may threaten their positions; they have therefore a great incentive to control governments. But if innovation stops, economies

stagnate. Can we invent a government that could keep open rather than close off the opportunities for economic and technological development?

Green-Belt Cities was written by Sir Frederic J. Osborn in 1945. This soft-cover reissue includes ten pages of "Prefatory Afterthoughts" and another ten pages of notes designed to bring us up to date on the developments of the last quarter of a century. Osborn, a lifelong advocate of the garden city concept of Ebenezer Howard, wrote a book, *New Towns After the War* (1918), in which he called for the development within the United Kingdom of one hundred garden cities of the size range of 40–50,000 persons. But nothing of the sort happened. This book called for a similar public policy for the years after World War II, and this time Osborn and his supporters had better luck. The New Towns Act, 1946, was passed with all-party support, but the actual development of garden cities was slowed because the Conservatives, always less romantic than Labour, were not greatly interested in a plan that had as its goal "the marriage of town and country." The first six towns were designated immediately and eight more by 1950. During the next decade, only one more town was designated. By 1969, 28 towns were being developed.

Howard's ideas have had some influence in the United States, and a few "green-belt" cities have been developed, both during the Great Depression and in recent years, but these have been primarily middle-class commuter suburbs. In the United Kingdom, in contrast, they have been multiclass cities that are quite self-sufficient, with a manufacturing base. Overall, half of their work force is in manufacturing and the remainder in trade, services, and the professions. There is some commuting, but the principal idea is to allow for urban living under conditions of relatively low density and with a good deal of open green spaces around homes, factories, and businesses.

Osborn describes his theory of town planning and offers a nice history of Howard's two garden cities, Letchworth (begun in 1904) and Welwyn Garden City (begun in 1920). Mistakes have been numerous, he concludes, but perhaps the most common and important one has been the failure to "leave some margin of space for the unforeseen" (p. 10), for technological or other changes that are not anticipated but demand space (e.g., parking lots after World War II).

The garden city has not received much attention in the United States despite its appeal to

the American idealization of the county-seat town of two generations ago. Perhaps this is because the commuter suburb has been an adequate and simpler equivalent. Today, concern for all persons irrespective of class or status in an urban environment has been revived partly as a result of a growing awareness that the findings of research in ethology may have important implications for the innate needs of man. The words of Osborn, written more than a quarter of a century ago, may have seemed like romantic sentiment at the time; today they may well suggest a much deeper meaning:

Howard's conception of a designated country belt around a town answers to an almost eternal sentiment or desire. . . . [He] was the first to unite Plato's conception of the population-limit, [Thomas] More's principle of adjusting by short-distance migration the balance of population in urban areas, and the universal sentiment for a country setting for a town, into a clear formula. . . . (p. 180).

Perhaps a rational public policy for man, a creature of the forest and the plains, must begin with an adequate amount of green space, even for those who claim to see themselves purely as urbanites.

Osborn's book has withstood the test of time, though its idealism exceeds its economic feasibility. Jacobs' book may well also withstand the test of time, though its economic soundness may exceed its political feasibility.

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The Economic Impact of the Six-Day War: Israel, The Occupied Territories, Egypt and Jordan. By Eliyahu Kanovsky. (New York: Praeger Publishers, 1970. Pp. xxi, 451. \$15.00.)

The economic aspect of the Six-Day War was "either ignored or dealt with rather superficially" by political-military analysts, writes Kanovsky (p. xix). As a major example of the economic myopia which the author ascribes to observers of the political-military scene, he refers to unnamed works that depict the economy of Jordan as "having suffered a fatal blow when Israel occupied the West Bank." On the contrary, according to Kanovsky, "a careful study of the official statistics reveals that main economic sectors [of Jordan] are, in fact, expanding vigorously, and that investment levels are higher than before the war" (p. xix). The penetrating discussion of Jordan's capacity to survive the loss of the West Bank does away with the predictions of doom forecast by United Nations experts and United States officials.

The author's undertaking—to show the economic effect of the war and the economic ineptitude of political-military analysts—is a laudable and valuable venture. The book is divided into sections for each of the three countries and the Occupied Territories. Impact is measured and reported in a straightforward manner that shows changes in conventional economic indices. Each section gives a "picture of the prewar economy, primarily in the 1960's" of the economic system, juxtaposed with much economic data of the postwar period (1967–1969).

Unfortunately, a major shortcoming of the book is that there is extremely little direct cross-country comparison. What, for example, were the *relative* effects of the war on tourism in the different countries? To find out, the reader must search for statistics in different parts of the book. Kanovsky seldom brings the facts together, leaving it up to the reader to make his own comparative analysis. Perhaps this was beyond the author's intended scope; if so, it is a regrettable omission.

On the positive side, Kanovsky provides detailed breakdowns of the prewar economic plans of the different countries—and mentions some of the discrepancies between what was planned and what was achieved. Reference to the various plans leads to expositions of important topics. The Egyptian government's intention to give a bigger role to private enterprise, for example, is shown to have been a response to internal criticism of state enterprises and ineffective bureaucracy. In Israel, the planned cooling-off of the prewar economy resulted in a recession, rising unemployment and deepening national gloom.

Kanovsky's descriptions of the postwar economies include glimpses of such phenomena as Israel's soaring defense expenditures and its acute labor problem. The rising costs of national security are shown to be based on a number of factors: longer borders, the need for more sophisticated equipment, and the escalation of the armaments race. The labor problem is a result of the postwar boom in the construction industry. Kanovsky also points out that the economic orientation of the West Bank toward Israel has been of mutual benefit: Arab workers are imported to help solve Israel's labor problem, while Israeli experts help modernize Arab agriculture (the major sector of the West Bank). This discussion of the territories is valuable because it is new to the literature.

Mr. Kanovsky's work is undoubtedly a useful reference book, in spite of the author's own admission that a substantial amount of information is not readily available or exists in dis-

torted form—especially concerning Egypt. This limitation leads, at times, to superficial anecdotal reportage; Kanovsky does not say whether the absence or distortion of data affects the accuracy of his measurements.

The book leaves the reader hanging; it sorely needs a fifth section to tie the other sections together. Unfortunately Kanovsky has abdicated the responsibility for developing analytic categories that would aid the comparative analysis of the countries and would better indicate the relative impact of the Six-Day War on the various countries.

EFRAIM TORGONIK

Tel-Aviv University

Fragile Victory: Prince Saionji and the 1930 London Treaty Issue from the Memoirs of Baron Harada Kumao. Translated with an Introduction and Annotations by Thomas Francis Mayer-Oakes. (Detroit: Wayne State University Press, 1968. Pp. 323. \$10.00.)

An enduring problem of politics everywhere is the question of who in the government is competent to decide what. The situation in the prewar Japanese government was particularly complicated because the Constitution of 1889, which represented a compromise between Western parliamentary forms and an entrenched oligarchy reluctant to share power, was purposely vague on a number of important points. One of these involved the authority of the government to enter into international agreements which would limit the size of the Navy. Both the Army and Navy had rather early fought to establish the principle that only the armed forces were competent to decide on such matters as military planning, strategy and operations, in short on the whole question of national security.

In 1930 Japan participated in the London Naval Conference which sought to head off an arms race among the great powers by continuing, with some modifications, an allocation of ratios in naval strength agreed upon at the Washington Conference in 1922. At the time of the London Conference the government was headed by Hamaguchi Yuko, a political party leader who was anxious to avoid the escalating costs of armaments in a period of economic depression. Some high naval officers, especially in the Navy General Staff section, argued that the ratio proposed in London would jeopardize national security and challenged the right of the government to enter into an international agreement under those circumstances. In the end the London Treaty was signed and ratified, but only after a long and bitter behind-the-

scenes maneuvering within the upper reaches of the government.

Professor Mayer-Oakes's book, *Fragile Victory*, looks at this struggle from the vantage point of Baron Harada Kumao, a political secretary of Prince Saionji, the last of the *Genro* or elder statesmen who advised the Throne on important state matters. Harada functioned as a kind of "leg-man" for the aged and retired Saionji, reporting the latest news and gossip to the Prince, and carrying messages from him to important personages in and out of government. Because he was Saionji's envoy, Harada had access to all leaders; even the Emperor himself phoned him on occasion. Baron Harada had the foresight to dictate once every three or four days his recollections of his conversations with the Prince and the numerous individuals he talked with as he made the rounds in government and court circles. Some years ago the transcript of these notes was published in Japan, and *Fragile Victory* represents a translation of that portion of the Harada material that relates to the London Conference.

Professor Mayer-Oakes has made an excellent translation. The English rendering not only reads smoothly, but it also conveys the spirit of the Japanese text. He has also included an Introduction which examines the origin and background of the Harada papers as well as the political setting in which the London Naval Conference struggle occurred. Finally, copious footnotes identify personages and events mentioned in the text and cite additional relevant works.

Fragile Victory is of obvious interest to students of Far Eastern politics, but in addition it should be useful to those political scientists concerned with the general problem of how different governments arrive at important policy decisions. One will find here all kinds of revealing information. For instance, we learn that both the Navy Chief of Staff and the Vice Chief who were opposed to naval limitation made public statements and leaked reports to the press to buttress their position. The Grand Chamberlain, a retired admiral and one of the three important officials in the Imperial Court, told Harada that he felt that the Navy Chief of Staff should be more discreet and circumspect, and added that "It is highly reprehensible to drum up popular support for his own notions and then to try to push them through because of the public opinion thus aroused" (p. 103). On the other hand, Admiral Okada, whom Harada saw on numerous occasions, acted as a mediator between the pro- and anti-treaty groups. To play this role he had to act as if he

avored the "hard line" policy because his role as mediator would have been weakened if he had been tagged as an exponent of the "soft" policy. He told Harada, "I must don the mask of the die-hards and play this in order to effect a settlement." In short, the Harada memoirs give us a good deal of insight into the political culture of Japanese elites and their political style in the 1930s.

NOBUTAKA IKE

Stanford University

Le Conflit sino-soviétique et l'Europe de l'Est: ses incidences sur les conflits soviéto-polonais et soviéto-roumain. Jacques Lévesque. Preface by François Fejtő. (Montréal: Presses de l'Université de Montréal, 1970. Pp. xiii, 387. \$9.85.)

Since the very beginning of recorded international politics, it has been a truism that the fate of small states is decided by the vicissitudes of great power conflicts. It is equally true, however, that the minor states have always quickly learned how to manipulate conflicts among the great powers to their own advantage. A major conflict remains, of course, a source of sparks, real or verbal, and consequently attracts most of the attention, publicity, and also scholarly research; but significant and lasting shifts on the international scene are often rather the result of maneuvers and realignments on the part of small powers than of the big powers' direct actions toward each other.

The Sino-Soviet conflict is a classical example of a great power confrontation, and it continues to be studied primarily in these terms, with the focus of attention on the two main protagonists. The overwhelming emphasis on the great power aspect, and consequences, of this conflict is perhaps better justified in this case than in many others. The country with by far the largest territory in the world confronts the country with by far the largest population; between them runs the longest common boundary in the world. If one adds the role which adjoining India, the power with the second largest population in the world, played in this confrontation both politically and militarily, one cannot help noting that never in world history did so few states involve in a conflict such a large share of the world's territory and population, all of it contiguous. The conflict might well have assumed a quite different character, more in line with that predicted by Harrison Salisbury in his book *War Between Russia and China*, had it not been that other geopolitical factors counteracted the impact of those al-

ready mentioned, specifically, had it not been for two factors: (1) the Sino-Indian and Sino-Soviet boundaries were able to protect sovereign "impenetrability" because of immutable physical geographical factors rather than from manipulable political and military might, and (2) the demographic, cultural, and economic centers of all three powers were asymmetrical in relation to the centers of national territory and located in opposite directions, far from the foci of territorial disputes.

The territorial contiguity of the protagonists, while emphasizing and acerbating the great power nature of the conflict, has unavoidably relegated to a distant plane any role which the small powers could play in it. With the sole exception of Mongolia, the small powers were peripherally located, on the other side of China and the Soviet Union respectively, and their role remained peripheral. The very location having precluded any pivotal geopolitical or geostrategical role for their territories, the small powers (or satellites of the communist world as they used to be called) would, perhaps, neither have been drawn, nor permitted to step, into the conflict at all had it not been for the ideological differences involved. Starting from the ideological plane the issues were then quickly extended to a host of political and economical questions.

Lévesque's study is the first systematic, competent book-length treatment of developments in some of the small communist countries as related to the Sino-Soviet conflict. Not surprisingly he chooses to deal with some of the so-called satellites from the western, not the eastern, end of the Eurasian land-mass under communist control. It may be a long time before a political scientist considers he has enough information and documentation to write an equally extensive study on the role of the Sino-Soviet conflict in the politics of North Korea or North Vietnam. It is also not surprising that Lévesque singles out for attention Poland and Romania, which are much more open to this kind of inquiry than, let us say, East Germany or Bulgaria; nor that he focuses on Poland in the period 1956-1959, the more or less "open" era of Polish politics, and on Romania in the period 1960-1968 when Bucharest became increasingly bold, admitting in private that it actually did manipulate the great power conflict. The subtitle of the book is a more accurate indication of the contents than the main title: The center of the stage is occupied by Poland and Romania, while the treatment of Eastern Europe as a whole is merely sketched in as

background information. One should add that throughout the book the emphasis is on the impact of the Sino-Soviet conflict on the politics of Eastern Europe. There is little about the reverse side of the medallion, and in this case again it was undoubtedly the relative availability of reliable information that was decisive. What impact the politics of the East European and Far Eastern communist countries may have had on Peking and Moscow may remain a secret to western scholars for a long time.

In the first part of the book the author deals with Poland and in the second with Romania, and he draws many interesting parallels and contrasts. He naturally emphasizes the role played by nationalism and quite correctly sees it as a constant factor which merely surfaces or submerges according to circumstances and the mood of the people. In the case of Romania, he points out that factional and personal struggles within the Party strongly influenced attitudes towards the Sino-Soviet conflict. In general, the author seems more at home when analyzing the Romanian than the Polish scene, but throughout he provides an informed, sometimes original analysis. He probably considerably exaggerates the impact of the Sino-Soviet conflict on Poland and Romania, even though he does not, like some analysts, ascribe most of the freedom of action gained by Romania, for instance, to a skillful exploitation of the clash among the rivals for leadership in the Communist World. In my opinion, the author has not sufficiently emphasized that by and large the Sino-Soviet conflict was not welcomed in Eastern Europe as a God-given opportunity for expanding elbow room; on the contrary, it was feared as a threat to the slowly expanding freedom during the era of *détente* between East and West. Tensions among the great powers of the world, whether belonging to different or similar ideological camps, were invariably accompanied by (1) Soviet attempts, mostly successful, to tighten their grip over the actions of the governments and parties of the East European countries and, (2) a tightening of these governments' and parties' grip over the domestic affairs and the freedom of action of individual citizens.

On the debit side of Lévesque's book one must note a surprisingly meager bibliography. This may be due to the author's lack of command of either Polish or Romanian. He does claim, however, to read Russian, yet not a single Soviet book or article is listed in his bibliography. One must also note the absence of a subject index; there is a name index, but what good is a name with a string of as many as

sixty or seventy page references listed after it, without further indication of the specific subject matter to which they refer?

LADIS K. D. KRISTOF

Portland State University

Colonial Rule and Political Development in Tanzania. By Gus J. Liebenow. (Evanston: Northwestern University Press, 1971. Pp. 360. \$11.50.)

Political scientists scarcely began to take an interest in Africa until the colonial era was almost over. They witnessed, recorded, and analyzed the final development of African nationalistic opposition to colonial regimes and the emergence of the new states. Thereafter, that is during the nineteen-sixties, political scientists have very largely concerned themselves with matters and problems relating to centralized, state government: new constitutions, political parties, bureaucracies, dictatorship, political coups, the role of the military or the trade unions, relations with East and West, African socialism, the politics and administration of development, and so on. All this has, of course, been both necessary and useful, even if somewhat lacking in theoretical sophistication and too much focused on political historiography. For each Apter there have been a score or more of fairly worthy journeymen. There has been a general recognition of the problems of ethnic disunity, regional diversity, and weak or nonexistent national identity, in these artificially created, new states; and from the central viewpoint a few valuable analyses of these factors have emerged. But we know really rather little of what has been happening at the local level and the regional level, and what is happening now in the first decade of independence of most of the African states. How far have indigenous political institutions, roles, values, survived? How, if at all, have they affected modernization processes in the twentieth century? How have other cultural factors, economic development, local chauvinism, affected political development? How have local areas, or distinctive regions, fitted into the new centralized regimes? Who have been the individuals, and what their roles, who have hindered or facilitated political change and efficient administration? Who have benefited most, and whose vested interests have been trampled on?

These kinds of questions are important in any radically changing country, but they are especially crucial to African countries because of their internal diversity and the absence of common tradition and consensus. And the fact is

that we can scarcely do more than guess at the answers or make generalized, often unsubstantiated, assessments. Unfortunately the anthropologists, who might have been expected to provide some of the data and answers, have been altogether too concerned with reconstructing and analyzing "traditional" political systems of local African societies.

Dr. Liebenow deliberately concentrates in this new volume on one ethnic group in southern Tanzania: the Makonde, about 300,000 people, and the third largest ethnic group in that country. In essence he has written a political history of the Makonde from the late nineteenth century to 1968. The descriptive content is marked, but throughout the author has attempted serious analysis and interpretation. He has accomplished something which we need for many, many parts of diverse Africa, but the like of which exists for only one or two local ethnic groups.

Dr. Liebenow rightly begins with an account of precolonial political organization among the noncentralized Makonde, with few specialized political institutions and roles. Perhaps with anthropological prejudice, this reviewer finds this part of the book least effective and comprehensive, verging sometimes on the superficial. And this defect dogs some of the later account where, it seems, insufficient account is taken of Makonde cultural bases as the people reacted and adapted to the successive phases of colonialism and of independent African government. It is clearly not enough, and not good enough, for example, to refer several times to "Makonde apathy" to innovation and opportunity. This shorthand expression hides more than it reveals; it evades some of the basic issues orienting Makonde behavior and defining issues and interests as Makonde perceived them.

The treatment of the early stages of colonialism, German and British, is much better, despite unavoidable scarcity of hard data; and the analysis of the introduction, failure, and replacement of "indirect rule" by the British is excellent. There is also a neat illustration of the "white man's madness" (a Makonde term, but one instantly recognizable by anyone who has experienced British colonial regimes) by which local policies, programs, and emphases, changed, often quite radically, with each of the frequent changes of colonial administrator. Dr. Liebenow has done the earlier and middle colonial period well, despite some inadequacies of material; although he tends to assume that the written records left by colonial administrators (in the District Book and elsewhere) are more

trustworthy than some of us would be prepared to accept.

The account of the late colonial period and on into independence starts admirably with the identification and description of five categories of Makonde: parochial traditionalists, tribal nationalists, accommodating modernists, dissenting modernists, and pivotal modernists. Like all such categories, they are not watertight ones; but their delineation is most valuable, and it would be interesting to see if comparable categories occurred elsewhere, and not only in Tanzania. Thereafter, Dr. Liebenow examines the gradual (really rather speedy) triumph of Julius Nyerere's nationalist party, and the early years of independence up to about the time of the Arusha Declaration. It is a difficult task to keep a balance between the focus on the local, the Makonde, whilst setting that in the wider national scene; and perhaps the author has not been invariably successful. One sympathizes with his problems, and as a pioneer in African micropolitical study he has succeeded in so much. It is to be hoped that he and others will follow up this study, learn from its undoubted merits as well as its deficiencies. One must applaud not only its intention and program, but also the generally high standard of its implementation. It is an important book, and just as much for those who could not care less what happened in this one corner of Africa.

P. H. GULLIVER

University of Calgary

The Politics of Formosan Nationalism. By Douglas Mendel. (Berkeley: University of California Press, 1970. Pp. 260. \$7.95.)

Professor Mendel's book invites two criticisms, one concerning its aim and the other its methods. As for his purpose, the author tries hard in the introduction and first three chapters ("The Growth of Formosan Nationalism," "Revulsion Against Early Nationalist Rule," and "Formosan Response to Cultural Integration") to establish his hypothesis that Taiwanese are not Chinese and Taiwan is not an integral part of China. Arguing that the overwhelming majority of Taiwanese abhor both the Nationalist and Communist regimes, Mendel consequently avers that Taiwanese separatism is justified and that the silent majority on the island would endorse secession even if it cannot say so now. After examining all evidence and facts cited by the author, any knowledgeable reader would find it impossible to accept the author's definitive but paralogical hypothesis. While Mendel contends that the legal status of Taiwan remains unsettled and that the

1952 Sino-Japanese Peace Treaty merely stressed Japanese renunciation of Taiwan and the Pescadores without naming a beneficiary state (pp. 174, 181-185), he fails to inform his readers of the legal significance of Article VI of the Sino-American Mutual Defense Treaty clarifying that "for the purposes of Articles II and V, the terms 'territorial' and 'territories' shall mean in respect of the Republic of China, Taiwan and the Pescadores . . ."; he also ignores Article IV in the 1952 Sino-Japanese Peace Treaty recognizing that "all treaties, conventions and agreements concluded before December 9, 1941, between China and Japan have been null and void as a consequence of the war." The latter provision will suffice to repudiate Mendel's contention that China has never extended sovereignty over Taiwan and clearly reaffirms a historical fact that Taiwan was taken away from China by Japan under the Sino-Japanese Shimonoseki Treaty of 1895, not from an independent Taiwanese regime. Besides, the author's justification of the Taiwanese secession movement rests mainly on a specious assumption: Formosans' resistance of cultural integration through "persistence of Japanese heritage" and constant use of local dialect.

To say that Taiwanese are not Chinese is as inaccurate as to say that Cantonese or Fukienese are not Chinese. It would be fatuous and impractical to insist on imposing on China the internationally approved independence of Taiwan. Any serious student of such matters would realize that neither the Chinese Government in Taiwan nor the Chinese Communist regime in Peking has the mandate or the sovereign power to preside over an international liquidation of China's territorial integrity. No one could realistically expect that any new Chinese regime could be dictated or goaded into accepting such an outlandish solution, even if one assumed that both the Nationalists and the Communists would give in or vanish. Mendel's support of the Taiwanese secession movement at the expense of China's territorial integrity cannot escape the charge that it is simply a replay of the defunct international scheme of partitioning China. Under the guise of nationalism, the author would equally not object to having Manchuria, Sinkiang, Tibet and Mongolia break away from China. Those who follow Professor Mendel's advice might ruefully find that such a course could lead eventually to a fratricidal war.

Furthermore, the author has mixed objective facts with value judgments. The lack of a conceptual framework for his research has meant

that the impressive quantity of his information does not lead to any credible or sound conclusions. My own reservations about the author's techniques of observation and method of analysis are threefold: (1) The omission of a bibliography is inexplicable, as is the exclusion of important documents. The sole appended document on "Declaration of Formosans" can hardly be considered objective or sufficient. (2) The selection of the categories of documents consulted seems arbitrary and defective. For example, evaluative categories covering the standpoints, values, and authorities are often based on the foreign information prejudicial to China's legal right to Taiwan. The author has used both indirect extensive observation and direct intensive observation. In the extensive observation Mendel has relied more heavily on the quota method than on the probability method of sampling, and has drawn too heavily on Japanese and American polls. (Out of the eight sources, four are Japanese polls and one is an American poll.) As for the probability method, his choice of the individuals interviewed is also subjective and his choice of strata is similarly unsystematic. For example, the author did not even bother to poll the Chinese residing in the United States, Japan, and other areas. Unfortunately the margin of error in these unrepresentative and limited interviews cannot be measured. (3) The author is not sufficiently meticulous about the statistics he cites. For example, says the author, "there were almost 500 junior high schools at the end of the Japanese occupation, nearly half of whose 16,000 students were Formosans and about 30 higher commercial and normal schools" (p. 48). But according to official Chinese statistics, there were only 41 secondary schools with a total of 29,005 students, and only 12,826 students were Taiwanese (see *Taiwan kuang-fu erh-shih-nien* by Taiwan Provincial Information Service, 1965, chapter 19, pp. 26-27.) Professor Mendel also said: "In 1963, only 13 per cent of Taiwan's total government revenues went for formal education, compared with about 50 per cent for defense . . ." (p. 48). While blaming the national government for spending less than 4 per cent of its budget for education, the author chooses to ignore the fact that the provincial government actually spent an average of 25 per cent of its budget for education annually in compliance with the minimum constitutional requirements.

Professor Mendel should be commended for his commitment to "freedom for all peoples," and his intention "to maintain a certain detachment" so that he can fulfil his duty as a politi-

cal scientist "to make his research available to the public as well as to governments and other scholars" (p. 4). Indeed, the book would have added some interesting polemics and informative articulation to the whole issue, if the author had exercised some academic continence by limiting his role as a research scholar. But unfortunately Professor Mendel changes his role from research scholar to revolutionary mentor, offering paternalistic advice for prodding the Formosan activists abroad to more militant, programmatic, and revolutionary actions to realize their goals (pp. 242-245). Regretfully, I believe his book performs a disservice to the betterment of Sino-American friendship.

RICHARD HSIUH YANG

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Public Opinion in Occupied Germany. Edited by Anna J. Merritt and Richard L. Merritt, with a Foreword by Frederick W. Williams. (Urbana: University of Illinois Press, 1970. Pp. xxiii, 328. \$7.95.)

According to Frederick Williams, who wrote the foreword to this book about German attitudes in the immediate postwar period, the military authorities in the American zone of occupation were

centrally concerned with change—change interpreted as the political maturing of the German people, an increase in their readiness to accept responsibility as individuals and as a great nation, a deepening in awareness of the nature of a free society, with its strengths and weaknesses, an improvement in the peoples' knowledge of the history of their own nation and the character of the tyrants they had supported (p. xx).

In order to detect the extent to which change was occurring, the Opinion Survey Section of the Office of Military Government, U.S. (OMGUS) was established and charged with the responsibility of examining German attitudes on a wide variety of issues and problems. For the most part, this book is a summary of the 194 reports issued by this office in the period between 1945 and 1949.

The OMGUS surveys summarized in this book are based on two kinds of samples. On the one hand, there were samples of the entire German population living in the American zone of occupation (AMZON). Overall, these were probably quite representative since they were based on the list of rationcard holders. Initially, these samples were rather small, but by December, 1945, the number of respondents had grown to 1,000 individuals, and by April, 1947, some 3,000 Germans were being questioned by the OMGUS interviewers. On the

other hand, there were also samples of specialized segments of the AMZON population. Included in the book are reports based on samples of German elites, German youth, former prisoners of war who had attended a course on democracy at Fort Getty, the residents of Vienna, and 188 people interviewed on the streets of Berlin.

Some of the information provided by the OMGUS reports, such as that dealing with German attitudes toward daylight saving time, will be of limited interest to political scientists. But the abundant data on the political attitudes of Germans and their attitudes towards the occupation period should be of considerable interest to German scholars. Fortunately, the reader interested in gaining an overall impression of the state of German opinion in the immediate postwar period will not have to peruse each of the 194 summaries. The Merritts have provided a balanced introduction in which they summarize the major trends in the reports. The first section of this introduction deals with the attitudes of Germans toward "Immediate Occupation Policies." Among the topics covered in this section are attitudes towards the occupation period, American forces, reconstruction, food allotments, expellees, the licensed press and other media, the economic situation and currency reform, and the division of Germany. In the second section of the introduction, the authors examine the data on "Democratizing Postwar Germany." Here they point out that while few Germans had read *Mein Kampf*, while few trusted Hitler up to the end of the war, and while most were satisfied with the outcome of the Nuremberg trials, nevertheless "large numbers of postwar Germans in the areas under American control continued to express perceptions characteristic of National Socialist ideology" (p. 31). They also note that although the principle of denazification was endorsed by the AMZON population, the implementation of the principle was not. With reference to political participation, the authors report that it was never high during the occupation and that it remained at a relatively constant level throughout the entire period. This latter finding, they suggest, "raises serious question about the overall effectiveness of the democratization program pursued by American occupation authorities—at least in terms of its measureable effects" (p. 50).

The final section of the introduction, "From Democratization to Anticommunism: The Reorientation of American Policy," is by far the most interesting. Here the authors argue that though the conditions were present, the allies did not bring about substantial cultural change

during the postwar period. The Merritts contend that the reason the allies failed in this regard is that they allowed the Cold War to get in the way of the goals they were trying to accomplish in occupied Germany. American policy, according to the Merritts, changed in the latter years of the occupation: "It became less interested in creating a new German society than in establishing Germany as a bulwark against communism" (p. 52). This change had serious consequences for the goal of cultural change in postwar Germany. "Promised education for democracy, the Germans ended up getting pushed off on another ideological crusade. . . . Promised new democratic leadership, they got a reentrenchment of leaders from the discredited Weimar period, together with those too clever or lucky to avoid entanglement in the webs of either Nazism or denazification" (pp. 57-8).

This book is unlikely to be widely used in the undergraduate classroom, but it will probably become an important reference manual for commentators interested in changes in German public opinion in the postwar period. Using the OMGUS surveys as a starting point, one can, with the help of the large body of public opinion data compiled by such commercial firms as Allensbach and EMNID, trace the development of German attitudes toward a variety of topics and issues across a period of more than two decades. It should be noted, however, that the OMGUS surveys are confined, for the most part, to Germans living in the American zone of occupation, and that the punched card data are no longer available. Thus, individuals interested in the attitudes of Germans in the immediate postwar period will either have to be satisfied with the summaries provided in this book or will have to wade through the 2,000 pages of the original reports available through the United States National Archives and Records Service.

This book does not contain a great deal of penetrating or controversial analysis. Indeed, it seems unlikely that anyone would sit down and read it from cover to cover unless there were some compelling purpose—such as being asked to review it! Yet, the book contains much useful information, and those interested in understanding the postwar development of German political attitudes will find it valuable. For reading the reports and compiling the summaries, therefore, the Merritts are to be congratulated.

KENDALL L. BAKER

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Barrios in Arms: Revolution in Santo Domingo.

By José Moreno. (Pittsburgh: University of Pittsburgh Press, 1970. Pp. 226. \$8.95.)

Vietnam and Santo Domingo; Santo Domingo and Vietnam!—the two great U.S. interventions and foreign policy crises of the mid-1960s continue still to haunt us. Obviously the far larger war and quagmire of Southeast Asia have overshadowed the smaller and now out-of-the-headlines conflict in the Dominican Republic; but one may well argue that it was the Dominican revolution and the U.S. response to it that undermined the confidence of opinion leaders in this country toward Lyndon Johnson, that presaged the crisis of credibility that pervasively plagues us, that destroyed forever the position and posture of the U.S. as a peacekeeping nation dedicated abroad as well as at home to the expansion of democracy and social justice, and that helped usher in the decline in our faith and sense of meaning and national purpose. Moreover, even though it now seldom makes the front pages, Santo Domingo—since the aborted social revolution that the U.S. snuffed out—has continued to disintegrate into virtually perpetual conflict and violence; it testifies to an incompleting revolution that has left a bitter legacy of hate, fear, bloodshed, and simmering anti-Americanism, as well as a political system that cannot cope with the uncontrollable forces pushing it toward breakdown let alone begin to solve pressing national problems—and much of the responsibility must be laid at the doorstep of the U.S. and its ill-conceived policies.

Initially our miscomprehension of the Dominican revolt was augmented by the coverage it received—journalistic, largely sensationalist and ill-informed, rabidly Marxist and economic determinist from one point of view, properly liberal and indignant from another, and a third perspective officially sanctioned or commissioned by the Johnson administration. Now, however, with Jerome Slater's 1970 book and Abraham Lowenthal's forthcoming study, the Dominican revolution and the intervention are beginning to receive the detailed, scholarly treatment they merit. To this distinguished group we must now add José Moreno's *Barrios in Arms*.

José Moreno was a Cuban-born Jesuit priest doing doctoral dissertation research in Santo Domingo on structural anomie in formal organizations at the time the revolt broke out on April 24, 1965. Immediately caught up in the revolution and generally sympathetic to its broad goals, Moreno found the original project impossible to research and soon dropped it. During the first days and weeks of the revolution he helped organize food dispensaries and medical services within the rebel zone and became a participant observer of the revolution-

ary process. Later, realizing that he was the only trained social scientist in the constitutionalist area (as well as virtually the only priest), Moreno began to study the revolution more systematically, drawing upon his earlier observations as well as the published sources, various private documents, and extensive depth interviews. The result is a book which, as would be expected given the circumstances, is sometimes uneven and ambiguous methodologically, but utterly fascinating as the first and probably only inside view by a professional sociologist of the dynamics of the revolutionary movement.

Moreno is careful to catalogue the mixed motives of many of the revolution's leaders, the diversity of leadership after some of the democratic-left elements took refuge in foreign embassies, the differing ideological perspectives and private ambitions that governed the revolution's participants. He also describes more explicitly than anyone has previously the important role the several Communist groups *did* play in the revolution (as distinct from the U.S. Embassy's infamous "lists" of 55 or 76 Communists that were demolished by James Goodsell's investigative reporting). But what Moreno also makes abundantly clear is the widespread opposition on the part of the Dominicans to the ousted Reid Cabral regime, the desire of the general population to cleanse the country of Trujilloism, of military corruption and oppression, and of widespread in-group favoritism and profiteering, and in so doing to establish a new order based on principles of democracy and social justice. Even with the qualifications that Moreno registers, the inescapable conclusion is that the revolution and its leadership were social-democratic in aims and aspirations, that the Communist influence was never dominant (and could not have become so), and that in essence the revolution represented a genuinely popular uprising of the poor and downtrodden against a corrupt military-oligarchic system that oppressed them and failed to recognize their human dignity. At the same time, the revolt is revealed as a movement with fairly limited goals: to use circumscribed and carefully structured violence to establish the right and legitimacy of the pro-Bosch forces to participate in the national life from which they had been excluded, that is, to secure the place in the system which democratically they were entitled to but without destroying totally the system *per se*. Misunderstanding this, badly informed, naively and mistakenly seeing in Santo Domingo a repetition of the Cuban experience and blind to other considerations save the "second Cuba

complex," the U.S. over-reacted, bottled up and frustrated the revolutionary movement, bolstered the efforts of a discredited and defeated military and oligarchy to regroup, rearm, and re-enrich themselves, and thereby set in motion the destructive, disintegrative processes in which both we and the Dominicans are now caught.

There are many fascinating glimpses as well as considerable imaginative analyses within the Moreno book. His investigation bears out that the democratic-left PRD (the Bosch, constitutionalist party) and its military supporters always remained the dominant force within the revolution, even though the realities of the situation and of their ideology obligated them to consult democratically with other groups. Moreno's treatment of the ideological currents within the revolution, of the emergence of leadership in a revolutionary setting, and of the forces allied on both sides of what became both an intervention and a civil war shows solid scholarship and original treatment. His division of the various groups participating in the revolution into political organizations, paramilitary organizations, and civilian organizations provides a new and intriguing perspective, and his analysis of the latter two is about the only detailed account we have of these groups and their activities.

There are also some flaws in Moreno's analysis. His treatment of the internal dynamics and sociology of the revolutionary movement is solid and informative, but his comments on the international aspects are sometimes weak and incomplete. That, however, is not Moreno's field and in any case is superbly treated in the Slater and Lowenthal studies. Occasionally also the book reads like the resuscitated dissertation at Cornell that it eventually became. And like many sociologists, Moreno sometimes oversimplifies the political complexities and undercurrents he describes. But on balance his remains an immensely worthwhile account, a very nice complement to the literature already available, and a fascinating glimpse into one of the most important but now largely neglected upheavals of our time.

HOWARD J. WIARDA

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The Red and the White: Report from a French Village. By Edgar Morin. Translated by A. M. Sheridan-Smith. (New York: Pantheon Books, 1970. Pp. 263. \$7.95.)

Ever since Tocqueville's nineteenth-century recollections of his rural constituency and Hal-

évy's reminiscences of his retreat in the Massif Central, village France has had an irresistible literary appeal to urbanites, French and foreign alike. In contrast to the popular French tradition that regarded the countryside as idyllic, if not bucolic, these writers have stressed the complexity of life in rural France and the great sophistication of the peasant. But paradoxically (and rural France is rich in paradox) the more specialized the study, and the more advanced methodologically, the more disappointing the results. Indeed, all has been downhill since Lawrence Wylie's classic *Village in the Vaucluse*, the least scientific and most profound French village study, appeared in the 1950s.

With the publication in 1967 of Edgar Morin's *Commune en France* and its translation as *Red and the White: Report from a French Village*, the corner has been turned. A specialized attention to both detail and essence, a combination of historical, economic, political and anthropological perspectives, and a prose style that even translation does not diminish, enable Morin to engage and hold the reader's attention through two-hundred and fifty pages of description of what life is like in *Plodémet*, a rural township far out on the Breton coast.

But can a case study of *one* village take us very far in arriving at valid generalizations—the real basis of all scientific work? The answer is yes, and not because *Plodémet* is presented as “typical” of all French villages, which it is not. First, while Morin's *genre* is descriptive narrative, the hard skeleton of the study is analytical. It is a book about the roots and the consequences of change in the rural periphery of advanced western societies in the 1960s. In its concentration on the itinerary of change, the study transcends the case study format. Its methods are well adapted to its aims, although the total balance of the interpretations would have been improved by a comparative perspective. For example, Morin shows no knowledge of such classic studies as Vidich and Bensman's *Small Town in Mass Society*.

The reader may well ask if the village case study does not inevitably suffer from the inability of the lone sociologist to “get deep inside” the village community in all its complexity. In this case, several years of data collection and analysis by an assorted team of researchers from several disciplines preceded the monograph. Morin drew fully upon their research in the preparation of this book, and this contributes to the richness of the presentation, if not to its theoretical coherence. For example, while Wylie could only look into the municipal rec-

ords of his Provençal village, the historical reconstruction of *Plodémet* and its political development cannot be matched.

What pulls the study together? Morin makes much of an approach he calls “the sociology of the present.” This seems to amount to a preference for participant-observation, and is in no sense a full-blown theory or methodology. The somewhat pretentious appendix devoted to this approach can therefore be ignored, and the theory-seeking reader must plunge into the narrative itself to divine the author's epistemology. Here he will find not only one implicit model, but two: Morin was trained as a Marxist-Leninist, and this shows in the criteria he used in selecting his material, particularly in the historical discussion. But he is also a gifted student of culture and has been deeply influenced by the French school of cultural anthropology. Thus, he emphasizes language, mass communications and religion, as independent forces of social change, as well as the push and pull of market forces, the control of the means of production and the clash between bourgeoisie and proletariat.

On the whole, Morin's dual perspective on social change enriches the analysis. Still, the combination of Marxist and cultural anthropological perspectives often produces odd results. For a Marxist to talk of the “personality” of a village practically in the same breath as he writes of “the dynamic forces of history” has an awkward ring. More important, neither approach gives much independent importance to local politics, and we see the local political elite mainly at the receiving end of two lines of change: the economic forces that have transformed the peasantry and the town dwellers, and the cultural changes that have produced a wide gap between the generations. The subtle interplay between local and national French politics, as seen particularly in the dual role of the small-town mayor, receives very little attention in the book.

I must say, in retrospect, that the best chapters are not those which, like “The Emerging Bourgeoisie,” bring out Morin's Marxist background, but are those which, like “Women, the Secret Agents of Modernity,” and “The Young and the Old,” stress most vigorously the cultural aspects of change. (Parenthetically, Morin does much for the cause of Women's Liberation by showing that change in *Plodémet* is chiefly inspired by women.) He is best in writing about human beings under the stress of changes they only partly understand. His book suggests that towns like *Plodémet* will never be “small towns

in mass society," if only because they are small towns in an *old* society, a society with the cultural resources to traditionalize change and modernize tradition.

SIDNEY TARROW

Cornell University

Africa Must Unite. By Kwame Nkrumah. (New York: International Publishers, 1970. Pp. xvii, 229. \$6.95.)

Kwame Nkrumah has reissued his 1963 call for continental unity unchanged beyond a short author's note to the new edition. In this, he points to the wave of military coups d'état and the increased imperialist and neo-imperialist activity in Africa as proof, in effect, of the truth of his original thesis. Nkrumah's main point is the need for political unity if the African states are to achieve significant and sustained economic growth. His sometimes companion theme—that it is necessary to achieve more than *de jure* political sovereignty—is muted in this book. This is probably fortunate, for his economic arguments are more persuasive without the strident political polemic.

Because the work stands unchanged, we must evaluate it from the perspective of events in Ghana specifically and Africa generally since he wrote it in 1962. Nkrumah was above all an advocate of continental unity and gracefully saw his own stronger draft charter for the O.A.U. bypassed in favor of the diluted, but widely acceptable, version. His vision of a united Africa, and his campaign for that goal, were certainly important in the creation of the O.A.U.

One is surprised that he did not take advantage of his enforced leisure since the February 1966 coup d'état in Ghana to revise much of the book. The first several chapters, which defend C.P.P. policies in the early 1960s, must be somewhat embarrassing to their author in light of the revelations and commissions of inquiry following his deposition. The paragraph on p. 53, for example, on C.P.P. "guidance and support" to liberation movements is, to say the least, whimsical after the discovery of camps to train opposition groups from other independent African states in the arts of violent seizure of power. Similarly, although his chapter on reconstruction and development is academically sound, the injunction to avoid inflation is one that the author did not take note of in his own management of Ghana's economy. In the same vein, Nkrumah's recommendation that small-scale personal enterprise be reserved to Ghanaians is one that awaited implementation from the post-1969 Busia government rather than the

author's "Building socialism" regime. One point in his internal policy does remain valid, however. His exposition of the virtues of unitary versus federal systems in the potentially fragmented state seems vindicated by the eventual violent revisions of the two major federal systems, Uganda and Nigeria.

While dropping much of the material on internal domestic politics would have strengthened the re-issued book, the portion on unity and development would have profited from expansion and updating. Nkrumah could have overwhelmed the reader with statistics showing that national growth rates barely matched population increases, or showing the continuing instability in the price of agricultural export items or the still unequal bargaining abilities of 'developed' versus 'underdeveloped' nations. A better appraisal of the advantages accruing to African States from affiliation with the European Economic Community would now be possible, had Nkrumah chosen to revise. So, too, would an evaluation of the myriad regional economic cooperative groupings that have come and gone. The East African Common Services Organization has struggled through the 'sixties, but its metamorphosis into a Common Market arrangement has meant a decline in areas of cooperation. Nkrumah's Ghana was one of the first to establish a national (often nonconvertible) currency and others have followed. The likelihood of establishing a common currency, a step strongly urged by the author in 1963, seems more remote than ever. Some halting steps have been taken to improve intracontinental communications—microwave stations now permit direct telecommunication links between some African capitals rather than via Europe, and the proposed East-West road system is moving beyond the talking stage. Nonetheless, the overall economic picture is still bleak.

I particularly regret that Nkrumah did not present his current thoughts on the O.A.U. As a first move toward continental political coordination, if not unity, it was viewed as a significant step in the right direction. Although the O.A.U. has had some successes, its overall record is checkered. The South African regime is more solidly in control than ever (Nkrumah's hopes for a revolution there, pp. 14-15, have not been borne out in the decade since Sharpsville); the regime in Southern Rhodesia has just been legitimated by the United Kingdom; and the liberation movements in Portuguese Africa have achieved significant success only in Guinea Bissau. Nkrumah's own prototype for unity, the Union of African States (Ghana, Guinea, and

temporarily Mali), which was to provide the nucleus of a future United States of Africa, has fared rather worse. Its only real accomplishment was to provide a legitimate haven for Nkrumah after the coup d'état. Though Touré greeted him then as "fellow president" of Guinea, Touré has shown no inclination to take advantage of Nkrumah's political expertise in the governing of Guinea.

ROBERTA E. MAPP

The University of Alberta

Chilean Politics, 1920-1931: The Honorable Mission of the Armed Forces. By Frederick M. Nunn. (Albuquerque: University of New Mexico Press, 1970. Pp. 219. \$10.00.)

This is essentially a history book, and it is problematical whether we should apply modern political science standards to an evaluation of it. It is a detailed chronicling of the role of the Chilean military, particularly the army, in Chilean politics during the most politically unstable decade the country has experienced in this century.

Briefly told, the "Honorable Mission" of the Chilean armed forces was "to set right Chilean politics and to assure governmental responsibility and action in the social and economic fields" (p. 3). In Nunn's view,

military deliberation of political matters became necessary in the 1920s because civilian leaders proved unable (in some cases unwilling) to carry on the affairs of state; social conditions had become acute. . . . The military was the only organized group in Chile capable of carrying out such action. No civilian interest group, faction, or party was capable (p. 5).

The "Honorable Mission" of the armed forces, therefore, was to purge the Chilean political system of the vices of excessive parliamentarism. The economic stagnation and social malaise of the post-World War I era were laid at the door of civilian politicians and the structure of parliamentary democracy. In Nunn's view, "action by military men put an end to the political stalemate in Chile" (p. 27).

The latter generalization is highly questionable, but this should not obscure the importance of these recurring themes in the study of twentieth-century Chilean politics. The second administration of Carlos Ibanez (1952-58), the Christian Democratic administration of Eduardo Frei, and the recent triumph of Salvador Allende's Popular Unity coalition all have this much in common: They, too, attributed the frustration of Chilean economic and social aspirations to root political causes and they, too, were concerned with altering a situation

perceived as one of "stalemate." But Nunn's study of military intervention in the decade of the 1920s is not set in the context of such important theoretical questions concerning Chilean politics.

Students of civil-military relations in developing countries are unlikely to find much material of cross-national relevance in this study. Occasionally, interesting observations are made which bear directly on some of the major topics in the literature of civil-military relations, but again, these are not followed by systematic elaboration. On page 9, for example, Nunn argues that "military professionalism and civilian parliamentarism produced an antagonistic civil-military relationship unheard of in Chile since the 1820s," thereby implying that the growth of a professional military establishment in the late nineteenth century contributed to the military's overt intervention in the civilian political process. Occasional reference is made to "corporate self-interest" as a possible cause of such intervention. In terms of the class bases of the officer corps, we are told that "in the 1920s the overwhelming majority of junior army officers came from a middle class, often provincial, background; most of the generals, however, identified with Chilean conservatism through family ties or political connections" (p. 55). As a summary of the causes of military intervention, we have the following statement:

The aftermath of the Civil War of 1891—army dissatisfaction with a corrupt, unresponsive government and its supposed favoritism toward the navy, increasingly serious social and economic conditions and mounting desire for a return to executive leadership, and the intra-army schisms—motivated the armed forces politically and brought civil-military relations to the boiling point by 1920 (pp. 11-12).

In the concluding chapter, "The Mission Ends," Nunn offers a few suggestions about those factors which contributed to a "return to the barracks." But on all of these matters—military professionalism and its relationship to military intervention, the bankruptcy of the civilian political process, the military's conception of its own institutional role, the class bases of the officer corps, the relationship of "social and economic conditions" to military intervention, the factors making for an end to overt military involvement in politics—Nunn says little which illuminates the theories of Finer, Huntington, and others about civil-military relations generally or the theories of Johnson, Lieuwen, and others with regard to such relations in Latin America.

A perhaps justifiable reply to this line of crit-

icism would be that to do all these things was not the book's intent. If the book is seen simply as an effort to detail events involving the Chilean military in the decade under consideration and is judged solely on those grounds, then we might come to different conclusions about its usefulness. It is a competent piecing together of the major historical events of a chaotic era in modern Chilean politics. Anyone who has studied Chilean politics knows that the task of "mere description" is no easy task; such study is akin to putting in place the pieces of an elaborate puzzle. Nunn has surveyed an impressive array of historical materials in an effort to piece together an important period in Chilean political history, a period which has had deplorably little coverage. For this reason the book is likely to be widely read by confirmed Chileanists. Its useful bibliography on the period will also enhance its utility for students of Chilean politics.

As a description of a particular period in Chilean political history, then, the book is a competent if somewhat dull undertaking. As an analysis of the events described, it is less than convincing. Specialists on Chilean politics will probably find themselves turning to it for essential historical information, but scholars with a wider comparative or theoretical orientation are unlikely to find much that would make it required reading.

ROBERT L. AYRES

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Words and Occasions. By Lester B. Pearson. (Toronto: University of Toronto Press; Cambridge: Harvard University Press, 1970. Pp. 296. \$10.00.)

It is academic blasphemy to suggest that it could take forty years to rise from Lecturer to full Professor in the hallowed halls of Canadian universities. Yet such was the case with Professor Pearson but with exculpation: by his own admission "... I had taken a long non-academic detour (where tenure was at times very dubious) before achieving this promotion," an ambition he harbored as an academic neophyte. Pearson's detour is described in this four-part anthology of his speeches, transcripts, and photographs as an External Affairs department official; Secretary of State for External Affairs; Liberal party and official Opposition leader; and, finally, as Canada's fourteenth Prime Minister. His career belies the cliché "Nice guys finish last."

The book provides the reader with glimpses of urbane self-mockery and other instances of warm humor through selections which an ego-

centric statesman would not have included—selections such as his maiden speech in the House of Commons and an interview which noted one of the scandals affecting his government. Although the author alludes to some of his many kudos, his sense of humility keeps him from citing the highest award he achieved, the Nobel Peace Prize. His efforts in the formation of NATO and the delight he expressed to some of his Algoma East constituents was overshadowed by one old-timer's basic political advice (which he quotes): "Well, that was a fine thing you did down there in Washington, Mr. Pearson, signing that treaty, but it won't help you much around here if you don't give us a new post office."

Pearson's entry into the political arena from the top as Secretary of State for External Affairs was compounded by his lack of a "grass-roots" upbringing: a sharp contrast to that of his indefatigable and indomitable opponent, John Diefenbaker. Even though he was a diplomatic artisan *par excellence*, Pearson did not have either sufficient political adroitness or Mackenzie King's "killer instinct" to prevent the scandals which became one of the hallmarks of his stay in power—a stay which rested on a plurality but never a majority of seats in the Commons. In this regard his civil service background was a handicap: "... I confess that I couldn't lose the habit of seeing more than one side of a question. A party leader, should only recognize two colours—white, his; and black, the opposition's" (p. 119). At the 1967 annual banquet of the Canadian Political Science Association, he cited his former Professor's definition of politics as "the skilful use of blunt instruments." Many political scientists and pundits wondered whether the Prime Minister had paid more than lip-service to this definition in view of his government's scandals and other blunders. In this same speech he was candid enough to state that he had "not escaped without some bruised political muscles and even a drop or two of spilled political blood." Notwithstanding bruises and blood, the Pearson government achieved measured progress in social welfare, federal-provincial relations, unification of the armed forces, bilingualism, and a distinct Canadian flag.

Paradoxically, while Pearson lacked political adroitness, nonetheless he was endowed with abundant diplomatic acumen and a keen sense of history. He displayed a perspicacious view of the future when he publicly dealt with the destructive potential of developments in atomic energy before an Ottawa Church group in 1934: similarly his distaste for war and commit-

ment to internationalism led him to believe that "all states will have to abandon some of the attributes of sovereignty in the interest of larger associations of nations" (p. 34). As a protagonist for international action, Pearson's qualities made him a positive middleman at the UN. His efforts over the Palestine issue resulted in the establishment of UNSCOP and approval of its partition plan in 1947. On the brink of a major power cataclysm over Suez, Pearson's UNEF proposal on November 3rd, 1956, was the epitome of positive diplomacy designed to "face-save" the Franco-British intrusion, assuage an aggressive Soviet Union and a shocked and chagrined American government, achieve a cease-fire, and, for the first time in history lead to the creation of a truly international peace force responsible to the Secretary-General and General Assembly. This was a remarkable achievement which made him an outstanding winner of the Nobel laurel.

This book is also a useful guide to students of Canadian foreign policy because it addresses a number of issues in policy formulation during the Pearson years: geography and the North American milieu, the British Commonwealth of Nations, and the strength and resources of a middle power ("Canada and the San Francisco Conference"). In his April 1951 speech he commented on Canadian-American relations "... the days of relatively easy and automatic political relations with our neighbour are, I think, over" (p. 107). Canada's position in the world had become more important and the United States was now a superpower. Pearson was a champion of the "quiet diplomacy" precept which Canadian foreign policy practitioners retained despite rumblings of its unpopularity during the 'sixties. His continentalism is brought to the foreground in the controversial speech of January 12th, 1963, which called upon Canada to accept nuclear warheads for North American defense, something that the Kennedy administration clearly wanted.

Independence of thought coupled with responsible action is one of Pearson's strongest characteristics which undoubtedly accounted for his election to the UN General Assembly's presidency in 1952. Nevertheless, such independence did much to annoy Mackenzie King, as Volume IV of King's "Record" has recently revealed. In "Some Principles of Canadian Foreign Policy" (January, 1948), Pearson questioned the King government's timid position on the UN Korean Commission and asked why the King government, "after a somewhat adventurous beginning, backed well away from the implications of our earlier words in the

sorry and tragic Palestine business? ... Personally, I would have liked in this Palestine issue to have taken a strong independent stand for what we thought was the proper and right solution and to have dismissed the British and Americans ... " (p. 71). Dismissal of the British but not the Americans over the Palestine issue was inconceivable under Mackenzie King. Pearson did not regain independence of action until nine months later when he was appointed External Affairs minister and won a seat in the Commons, while Mackenzie King retired.

Metaphorically, *Words and Occasions* is the delectable *hors d'oeuvres* plate before the "Pearson Memoirs" main course. It whets the appetite on a number of important international issues which the Memoirs will elaborate upon in detail. His human touches provide the reader with an important dimension of the author's personality, not to be overlooked in future evaluations of the Pearson Years.

ZACHARIAH KAY

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Libyan Independence and the United Nations: A Case of Planned Decolonization. By Adrian Pelt. (New Haven and London: Yale University Press, 1970. [For the Carnegie Endowment for International Peace.] Pp. xxviii, 1016. \$35.00.)

Mr. Pelt's book is a comprehensive presentation of the role performed by the United Nations from 1949 to 1951 in the establishment of a sovereign and independent Libya following Italy's defeat in World War II. The reader gets a panoramic view of the functioning of the World Organization in a unique colonial situation, as painted by former UN Commissioner in Libya, Adrian Pelt. Not only does the author trace the circumstances in which the Big Four Powers, out of mutual disagreement on what to do with the former Italian colonies, turned to the General Assembly of the UN for a determination of the case. He also delves into: the historical background of Italy's colonization of Libya as of 1911 (explaining the evolution of the Libyan nationalist struggle against its western colonizer); the sectional orientation in Tripolitania, Cyrenaica, and Fezzan which delayed the emergence of a united Libya; the attitudes of UN Member States towards the various issues raised by the Libyan case; and finally, the emergence of the federal Libyan structure, under King Idris I, in 1951.

The author deals with his subject from the vantage points of having been involved personally in those historical events, of having access

to archives and other material not readily available to other researchers, and of having known or witnessed those who played various roles inside and outside the UN in relation to Libyan decolonization.

His references to confidential (later cleared for his purposes) papers, works translated from Arabic, personal papers (such as the Omar Loutfi papers in Cairo) and reviews and correspondence (such as those with Abdel-Rahman Azzam, the distinguished first Secretary-General of the League of Arab States), all add to the richness and variety of his bibliographical data.

Mr. Pelt's description of the endless UN compromises which led to the adoption of General Assembly resolutions 289 (IV) of 1949 and 387 (V) of 1950 illuminates the nature and functioning of international organizations. His analysis of the functional interaction between sources of authority and responsibility in pre-independent Libya is equally enlightening. In addition to the Commissioner himself, these factors were: the UN Council for Libya, created to advise the Commissioner on assisting the Libyans in working out their constitutional arrangements for an independent Libya; the United Kingdom and France in their capacity as administering Powers in the former Libyan territories; the National Constituent Assembly of Libya (NCAL) which opted on 2 December 1950 for a federal form of government; and the King-designate, Idris al-Sanussi. In clarifying the complex process of nation-building under the aegis of the United Nations, Pelt emerges as the outstanding advocate of "King" and "Federation" in Libya, even though these have both been discredited in Libya during the 1960s. His opponents remain, then and now, the forces that opposed "federation" and called for "unity" among Tripolitania, Cyrenaica and Fezzan, as a means of checking imperialist British and French influences.

Unfortunately, Pelt has no ideological contribution to offer in terms of the new positions adopted by the UN as of 1960. He recounts those experiences of the late 1940s and early '50s as if his thinking about the role of the UN in national liberation had completely frozen at the level of 1951. Nothing in the book links what the UN did in Libya in the early '50s with what the World Organization has been doing since 1960.

Not only does the author completely ignore evolutionary change in the relationship between the UN and colonial peoples, he also implies that the close supervision exercised by the UN in Libya is still a valid approach to nation

building in the 1970s. It is unrealistic to expect the present leaders of the African revolutionary movements, who have been accorded full recognition by the UN and the OAU as national spokesmen of quasi governments, to accept the UN's Libyan pattern of supervision and interference-by-advice in their constitutional development.

What seems to be paternalism on the author's part manifests itself in three other important respects. First, he distinguishes between what he calls "Libyan nationalism" and "Arab nationalism," as if they were two distinct and mutually antagonistic forces. The establishment in September 1971 of the Federation of Arab Republics, consisting of Egypt, Libya, and Syria, illustrates the fallacy of this distinction. Second, he regards the Western ideal of democratic government, with constitutional checks and balances, to be the only acceptable form of rule for newly emerging nations. Third, he looks upon the Libyan leadership and people of that period as wards in need of his helping hand. In fact, he states that it would have been preferable to delay independence by one year to allow the Libyans "a better chance of assimilating the new ideas, of studying the problems . . . etc." Pelt's inability to distinguish between the popular urge for freedom in colonial areas and the technicalities of framing constitutions and laws is evident throughout his work.

It is not therefore surprising that Mr. Pelt, as the Commissioner, had found it "difficult . . . not to overstep the mark between help and pressure." Such paternalistic attitudes towards what the author calls "young" countries ignore the historic realities of Libya's long armed struggle against Italian imperialism, then against Italian fascism. The Libyan war of national liberation, the first African war of this kind in the 20th century, has done a great deal to sharpen Libya's sense of its nationhood over a period of 30 years of conflict. The author's insensitivity toward the impact and meaning of that struggle is illustrated by his disparaging comments on the formation of the National Council for the Liberation of Libya (NCLL) under Bashir Saadawi in 1947.

The book's orientation toward what should be described as an outdated Western approach to the Third World is also apparent in the author's comments on his sources. Pelt constantly throws doubt on his non-Western sources of information, without indicating the reason for his doubts. For example, he relies heavily, in the "historical background" part on the work of

the Libyan scholar Al-Tahir Ahmed al-Zawi's *Jihad al-Abtal*. Nevertheless, he prefaces several of his quotations by statements such as "if al-Zawi's version of the events is to be believed." In questioning the credibility of his non-Western sources, the author, by the same token, undermines the credibility of important sections of his work.

In summary, within the covers of this 1000-page study, the reader finds a very lengthy case history of decolonization under the UN in Libya, which abounds in source material for future analysis, but misses the relevance of the study to the issues of decolonization today and to the UN's role in them.

YASSIN EL-AYOUTY

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Modernization and Diversity in Soviet Education. By Jaan Pennar, Ivan I. Bakalo, and George A. F. Bereday. (New York: Frederick A. Praeger, 1971. Pp. 395. \$20.00.)

Soviet modernization has long interested western social scientists, and the present work reflects a continuation of that interest. As the title suggests, the authors have specifically looked at the interactions between Soviet education and Soviet modernization. They demonstrate how the changing needs of Soviet economic development have affected the content and structure of the public school system. Conversely, they show how the schools prepare the younger generation for different labor roles and how the educational system has helped shape and sustain rapid economic growth. In analyzing these interdependent phenomena, the authors have assembled and evaluated a large amount of primary data. But most of these data have been analyzed before by such scholars as DeWitt (*Education and Professional Employment in the USSR*) and Osborn (*Soviet Social Policies*). Consequently, the book does not substantially add to or advance our knowledge of the subject.

A more serious flaw stems from the authors' exceedingly narrow treatment of the concept of modernization. They list four goal objects of Soviet educators. We are told that the public schools have been charged with the task of rearing a "new man, one that would be isolated from the decadent tendencies of the old society." A second goal is that of creating a collectivist value orientation so that the school graduate will ask not what his country can do for him, but what he can do for his country. Third, "The Soviet leaders are intent on creating a politically docile man characterized by joyful obe-

dience to the government." And finally, education must produce an industrial man "to prove that Communism is the quickest way to enrichment" (p. 98).

This reviewer would have added other goal objects which the state has assigned to the educational system. Whether there are four, five, or eight socialization goals, however, is not the main point. What is important is how the educational system functions to achieve these goals. Is the classroom experience structured in such a way as to foster the development of a collectivist as opposed to an individualist identity? How does the school curriculum generate political legitimacy and political support for the communist regime? What aspects of the educational program are designed to nurture the personality traits of the New Soviet man? How successful has the educational system been in realizing the goal objects identified by the authors?

These and many other questions have to be answered if we are to understand the correlations between Soviet education and Soviet modernization. Yet the reader finds very little information which might illuminate these issues. After presenting a multidimensional picture of the goals of Soviet educators, the authors choose to concentrate exclusively upon those educational aspects which relate directly to the training of industrial man. Any work which fails to discuss the political, cultural, and social psychological dimensions of modernization suffers not only from imbalance, but also distorts the very phenomenon it sets out to explain.

Along with its narrow focus upon labor training, the book is repetitious. Many events, such as the Educational Reform Act of 1958, are discussed time and again. With proper editing the work could have been shortened substantially without impairing its basic theme or value. As a reference source on Soviet education and economic growth the book has utility. But as an empirical study of comparative modernization, the book badly fails.

JOEL J. SCHWARTZ

University of North Carolina

The Politics of New States: A General Analysis with Case Studies from Eastern Asia. Roger Scott, ed. (George Allen and Unwin, 1970. Pp. 201. £2.10.)

This collective enterprise takes the form of a number of essays (completed early in 1969) designed to introduce Australian undergraduates to a growing field of academic enquiry. The blurb on the dust-jacket represents this

book as satisfying "the special need for a comprehensive course of study," but the author with becoming modesty views the endeavor as "a selective study." To this end a general introduction explaining the nature and problems of politics in new states is followed by a number of chapters whose prime unifying link is the relevance of the countries to Australian interests.

In many ways this is an unexceptionable book. The editor provides the reader with an elementary but useful survey of the principal characteristics of new states and deals competently in the process with the relevance of conventional concepts, the significance of precolonial influences, the experience of decolonization, the problems of political and national integration and the failure of postindependence political institutions. In all this, there is perhaps a tendency to overgeneralize from African experience—given the geographic orientation of the case studies—but in the main the introduction matches the terms of reference set by its author. What follows is no less unexceptionable though perhaps somewhat less useful for the student constituency for which this work is intended. Chapters on Japan and China examine the course of critical political change though in the case of the former state the survey ends in 1947. There follows a general inquiry into the position of the Overseas Chinese which poses the problem in terms of absorption, succeeded by two studies of social and political change in Vietnam and Indonesia in which the former chapter by Peter King is the more worthy synthesis. Finally, the relevance of African experience is applied to the political condition and prospects of Australian administered New Guinea. With the possible exception of this last chapter, it is doubtful if students will find anything in this collection which is not available in more original and stimulating form elsewhere. However, providing they are not so complacent as to conclude that all their intellectual needs in this field will be satisfied within this set of hard covers, this volume is not without merit as an introductory text.

MICHAEL LEIFER

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Colonialism in Africa 1870–1960. Volume 3: Profiles of Change: African Society and Colonial Rule. Edited by Victor Turner. (Cambridge: Cambridge University Press, 1971. Pp. viii, 455. \$17.50.)

This is the third of five volumes to appear in the survey of Colonialism in Africa 1870–1960 under the general editorship of

Peter Duignan and L. H. Gann. Unlike its two predecessors, in which historians concentrated on the aims and activities of the colonizing powers, the most recent volume focuses on Black Africa itself and the impact European colonialism made on various aspects of its societies and cultures. What is there for a political scientist in this collection written primarily by social anthropologists for the benefit of historians?

First, there are several important things he will *not* find in the book. He will not find a representative survey of what happened to different sorts of societies under different sorts of rule in different parts of Black Africa. While no single volume can provide ideal coverage, the concentration on English-ruled areas, particularly of East and South Africa, does seem unnecessarily heavy. Only one contributor deals specifically (and well) with French rule—some references by other writers to French practices suggest they are not on their home ground—and the Belgians and Portuguese get only passing mention. Neither will the political scientist find a set of ready-made and agreed upon categories for the analysis of externally imposed social change, nor a general overview of the colonial impact on just Africa. The editor, out of wisdom, discretion, or desperation, reports that he tore up his attempt at synthesizing his contributors' efforts and limited himself to a five-page, and oddly defensive, introduction.

The book's main interest for the political scientist lies in some excellent, primarily political case studies of what happened to particular societies under colonial rule; three synthesizing studies of subjects that have not received enough attention from political scientists dealing with Africa and one superb general essay by Max Gluckman that stands in a class by itself. In the case studies Ronald Cohen traces the effects of British rule on the Bornu Empire of Northern Nigeria; Martin Klein (the lone historian represented) shows how French suzerainty transformed the role of the chief in Senegal's Sine-Saloum area in the years before 1914; John Middleton and Peter Rigby each deal with the effects of colonial regimes' introduction of formal political chieftainships into small-scale East African societies (Lugbara and Ugogo) in which formal authority had previously been decentralized and closely linked to ritual-performing roles. The authors are on very familiar ground, each having previously written extensively about the society in question.

The most useful of the more general, synthesizing studies are those by Elizabeth Colson on how colonialism affected land rights (the

best short discussion of this much analyzed problem I have seen), Aidan Southall on imperialism and urban development (Michael Banton's related piece on urbanization and the color line is less successful), and F. B. Welbourn on African responses to missionary stimuli (a strong chapter which badly needs a companion dealing with the responses to Roman Catholic missions, particularly in West and Central Africa).

The reader of these contextually rich studies will draw many tentative generalizations of his own. This reviewer was particularly impressed with the repeated illustration of how European rule rigidified once flexible African social and political systems; for example, imperial rule reduced the ability of both the large-scale Bornu Empire and small-scale Ugogo to deal with human physical movement, particularly of herdsman—as the Europeans brought in a conception of order utterly dependent on a fixed relationship between geographical boundaries and political authority. The insistence on a symmetrical and hierarchical system of government frequently increased African political authorities' power in some of the least socially useful spheres by removing ritual and other checks and balances and indirectly encouraged them to extort larger amounts of wealth from their subjects, or at least to retain what they received for themselves. As Colson and Southall show, the most flexible and broadly progressive uses of rural and urban land came about in those areas where European influence was least felt. Are such rigidities in any way "necessary" aspects of the development of centralized rule? Are they peculiar to European imperialism, or might any outside intruders have produced such results? Studies of this sort cannot tell us, but they do call attention to the need for historically and culturally comparative work to attempt general answers.

A useful step in such a comparative direction is made by Max Gluckman's "Tribalism, Ruralism and Urbanism in South and Central Africa," a paper whose title belies the writer's intentions to sum up some of the principal political lessons of his many years' experience as an anthropologist in Africa. The essay brings together straightforward theory, evidence filtered through experience, and comparative data from other places and other times. In short, it does what is necessary to arrive at solid general conclusions. The clear statement of his terminology and the application of his familiar conflict and equilibrium model of social relations make it possible for others to pinpoint areas of disagreement on facts or interpretation and assess their relevance to the structure and conclusions

of the argument as a whole. The brief comparisons of contemporary African situations with pre-twentieth century European situations add a useful sense of perspective missing in the papers of other authors in the book. (Thus, Southall in particular might have recalled that, however much it helps, imperial conquest is hardly an historical requisite for the construction of appalling urban slums.) Gluckman's political conclusions, pointing to continued relative stability for South Africa and sterile instability for much of Black Africa, are likely to make most of his readers just as unhappy as they quite evidently make their author. The theoretical implications of his analysis, indicating that "we need to abandon the view that in these kinds of colonial societies the principal social factor was the forceful domination of self-interested alien minorities" (p. 162), will make some scholars and many ideologues even more unhappy. Let us hope that in attacking his position his critics show the same high standard of analysis that Gluckman displays.

WILLIAM J. FOLTZ

Yale University

Decolonization, the Administration and Future of the Colonies, 1919–1960. By Rudolf von Albertini. (Garden City, N. Y.: Doubleday & Company, Inc., 1971. Pp. 680. \$12.95.)

The author of this substantial volume sets forth as the subject of his inquiry a survey and analysis of the attitudes of European countries, and notably Britain and France, toward their colonies during the four decades following World War I. In these decades colonial administration had to be re-oriented, decolonization came to general acceptance, and new relationships had to be worked out between the colonial power and its former colonies. He is explicit in stating that he is not undertaking a contribution to the history of Asian and African states, which he sees as beyond his scope, but is examining an important aspect of European history. In consequence relatively little attention is given either to the actual administration of colonies and the changes therein over the decades or to the rise of nationalist movements. The material on which he has drawn consists primarily of official British and French pronouncements, of party programs, and of speeches and writings of influential political leaders, colonial administrators, and other commentators. It might be remarked that much of this material overflows into the abundant footnotes which occupy 113 pages.

Von Albertini sees decolonization as having been hastened by many factors, but finds its principal source to be the process of modern-

ization instigated by the colonial powers. "This process in turn was simply a part of current world technological revolution and the spiritual and social changes that accompany it. Colonial conquest itself stimulated the forces which would one day lead to emancipation" (p. 525).

He rightly warns, however, against any assumption that the colonial powers embarked at an early stage on a purposeful and ethical policy of decolonization. Indeed, one of the most interesting, and somewhat startling, features of the book is the repeated citation, from World War I up virtually to the abandonment of colonial rule, of authorities of all varieties who insist that independence is not presently fitting or tolerable. Whether they arrived at this conclusion because of a desire to cling to the fruits of empire or because of the conviction that the "natives" are not competent to run their own affairs must remain among the puzzles of the legacy of colonialism.

As it became increasingly evident that colonialism's days were drawing to an end, an effort was made in a number of quarters to evade or postpone the ultimate issue. For the British the tendency was to move with all deliberate speed to some in-between stage of self-government or partnership, while the French moved toward assimilation and integration but shied away from the drastic consequences of any full-scale implementation of them. Both countries also in their different fashions invoked the device of a Commonwealth or Union or Community which would have the look of equality for members, granting local control of local affairs, but continuing to vest the major concerns of sovereignty in the imperial center. In both instances the effort notoriously failed as the demand for full independence proved irresistible. The French Community dissolved almost totally as a formal institution although the ex-French colonies in Africa, save Guinea, rallied around de Gaulle and looked to Paris as part of their common heritage. As for Britain, von Albertini sees the solution pragmatically worked out by and for India as the turning point for the transformation of the British Commonwealth of Nations into a multiracial Commonwealth.

A central theme throughout the book is inevitably the familiar difference between the British and the French in their basic colonial philosophy, the French looking to assimilation through direct rule, the British to the development of distinct societies through indirect rule. Von Albertini is, of course, well aware that the distinction is far from absolute and that in the actual working out of colonial administration the two often responded in similar fashion to

similar challenges and situations; but this reviewer is persuaded, as the author appears to be, that there remains a fundamental difference in approach. In relation to decolonization, with freedom and equality replacing domination and super-imposition, von Albertini portrays the French as setting their goal in terms of the individual who finds his freedom within the state and society which embraces both the mother country and the former colonies, while the British set out from the separate communities of mankind and find freedom in the attainment of sovereignty and independence. Although the author abstains from openly taking sides, it seems reasonably evident that on the whole he finds the British position more acceptable although he has no illusions as to the shortcomings of indirect rule.

This book is based on a wide variety of sources, covers a large amount of ground, and maintains a high level of scholarly moderation and sensible interpretation. It is very useful to have such a book available because of the quantity and quality of the material it brings together between two covers on a topic of marked current interest, including four brief chapters on the colonial policy of the United States, the Netherlands, Belgium, and Portugal. This is not, however, to say that the book adds very much to the existing stock of knowledge in either interpretation or substantive material.

One further critical comment which must unhappily be made is that the book has been carelessly edited, letting errors slip in which cannot help but raise doubts as to the accuracy of other statements and citations. To indicate errors which caught this reviewer's eye: the terminology of mandates and trusteeships went sadly wrong; for example, the League's Permanent Mandates Commission had to present its reports through the League's (nonexistent) Advisory Council and "could reach government level only through this body" (p. 8); the UN Trusteeship Council, "made up of both mandated and non-mandated territories," sent out "special delegations" whose reports were submitted to the "special commission of the General Assembly"; the Committee on Information from Non-self-governing Territories "was formed to force the colonial powers to give political information and to fix 'timetables' for independence" (p. 24). Kaduna, Nigeria, becomes Kadvan (p. 221); the date of Algerian independence is wrongly given as 1960 (p. 335); "the legendary Van Vollenhoven" becomes even more legendary when he is made Governor-General of Indochina for 37 years (actually 1914-15) p. 338; and Taft reports

on the Philippines between 1900 and 1912 to President Theodore Roosevelt (p. 475).

RUPERT EMERSON

Harvard University

The Chinese Road to Socialism. By E. L. Wheelwright and Bruce McFarlane. (New York and London: Monthly Review Press, 1970. Pp. 256. \$7.50.)

This is a well-written book. The goal set by the authors is to assess the impact of the Cultural Revolution on China's economic policies, planning, technology, and production. The book consists of 12 chapters and is divided into three parts. It starts with a general survey of Chinese economic strategy up to 1965, containing facts which are already familiar. The second part traces the origin and the development of the Cultural Revolution and the impact of the Cultural Revolution on planning and financial policy, moral incentives as well as technological policy. In this part, the authors provide some interesting observations based on their separate tours to China. The concluding part summarizes the authors' view on Maoists' strategy of development and what they conceived as the Chinese "road to socialism."

The main argument is that Chinese economic performance cannot be judged by pure economic standards such as real productivity per man-hour or rate of return to the state on invested capital because noneconomic aims are being "fed" into the planning system. As the authors see it, the aim of Maoist socialism is not the maximization of economic growth but the building of a new society in which equitable distribution of income among various segments of the population, particularly the peasants and the working class in the hinterland, is more fundamental. They believe that "a genuine attempt is being made in China to construct a new morality, a new moral basis for building a socialist society, and on a scale unprecedented in the whole of human history" (p. 214).

Evolving from such a theme, the book is primarily devoted to the economic justification of every major act of the Maoists from the Great Leap and the commune to the Cultural Revolution. The Great Leap which has been condemned by most experts on the Chinese economy was hailed by the authors as a kind of "crash industrialization" program in the context of developing agrarian socialism without large-scale labor transfers to the cities. The development of small industries is viewed by the authors as a great success. They have quoted official statements issued during the heyday of the Great Leap to support this view and ignored the self-

criticism made after the tragic collapse of the Great Leap.

The commune is regarded by the authors as "a means of improving the division of labor in the agricultural sector" (p. 49) with "the advantages of large-scale cooperation for irrigation and canal building and for water conservation" (p. 49). To support their view, the authors believe there was a 30 per cent increase in food production in 1958, which implies 240 million-ton grain output in 1958. No one in the field of Chinese studies could possibly accept such a figure. In 1970, after nine consecutive bumper harvests, the official figure for food-grain production was only 240 million tons (based on Edgar Snow's interview with Chou En-lai, *The New York Times*, March 13, 1971), while the Chinese population went up more than 100 million during the same period.

As to the Cultural Revolution, the authors reject the concept of power struggle, or a clash of personalities. They see it as "revolt against the power system which industrialization engenders" (p. 28). The aim of the Cultural Revolution, they say, is to promote the human factor in economic development by breaking down the great gulf between the senior elite group of technocrats, managers, and party administrators on the one hand, and the mass of peasants and workers on the other (p. 15). Once again, the authors ignore the fact that "ideologists" such as Chen Po-ta who led the Cultural Revolution up until 1969 now share the same fate as Liu Shao-chi, whom the authors brand as a leading "rightist" in economic policy (p. 88).

There are a few minor errors in the book. On page 129, the Cultural Revolution Group of the Central Committee is said to have been "headed by Mao, Chiang Ching, Chou En-lai, Chen Ta [should be Chen Po-ta] and Kang Shen[g]." This is not correct. Mao as the chairman of the CCP has never headed the Cultural Revolution Group. Nor has Chou En-lai been a member of the Group. The Group was led by Chen Po-ta with Chiang Ching as his deputy and Kang Sheng as adviser. On page 32, the authors state that output per man and per acre in Chinese agriculture was very low. This is only partially true. Although output per man was low because of the extremely small per capita holding of land, yields per acre were by no means lower in China than in advanced countries. In the early 1930s, for instance, yields per acre of rice and wheat were higher in China than in the United States and the USSR. On page 170, the 12,000-ton hydraulic free-forging press is said to have been produced by the Shanghai Heavy Machine Building plant; it

was, in fact, made by the Kiangnan Shipyard, one of the oldest firms in China.

Despite this, the book is well organized, adequately documented, and very informative. It offers useful summaries of the developmental strategies during the past two decades and of the Cultural Revolution. It also provides much firsthand information on the micro level—information derived from the authors' brief tours to China in 1966 and 1968. Fragmentary as they are, the data give some insight into the operation of the commune system and the struggle between the revisionist and the revolutionary lines in the factory level before and during the Cultural Revolution. A valuable appendix consists of a collection of ninety-nine "latest instructions" issued by Mao Tse-tung between August 1966 and July 1969. The instructions not only may serve as a gauge to Mao's influence on internal development but also illustrate his basic ideas in reshaping the Chinese society.

CHU-YUAN CHENG

Ball State University, Indiana

The OAS in Transition. By M. Margaret Ball. (Durham, N.C.: Duke University Press, 1969. Pp. 721. \$19.50.)

The appearance of this important, monumental, and authoritative work on the oldest regional international organization—The Organization of American States—is much to be welcomed, particularly from the pen of such a noted authority as Professor M. Margaret Ball. In her preface Professor Ball declares that the book concerns "the organization and function—and problems of the OAS yesterday, today, and tomorrow," and candidly admits that writing about an organization in the process of undergoing a major transition "has not been a simple one" an admission any person even remotely acquainted with the complexities of the OAS will readily understand.

The author has divided her book into three major sections: (1) Evolution, Principles, and General Structure of the OAS; (2) Organs and Agencies; (3) Activities. Her arrangement of material might raise some question until one has read the book and appreciated the skill with which the separately constructed parts have been linked together.

The first chapter traces the growth of inter-American organization beginning with Bolivar's dream and ending with the signing of the 1967 protocol which drastically revised the Charter of the OAS. In her discussion of the principles underlying the inter-American system, Professor Ball points out that while such principles as equality of states, the rule of law, pacific settle-

ment of disputes, nonrecognition of territorial conquest, nonintervention, self-determination, the promotion of representative democracy, respect for human rights, collective security, and general responsibility for the raising of living standards do not differ materially from those of the United Nations or other regional organizations, nonetheless they carry more import in the inter-American system. "[T]he statesmen of so many OAS members have a philosophical bent which makes them prone to operate in terms of principle rather than on a pragmatic basis . . . [and] to view the OAS in juridical, rather than political, terms" (p. 44). The organs and agencies which implement the principles and purposes of the OAS are carefully explored, including those agencies which, although considered basic to the international life to the Western Hemisphere, have for historical or political reasons remained outside the principal structure of the OAS.

Part II begins with a study of the regular Inter-American Conferences—the former basic policy body of the OAS—and discusses the shift of this organ's functions to the newly established General Assembly. In reviewing the specialized conferences, the author points out that with the ratification of the Protocol of Amendment, the way will be cleared for substantial improvement in the coordination of the activities of the specialized conferences to prevent overlapping and duplication of effort. The Meetings of Consultation of Ministers of Foreign Affairs of the American Republics, which in fact evolved into the principal instrumentality for OAS policy determination, are evaluated by highlighting the important results of the major conferences. Professor Ball concludes that these meetings are likely to remain an important element in the inter-American picture even though they will undergo some adjustment in view of the creation of the new General Assembly.

As the heart of the new amending protocol of 1967 relates to the changes made in the composition, functions, and structure of the old OAS Council and the Pan American Union, the author makes a detailed comparison between the old council and the new Permanent Council, the old Pan American Union and the new General Secretariat, reviewing the composition, the general and special functions, the organizational set up, and the procedures of each. The new status of the technical councils, that is the Inter-American Economic and Social Council, the Council of Jurists, and the Cultural Council (now upgraded to the Council for Education, Science and Culture) is meticu-

lously surveyed. Complete coverage of the evolution, general structure, programs and activities of the specialized organizations dealing with health, women, children, geography, history, Indians, agriculture, and statistics is also included. Nor does the author overlook the other important inter-American agencies such as the Inter-American Peace Commission, the Commission on Human Rights, the Defense Board, and the Development Bank.

In the final portion of the book, Professor Ball, in a remarkably complete fashion, investigates the major activities undertaken by the OAS, including the settlement of disputes, the military and political defense of the hemisphere, the support of representative democracy, the protection of human rights, the efforts to extend suffrage, and inter-American economic and social cooperation. The sole criticism one could level at this portion of the book is the scant attention given to the extremely important work of the Inter-American Human Rights Commission during the Dominican Republic Crisis of 1965. But this tale was undoubtedly left untold not because of oversight but because in a volume of this size a halt must be called somewhere.

This scholarly study, with its masses of complex detail, is enhanced by the author's style and clarity. An excellent table of contents, a skillful index, an appendix containing the important documents of the OAS, and a twenty-nine page bibliography add to the usefulness of the book as a reference work. But its appeal is wider since it contains so much of special value to those directly concerned with or intensely interested in the international functioning of a regional system. In short, this is a good book for reading, a rewarding book for study, and an unduplicated source book for reference.

ANN VAN WYNEN THOMAS

Southern Methodist University

The Foreign Affairs Fudge Factory. By John Franklin Campbell. (New York: Basic Books, 1971. Pp. ix, 292. \$6.95.)

Few investigators of government foreign affairs organization have raised fundamental issues in as stimulating and provocative a way as John Campbell. Thus his sudden death at age 31 is a particular tragedy. His book gives new life to old but important questions, and we shall miss his clear voice in the debate over their resolution.

Campbell was a career Foreign Service officer who took a leave of absence to study the "system" in which he had served. The critique he produced has two targets. The first is organi-

zational—"excessive size and fragmented authority" (p. 229) in government foreign policy-making institutions which have confused operations and weakened State Department influence. What we have, he argues quoting Joseph Kraft, is not a decision-making instrument but a "fudge factory." The second target is substantive—an interventionist foreign policy which has exaggerated both our interests and our ability to remake the world in our image. Campbell felt that the organizational flaws were partly responsible for the substantive, and that his proposed reforms would help ameliorate both. He would strengthen State by drastically reducing its size, and by internal reforms aimed at "more clearly fixing responsibility in departmental business" (p. 233). He would give the Department "new authority over the forty-odd other agencies which play partial roles in foreign affairs" (p. 41). And he would sharply curtail overseas military, propaganda, aid, and intelligence activities. With the foreign affairs government thus dominated by men possessing a realistic (rather classical) appreciation of the nature of interstate relations, the U.S. could avoid recurrence of recent disasters.

As Campbell himself notes, his basic argument is "not entirely new" (p. 41). In championing the "old diplomacy," he cites Harold Nicolson, George Kennan, and Jules Cambon. His organizational prescription draws heavily on the report of the 1949 Hoover Commission foreign affairs task force. Thus critical reviews in the reform-minded *Foreign Service Journal* have characterized the book as "The Remembrance of Things Past" and "Metternich Revisited," and the major defense has come from retired Ambassador Ellis Briggs. Much of Campbell's critique of the "new diplomacy" seems warranted, especially in the light of Vietnam. Yet his organizational prescriptions tend to be based more on what seems intuitively reasonable than on any careful analysis of how they would affect the actual patterns of influence within government.

It is clearly plausible to argue, for example, that since many of the problems of managing the foreign affairs bureaucracy are related to the large number of officials who must be "coordinated," to have fewer officials should make things better. But assuming staffs will remain quite large, how much effect on the nature of the policy-making system will specific reductions have? Why should we expect that Campbell's 3,000-man State Department won't have many of the same problems of "elephantiasis" that the 7,000-man Department has today? And how will the personnel reduction

process Campbell advocates avoid the same pitfalls he cites in recent Johnson and Nixon Administration efforts—placing seniority over ability, or featuring interagency bargaining on an “all programs are equal” basis rather than imposition of a rational set of personnel priorities? Thus one must ask a further question—whether such sweeping condemnations of size à la Galbraith are not a retreat from serious analysis of the problems of managing complex—and almost by definition large—modern organizations.

Campbell would doubtless respond that he did not limit his proposals to reducing numbers. He also stresses organizational simplification. He calls for eliminating nine of State's present sixteen bureaus, leaving only the five geographic regions, administration, and a proposed “Bureau of Multilateral Affairs.” Yet while this would have the virtue of reducing lateral clearances, it would tend to make the Secretary of State the captive of regional, “country desk” and ambassadorial viewpoints, even more than he is today. This would scarcely enhance his acceptability as government-wide foreign affairs leader, either to the President or to his Cabinet colleagues. Yet the book does not face this sort of question.

Finally, one must question the way Campbell would reverse “the preponderance of military influence in foreign policy” (p. 197). He seems to consider “foreign policy” to be a relatively narrow and clearly delimited preserve from which military poachers must be excluded. Thus, just as the Hoover task force criticized the National Security Council for getting “into numerous matters of foreign affairs which are strictly not its business,” Campbell urges abolition of the Defense Department's foreign policy staff—the Office of International Security Affairs (ISA). Yet one has only to go down the list of major U.S. foreign policy issues today—Vietnam, China, Europe, Japan, arms control talks with Russia—to see that virtually all of them have strong military components. If the State Department really wants foreign affairs primacy, it must seek not to keep the military out of its own “diplomatic” business, but rather to get its own people more effectively into the military business. To do this, State must strengthen (not abolish) its own political-military affairs staff, and must build alliances with offices elsewhere (like ISA) which share its interest in subordinating military strategy to foreign policy.

Campbell is right in arguing that there are strong reasons for shifting the foreign policy leadership role from the N.S.C. staff to the

Office of the Secretary of State. He is wrong to suggest that a State Department responsive mainly to its own traditional values and priorities could attain either the substantive expertise or the bureaucratic clout to bring this about. His book is nonetheless valuable for the issues it raises, and for the clarity and persistence with which its major proposals are advanced.

I. M. DESTLER

Princeton University (Visiting)

De Lege Pactorum. Essays in Honor of Robert Renbert Wilson. Edited by David R. Deener. (Durham, N.C.: Duke University Press, 1970. Pp. xiii, 274. \$13.50.)

Against all odds in this type of academic ritual (essays in honor of somebody), this is a well-focused and well-edited collection of 16 essays on the law of treaties, all especially written for this volume.

The law of treaties has probably been the most “in” topic in international law in the last few years. The 1966 Draft Articles of the International Law Commission and the 1968 and 1969 Vienna Conferences on the Law of Treaties, eventuating in the 1969 Vienna Convention, have prompted a quantum leap in scholarly concern for treaties. In October 1967, two of the oldest established journals in the field, the *American Journal of International Law* and the *Zeitschrift für ausländisches öffentliches Recht und Völkerrecht*, saw fit to depart from their traditional format to put together a special issue on the law of treaties. Two worldwide bibliographies on treaties have recently been prepared: one by the United Nations Secretariat (A/CONF. 39/4, 1 Feb. 1968, 150 pp.) and the other by the Peace Palace Library at The Hague Academy of International Law (mimeo, 1968, 94 pp.). And the National Science Foundation has helped in bringing at least one part of this information explosion under control by funding the Treaty Information Project at the University of Washington.

Even viewed amidst this scholarly bumper crop, the present collection makes a distinct contribution. Its team of authors has been carefully chosen. Robert Wilson, in whose honor the volume has been published, is one of the world's leading authorities in the field of treaties and agreements. David Deener, the editor, is a widely known specialist in the relation between international agreements and domestic legal processes. All contributors, including the editor, have at one time or another studied under Robert Wilson, and have since continued and developed their teacher's interest in treaties. Deener's excellent introduction presents

Wilson's career in a sketch of how international lawyers have responded to the world around them during the last half century. Since space does not allow to review all sixteen essays I have selected the following five on the grounds that they are likely to appeal to a wider range of political scientists than just the international law fraternity.

(1) Hans W. Baade, "Constitution-making Treaties' and International Courts: Judicial Review Within the European Communities," pp. 19-39. A few specific examples show the ever-increasing complexity of the subtle interlacing of unitary, federal and international legal processes in the European Communities. The whole essay represents a giant step in sophistication beyond the centralization vs. decentralization model usually applied to the emerging European system.

(2) David R. Deener, "Treaty Norms and Statutory Norms: Patterns of Priority and Choice," pp. 73-92. This is a masterly synthesis of the interplay of domestic and international legal rules. An imaginative matrix shows all possible dyads for and against priority rule among plaintiff, defendant, court, treaty, and statute. The treatment is richly comparative. It covers constitutional provisions and scholarly writings for many more countries than are usually surveyed in writings on Anglo-Saxon jurisprudence.

(3) Conley H. Dillon, "The Role of International Labor Conventions in Economic Development in Emerging Nations," pp. 93-111. Drawing on many years of expertise on related matters, including a book on ILO conventions as early as 1942, the author now reviews the impact of ILO conventions on domestic labor standards and legislation in African, Middle Eastern, and Asian countries. He develops an intriguing "conformity score" according to which Africa leads the group scores with 90 per cent followed by the Middle East with 75 per cent and the Far East with 61 per cent. For comparison, Western Europe shows 71 per cent and Latin America 44 per cent.

(4) R. R. Oglesby, "Succession to Treaty Responsibilities in Civil Wars," pp. 185-201. This is a timely essay with clear relevance to legal-political writings on Vietnam, but it does not mention Vietnam as such. The range of examples stretches from the American War of Independence to that of Algeria. It shows that insurgents of various times and places have compromised somewhere between the "clean slate" and the "full continuity" doctrines, as is generally true for the broad interface between new states and international law and politics.

(5) Claude S. Phillips, Jr., "The Bilateral Treaty Network of Non-Western States," pp. 202-232. This is the only piece in the group that is primarily quantitative. I should declare my general bias in favor of such new methods in international legal studies and my part in supplying the author with statistical data from the Treaty Information Project. Having said this, I feel free to add that this paper would strike even a hard-core traditionalist as an imaginative piece of work with thought-provoking tables showing the comparative infrequency and the peculiar patterns of treaties among non-Western states.

The other eleven essays are by Alona Evans on self-executing treaties; Mohammed Chaudri on reservations to multilateral treaties; Robert Clute on treaties on outer space; Charles Hagan on weighted voting in commodity agreements; John Howell on reservations in dispute settlement agreements; Ralph Jones on the consensual basis of international law; Alan Leonhard on teleology in treaty interpretation; Gary Maris on leasehold agreements; Don Piper on special agreements for atomic ships; William Simpson on treaties, tariffs and cotton; and Herman Walker on U.S. commercial treaties.

All in all, this collection will be particularly useful to treaty specialists, but it will also provide new facts and thoughts for generalists in the international field. A graduate seminar or law school seminar might well be experimentally structured around this book.

PETER H. ROHN

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Economic Warfare in the Communist Bloc: A Study of Soviet Economic Pressure Against Yugoslavia, Albania, and Communist China. By Robert Owen Freedman. (New York: Praeger Publishers, 1970. Pp. 210. \$14.00.)

Economic sanctions as a weapon in international relations seldom achieve the results intended by those who invoke them and often in fact tend to reinforce or create the very situation they were invoked to change or avoid. That this commonplace proposition has also proved valid in the case of Soviet economic pressures directed toward other Communist-ruled states and that successive Soviet leaderships have learned some but not all of the appropriate lessons is the thesis of Mr. Freedman's series of case studies of economic warfare in the Balkans and the Far East since 1948.

Yugoslavia, Albania, China, and North Korea (the last is briefly considered in the chapter on China) were all in varying degrees "vulnera-

ble" to Soviet economic pressures when Stalin or his successors applied them in efforts to maintain Soviet hegemony—or later merely unchallenged Soviet supremacy—within what Mr. Freedman alliteratively calls "the Communist camp." Each of these countries was hurt by Soviet sanctions, with Yugoslavia in particular being brought close to economic collapse, but each found alternative sources of trade or aid which brought the price of resistance down to levels that determined regimes and peoples who were responsive to appeals to national patriotism were willing and able to bear.

Besides the general observation that such sanctions are never likely to succeed if the victim has or can find access to alternative markets and creditors and that they can prove dangerously counterproductive if the alternative supplier is a competing power eager and able to capitalize on its opponent's mistakes, Mr. Freedman draws our attention to several more specific conclusions to be drawn from the tale he tells.

Perhaps the most interesting of these, if only because its obviousness has been curiously overlooked by many writers in the field, is that one reason for the failure of Soviet experiments with intrabloc economic warfare was the gradualism with which Stalin escalated his economic sanctions against Yugoslavia over a period of eighteen months in 1948–49, a strategy repeated by Khrushchev when it was the turn of Albania and China a decade later. In each case, Mr. Freedman argues, this gradualism is explainable in terms of Soviet overestimation of the impact of the mere threat of sanctions and underestimation of the unity of the Communist leadership of the "target" country and the willingness of both leaders and peoples to give higher priority to national independence than to a high economic growth rate. In the Albanian and Chinese cases, he further suggests, an additional factor was a lesson learned from the Yugoslav experience, when a total blockade forced the Yugoslavs not into submission but into a reorientation of their foreign economic relations that led to enduring links with the capitalist enemy, permanently liquidating serious vulnerability to later Soviet economic pressures. With Albania and China, therefore, the embargo was never complete (credits and technicians were withdrawn, but COMECON states other than the Soviet Union continued to trade with Albania and all of the bloc continued trading at a reduced level with China), presumably to avoid forcing them into the "enemy camp" and to make the path to eventual reconciliation easier. The difference

may also have reflected the changed and more modest goals of Soviet intrabloc policy under Khrushchev, who recognized the limitations of Soviet power over states with independent Communist revolutions and who sought supremacy in place of the absolute hegemony that had eluded Stalin in this wider Communist Commonwealth.

Mr. Freedman has produced a competent and useful addition to the growing shelf of specialized case studies, dealing with specific aspects of Communist policies and societies, in which material of relevance to the subject is conscientiously collected from existing secondary sources, collated with appropriate selections from standard translation services like Radio Free Europe and *The Current Digest of the Soviet Press*, and added to an original perusal of one or more specifically relevant periodicals. In this instance the principal primary source is *Vneshniaia Torgovlia*, the organ of the Soviet Ministry of Foreign Trade.

While such studies may tell the Communist area specialist little he did not already know, they assemble, focus, and analyze a significant segment of that information in a convenient and useful form. Among nonspecialists Mr. Freedman's study will also be of interest to students or practitioners of the games people play in contemporary international politics. Such people may join Mr. Freedman in wondering "whether a high official in the Soviet Union's Ministry of Foreign Affairs, in examining the USSR's use of economic pressure against fellow communist states over the past twenty years, would come to the same conclusion as to the efficacy of economic pressure as a foreign policy instrument as did Dean Acheson," whose comment on similar American experiences he quotes: "Almost all recipients of our economic or military aid have shown us how useless threats to stop aid are in trying to pressure the recipients" (p. xv).

The book is written in a fashionably workmanlike if pedestrian style marred by occasional colloquialisms or neologisms (e.g., "re-hash" on p. xv, "factionalize" on p. 151) and inconsistent use of throwbacks to the now unfashionable phrasing and terminology of the partisan scholarship of the Cold War epoch (as in "Communist camp" and a chronic uncertainty as to whether the Council for Mutual Economic Assistance should be called COMECON or CMEA). There is also some unnecessary repetition not only of arguments but also of quotations, some of them lengthy (on p. 5 repeated on p. 39, pp. 8/131, pp. 10/125f, pp. 11/27, *et al.*).

Finally, while the temptation to deduce what a leader like Khrushchev or Tito personally thought or intended from the policies his Party or government pursued is sometimes hard to resist, especially in writing about a relatively closed society where individual roles are often obscured, to state categorically (for example) that in 1962 "Khrushchev's interest in the renewed rapprochement [with Yugoslavia] was primarily political . . . [but] Tito's reasons were more economic in nature" (p. 46) requires more justification than is offered. The same is true of the contention that "as an added form of pressure on China, Khrushchev embarked on a policy of rapidly increasing both trade with and aid to India—including substantial amounts of military assistance—in an effort to help develop India as an alternate power in Asia" (p. 149; cf. p. 139: "the Soviet Union under Khrushchev was trying to build up Indian military power to help balance the Chinese in Asia"). Tito has usually given political (or even ideological) considerations a higher priority than economic, and at the level of weakly substantiated speculation one could equally well argue that the primary motivation for Soviet aid to India was a desire to break through the SEATO link in Western "imperialism's" encircling containment or to build Soviet influence in the Third World on Indian foundations.

DENNISON RUSINOW

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The Future of the Oceans. By Wolfgang Friedman. (New York: George Braziller, 1971. Pp. 132. \$5.95.)

Professor Friedman writes that his book "... is not a neutral book." His somewhat alarmist attitude can be seen in his conclusion, on page ii of Preface, "To remain silent or passive toward such a development [partition of sea-bed and oceans] is to participate in the destruction of the principal achievements of international law in the last few centuries and to accept the preparation of an Orwellian nightmare." It is difficult for this reviewer to reach such a dramatic conclusion on the basis of the material in the book. The author disregards the fact that since its codification in the beginning of the seventeenth century, many of the original precepts of international law have been modified, and where the law proved obsolete, either completely discarded or rewritten. No basic law has been enacted on the exploitation of the sea. There was no Grotian admonition, "*cujus est solum ejus est usque coelum*," (he who controls the land controls the air above it up to the heaven) applicable to the control of the depth

of the seas, nor was there any mention made of continental shelf, slope, trench, etc. Whereas the laws governing the surface navigation of the seas developed over many centuries, exploitation of the ocean-bed is in its frontier stage.

The book, which has an index and bibliography, is divided into ten very short chapters, apparently intended to present limited summarizations of existing literature rather than to offer additional data. Professor Friedman restates arguments presented by many other publicists that the resources of the seas are *res communis* (communal property) not *res nullius* (belonging to no one), and therefore, are not within the domain of a small number of coastal states. This assumption is based on the fact that these mineral and metal deposits flowed into the seas in form of sediment carried by rivers which drained the coastal territories.

The author all too abruptly dismisses all the earlier efforts on the exploitation of ocean-bed resources. His attention is focused on the new era which began in 1945, with the Truman Proclamation of the Continental Shelf. He justifiably stresses the technological progress made since 1945 and the increased effort by several nations to exploit ever larger areas of sea floor. This trend has accelerated the danger of military and political struggle for the control of these resources. To prevent such tragedy an international agency, independent of the United Nations Organization, must be established to administer and enforce a universally acceptable set of regulations. Friedman estimates the potential deposits of minerals, metals, and food that seas could provide. He supports the program advocated by a British agricultural expert, Sir John Boyd-Orr, for the "establishment of a world food authority, that would redistribute vital foodstuffs from surplus to deficient areas" (p. 13). It is very difficult to visualize such a distribution of food produced at high cost from the seas, since food produced on land at a much lower cost is now withheld from starving nations either because they lack purchasing power or because they refuse to accept the political, military or ideological viewpoint of the donor.

The author clearly reviews present exploitation of the ocean-bed, pointing out that one-sixth of the nations could profit directly from under-water resources but only six nations can afford such luxury. Under present international law one-fourth of the nations have no claim to any of these resources, because of their geographic location. The author also indicates that ecological pollution which would result from sea min-

ing would destroy much of the undersea food supply unless new methods were discovered and employed. This inevitably would result in higher production costs and would limit the number of participating states. Present technology for the recovery of metals and minerals from the ocean-bed at depths greater than 300 meters is too costly to be practical. There is no indication that technological progress in the foreseeable future will make sea mining at great depths an economical venture.

The author believes that before any significant effort can be made to share these resources among all nations, contemporary views on territorial seas, on freedom of the seas, and on the rights of land bound states must be changed. Probably even more important, military uses of the ocean-bed must be resolved first. The Russo-American convention on the prevention of installation of nuclear or other weapons of mass destruction on the ocean-bed is an attempt in the right direction, he feels. Similarly, the establishment by the United Nations General Assembly of a forty-two nation commission to study these problems in 1968, and the United Nations Resolution of 1970, which doubled the membership of the commission and received majority support in the United Nations, are welcome signs of united activity to resolve these issues. Professor Friedman, however, does not attempt to analyze the plans presented by the United States, Soviet Union, Malta, and Tanzania.

Construction of the Mont Blanc tunnel, the Moselle canal, and organization of Air Afrique are cited by the author as examples of international cooperation to solve common problems. It should be remembered that the latter projects are insignificant in scope, legal technicalities, technology, and capital investment compared to the problems entailed in the exploitation of the ocean-bed by the international community.

Despite its many limitations, this book is a contribution to the study of the future of the ocean-bed exploitation in that it focuses the attention of the reader on the pressing need for immediate effort to resolve the issue at hand, and that it offers some suggestions for an equitable solution of the problem by the United Nations.

STANLEY S. JADOS

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The United Nations in International Politics.

Edited by Leon Gordenker. (Princeton University Press, 1971. Pp. 241. \$7.50.)

Although the United Nations is accepted as a force in international politics, what standards are to be used in determining the degree of its influence and its value, and what research is needed to aid our understanding of the past and to forecast its future? Around these questions the six essays of the book offer a theoretical framework for seeking answers that focus on the central problems of the United Nations operating to establish international peace and security. The contributions of each of the authors reflect their association with the Center of International Studies at Princeton University. Their points of view are related in a systemic approach which considers the United Nations as a subsystem within the larger system of the world political environment. Each of the individual contributions suggest areas of investigation for the student, and the book as a whole should stimulate more disciplined scholarship among those seeking an understanding of United Nations activities.

The Oran R. Young chapter on "The United Nations and the International System" offers a theoretic framework for exploring various roles and functions of the United Nations Organization. The Organization is presented as a regulator in providing a minimal level of stability; as an effector of great-power agreements; as a partisan political instrument; as a force for political change; as a creator of long-term viability; and, as a creator of norms. In pursuing this analysis Young notes the shift in membership patterns and the significance of these shifts in explaining the underlying axes of political contention in the international system. These axis patterns and their variations, he concludes, indicate a "contextual fluidity" of the international system which offers both pitfalls and opportunities to the United Nations Organization in the roles it is called upon to play.

S. J. Michalak, Jr. provides a comparative outline of the relationships between the behavior of the universal international organizations in the realm of peace and security, and the systemic conditions within which they operate. His comparisons of the behavior of the members of the League of Nations and of the United Nations offers hypotheses for analysis in which the variables indicating homogeneous or heterogeneous policy development operate in conditions of bipolar, multipolar and bi-multipolar systems. His theory seeks to determine which variables are most crucial in exploring the context and differentiations among member states acting in the world system.

One difficulty in the study of United Nations

activities is indicated in the G. R. Lande chapter, "An Inquiry into the Successes and Failures of the United Nations General Assembly." That difficulty lies in using the printed or recorded statements of position, or votes of states, in committee and plenary meetings as a basis for revealing the complex interrelationships among states. Is agreement or noncompliance with U.N. recommendations the result of discussion within the U.N., or do factors outside the U.N. in the international system determine the attitude and actions of states? The Lande study appraises 29 resolutions of the General Assembly to determine which "proved successful or partially successful with respect to both implementation and effect and which were abjured and had little or no consequence" (p. 108-9). The framework for analysis is excellent in providing for consideration of methods, procedures, and the behavior of actors in the United Nations context. But, as pointed out in the conclusion, the most telling factor in revealing the efficacy of the General Assembly is the extent of the efforts made by states to explore a variety of courses of action that might best ease the tensions between opposing parties and enlist cooperation from all concerned. Such an analysis requires the complete United Nations record. The written record may be too summary, and the full tape recordings are available only in the original language. The more difficult problem, however, lies in achieving an awareness and appreciation of underlying related issues, often in situations involving other subsystems of the international system. Nonetheless, both the Lande study and the essay by L. B. Miller, "International Organization and Internal Conflicts—Some Emerging Patterns of Response," provide the sort of framework that warrants extensive use in evaluating United Nations activities.

Leon Gordenker emphasizes the political importance of the administrative processes through which international organizations contribute to the adjustment and pacific settlement of conflicting interests. His chapter, "The United Nations and Economic and Social Change," describes the provision of institutionalized means available through the expanding activities of the family of United Nations organs. It presents a general appraisal of the influence that international organizations can exert. Here also, the book clearly shows the need for more such studies to document the viewpoints offered. Fortunately this need is beginning to attract the attention it deserves. The recorded data of United Nations mission, sur-

veys, Secretariat reports, and development consultants' project reports, along with the personal experiences of international and national civil servants in debriefing sessions, can add the dimension needed to indicate the relationships between the peace-building activities in economic and social fields and the peace-keeping responsibilities of the Organization.

Richard A. Falk in the concluding chapter offers "a very preliminary effort to evolve a disciplined procedure for assessing the degree to which the United Nations has met, is meeting, and will meet the needs of international society" (p. 188). His view of the Organization combines descriptive analysis with normative appraisal. He outlines two alternative systems to be tested in identifying the autonomous and semi-autonomous roles of the United Nations. The analysis reflects the gap that separates the United Nations Charter goals and the limited capabilities of the actual United Nations system operating in the international environment. His models in a Charter System and a United Nations System offer a basis for a realistic assessment of the roles of the Organization, recognizing that the assessment must reflect patterns of political influence and must be correlated with implementing capabilities.

"The United Nations in International Politics" offers a real challenge in emphasizing the need to examine critically and perceptively the mass of information generated by the United Nations and to search other relevant sources. The challenge is defined in presenting a comprehensive systems analysis as a framework in which to bring to bear all the talents and skills of a multidisciplinary approach to international politics. Its suggestions for further research should enable us to develop the standards for judging the roles and actions of the United Nations and provide forecasts of future roles for the Organization.

WILLIAM J. BRUCE

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The Stable Crisis: Two Decades of German Foreign Policy. By Wolfram F. Hanrieder. (New York, Evanston, and London: Harper & Row, 1971. Pp. 221. \$3.50.)

In the 'fifties the expression "stable instability" was used to characterize the domestic policy of the French IVth Republic (e.g., by André Siegfried in 1956). Wolfram F. Hanrieder uses the slightly modified term to characterize the foreign policy of the Federal Republic of Germany, and he has indeed coined an impressive and illuminating formula. In his earlier book,

West German Foreign Policy, 1949–1963: International Pressures and Domestic Responses, Hanrieder had disclosed his inclination and his talent for conceptualizing the foreign policy of the Federal Republic. His new book covers a longer period and is more of a narrative, but it is clearly different from Waldemar Besson's book (*Die Außenpolitik der Bundesrepublik: Erfahrungen und Maßstäbe* [Muenchen: R. Piper Verlag], 1970), which appeared almost simultaneously.

In three chapters Hanrieder outlines the evolution of the three most important goals of the Federal Republic's foreign policy: security, political and economic recovery, and reunification. In pursuing the first two goals Bonn's expectations were not always fulfilled (as in the Multilateral Force Project), and serious controversies even within the government party could not be avoided (for example, those between the "Atlantic" and "Gaullistic" wings). Nevertheless, the story of the efforts to attain these goals is on the whole a true success story; the third chapter, however, dealing with the "supreme" goal only reports failures. Yet success and failure are most closely connected, for (as Hanrieder suggests) the security pursued was a "Western" security, the rapid economic recovery was a "Western" recovery; these achievements thus necessarily excluded a successful solution of the reunification problem, a solution which had to be an "Eastern-Western" success. A fourth chapter, "Foreign Policy and the Domestic Political System," concentrates on the connection between the dilemma in foreign policy and the domestic political system. The question could be asked: Which domestic forces established priorities in foreign policy with a view to which model domestic system—that is, which forces preferred security and recovery within the framework of Western policy to reunification within the framework of an understanding between East and West? Hanrieder does not present the possible answer that the German bourgeoisie placed security first in order to reconstitute the capitalist system, because this answer evidently does not correspond with historic reality. Despite some criticism of Adenauer's policy as being too concerned with small European-Catholic countries, nearly all critics (except the small Communist party) agreed with the Chancellor's maxim that reunification should not be achieved at the expense of security and political-economic recovery. None of the important groups placed reunification "above everything," and the Social Demo-

cratic Party, too, rejected a neutrality which would have surrendered a reunified Germany to the pressures and interventions of the neighboring superpower.

In other words, the Federal Republic's decision in favor of the West was not a party decision and cannot as such be reduced to particular social interests. All controversies concerned methods and, at most, degrees, and for this reason the dilemma in foreign policy was a dilemma of West German society as such, and not only the dilemma of a single class or party in this society. Therefore, in this chapter, Hanrieder is on precarious grounds, for he neglects the fundamental consensus in favor of the differences of opinion, so that the Social Democrats appear less "Western" and the right wing of Adenauer's government coalition, especially the small "Deutsche Partei," more nationalist than they really were.

With regard to the present, Hanrieder describes an antagonism which is in no way basic or even ideological, and which cannot clearly be associated with political or social groups: the opposition of "traditionalists" and "reformers" (p. 182). The "traditionalists," according to Hanrieder, stick to the old demands for reunification and its implications, e.g., the Hallstein Doctrine, while the "reformers" want to conduct a "flexible and dynamic" foreign policy on the basis of the status quo now firmly established by international political developments. The shift toward reform of foreign policy was initiated by the Christian Democratic Union Foreign Minister Gerhard Schroeder, continued by the government of the Grand Coalition, and it is now being completed by the Brandt-Scheel government. Hanrieder leaves no doubt that he considers the "New Eastern Policy" necessary and right.

I doubt, however, whether Hanrieder's categories suffice for an understanding of the complexity of possibilities and tendencies which, especially now, are immanent in German foreign policy. It might perhaps be better to distinguish between "accepters" and "nonaccepters." Some of the accepters obviously accept the status quo only because they want to change it with the help of new methods in order to reach the old goal of reunification under Western auspices. Others have put up with the status quo because they consider it essentially unchangeable. A third group may accept the status quo because it is regarded as an intermediary stage on the way to a Communist reunified Germany. On the other hand, some of the "nonaccepters"

subscribe to Adenauer's view that by taking advantage of the changing world political situation, and with the backing of a united Western world, it might one day be possible to force the Soviet Union to surrender her German satellite state; for others, revisionism is only the Federal Republic's most advanced line of defense; and a third group feels bound to the principles of international law and democratic ideology. Which tendency or which combination of tendencies will finally gain the upper hand cannot yet be foreseen; at any rate, contrasting "traditionalists" to "reformers" does not do justice to a complex reality. The most interesting and most surprising tendency is doubtless the Communist one, because, although for some time hidden behind demands for academic reform, it succeeded spontaneously in the most active sector of student youth. Hanrieder cannot be reproached for not having noticed in 1969 in the United States what in 1971 does not really seem credible even to the general public of the Federal Republic. But one day it will not be possible to overlook the fact that the genesis of a strong "Party of the East" among the student youth in the model affluent nation of Western Europe was the most surprising event in the history of the Federal Republic, and that in the long run this development might have extraordinary significance especially with regard to foreign policy.

To summarize: Hanrieder's book is readable, concise, and stimulating; the analysis and narrative are well balanced; and furthermore, it contains a very useful bibliography of about two hundred titles. It leads the reader to the threshold of the immediate present of German foreign policy. The book appeared too early to take into account (except for a casual remark on p. 195) that "re-ideologization" which has not yet reached the level of big politics. There exists now, however, a definite possibility that by the acceptance of the status quo, the "stable crisis" of this foreign policy will not turn into a stability free from crises but into an unstable crisis wavering between fundamentally different possibilities of orientation. In his final paragraph Hanrieder seems to express a similar doubt when he asks whether, in the long run, pride in economic achievements can be a substitute for a "genuine national sense of purpose" (p. 198). Hanrieder appears to expect something like a new nationalism, perhaps in the style of that presumptive coalition of Christian Social Union and National Democratic Party to which he alludes on p. 195. Yet politi-

cal thunderstorms, we have learned, often come from a direction quite different from the expected one.

ERNST NOLTE

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American Imperialism in the Image of Peer Gynt, Memoirs of a Professor-Bureaucrat. By E. A. J. Johnson. (Minneapolis: Univ. of Minnesota Press, 1971. Pp. 336. \$12.75)

This intellectual autobiography is more memoir than interpretation. It summarizes the public experiences of a sensitive, imaginative, intelligent, and highly literate scholar with a tough will and a searching mind. Its title and its first chapter announce its intention to provide a general critique of American foreign policy in its current imperialist mood. But while its criticisms are accurate and cumulative, they are also fragmentary and impressionistic.

The very disingenuousness of Professor Johnson's theme makes a greater impact than do many more pretentious efforts at explaining and reconstructing American foreign policy. Johnson explains American imperialism as neither a coherent misguided theory nor a vile conspiracy against weaker peoples, but as the work of bunglers, incompetents, and minor sinners. American imperialism (which is nowhere defined in the book) appears as an accident of power. In this context Vietnam becomes a venial sin, not an unjust war. If the same sinners responsible for Vietnam also successfully reconstructed Europe and salvaged Korea, it was because America's intention is generally to do good, and because some good technicians and administrators are able to rise above the weaknesses of casual policies, interfering interests, and foolish organization styles.

This view of American foreign policies scarcely permits the emergence of much moral indignation: that passion Johnson reserves for the Nazis, Senator Joe McCarthy, and a few small-minded bureaucrats like the military authorities who denied him the Legion of Merit and gave him the Bronze Star instead (page 127), and the Yugoslav officials who kept hedging about a house their government was supposed to have provided for him when he was assigned to the AID mission there (pp. 249-50). Syngman Rhee earns contempt for his grand and petty thieveries, and so does the Cosmos Club for denying membership to Carl T. Rowen. Some of Johnson's recommendations were stupidly overruled or ignored, and this fact, too, is the occasion for indignation:

his strong position against letting the free market allocate rice during an anticipated food shortage in Korea and his efforts to reduce the swollen and indolent American mission to Greece in 1961 in order to supplement its hard-pressed and overworked counterpart in Yugoslavia, for example. He still resents being forced to "doctor" some statistics in order to justify an otherwise unjustifiably large shipment of wheat to Yugoslavia in 1952, merely to satisfy an insistent farm lobby. His scorn for the incompetencies and stupidities of American decision making in foreign policy is still fresh: for the stop-and-start funding processes that prevent programs from developing momentum and coherence (pp. 195-96), and for the powerful bureaucratic rivalries and disagreements that studded the decisions that came his way (pp. 223-24).

But this scorn is not Olympian. Johnson is not a raging Timon of Athens; his ambitions and perceptions are not Faustian; he sees not Machiavelli but Ichabod Crane at the tiller of U.S. foreign policy; the American presence in world affairs is Innocents Abroad, not conquering Caesar. Indeed, the Peer Gynt that Johnson sees is Ibsen's pathetic figure, not Grieg's brooding one. American imperialism has the soul of a button; it is not a "sinner in the larger sense," it is a "tragic non-heroic hero," a "clump of nothing" that "could not endure to have his ego 'shrunk to scale.'" Its unrest is that of a wanderer of the earth "trying to discover what it is he ought to be or do." Heedless, unguided and untrained, without great purpose or malice, then, America let a succession of events that accompanied power grow into an inadvertent imperialism culminating in Vietnam, a venture which is mentioned only once in the book (where it is described with the adjective "unfortunate").

The examples specified as "Gyntism" make it clear that though the offenses may be small, the consequences can be great: allowing vengeful thinking to interfere with necessary planning for the feeding and reconstruction of Germany; greedy, ignorant, and opinionated American military government officers who condoned involuntary sabotage of political reconstruction after the War (page 147, 167); corrupt overpaying of American contract advisers that only swelled the American presence abroad (p. 187); the mindless compromises that nullified principle (p. 158); individual cowardices and defalcations of duty (p. 216); and the repeated deceptions and concealments about the role of the U.S. government in postwar benevolent activities, especially in education, that under-

mined the credentials of foundations and private scholars everywhere (p. 286); together with the racism even of liberal elites, as revealed in the Cosmos Club incident, that discredited the American intentions everywhere in the Third World (page 304). "Gyntism" is not merely an American syndrome, of course: the French were also guilty of it in their wartime collaboration and their postwar confusion about punishing it (p. 138). Indeed, it is scarcely a syndrome at all: it is merely human weakness.

Is it enough to explain the ambitions and shortcomings of American foreign policy as human weakness? Within the limitations of human capacity among policy makers, are there not possibilities for an institutional rationality that will offset collective inadvertence and smallmindedness? Or, if even posing such a possibility demands too much of any Peer Gynt, can we not assess the causes of what psychologists used to call ineffective behavior? Stanley Hoffmann, in *Gulliver's Troubles*, defines similar facets of American character systematically enough to permit us to distinguish among the varieties of Gyntism of different nations. Many scholars of foreign policy have suggested how the processes of decision making, the perceptions of decision makers, and the coexistence of incompatible political forces, can explain some of the political aspects of foreign policy. In recent years, efforts to explore the bases of individual and social morality have applied psychoanalysis and philosophy to the problems of international aggression and misunderstandings. Quantitative analysis of both conflict and peaceful advantage seeking has also helped explain international events. But it is doubtful if an individual's memoir, however thoughtful and eloquent (this is both), however strategically based (this is an overseas view of administrative problems and achievements, and thus a useful counterweight to the Olympian view from Washington) can provide enough leverage upon experience to reach these hard questions. Nor can Mr. Johnson's professional skill provide the means of surmounting his data (he is an economic historian so dedicated to his craft that when he describes his boyhood chores on a farm in Illinois he justifies his immigrant parents in a footnote citing a study of child labor in Sweden).

The memoirs are an important source-book for a generation that did not experience the excitement and purpose of the postwar reconstruction and the first decade of foreign aid. It contains compelling vignettes of the military government and of the confident spread of

faith in U.S. technological and economic capacity for modernization. It suggests the sense of self-fulfillment the American scholars in the postwar world have experienced when their advice and skills were sought and accepted, and it reveals as well their weaknesses as decision makers and administrators. Moreover, in the end Professor Johnson is probably right: there was more good than evil intention in these postwar manic decades of American activity. America's failures partake of the tragedy of mediocrity more than that of malevolence. Peer Gynt is an apt simile. But in this book it is only a simile.

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Europe's Would-Be Polity: Patterns of Change in the European Community. By Leon N. Lindberg and Stuart A. Scheingold. (Englewood Cliffs, N.J.: Prentice-Hall, 1970. Pp. vi, 314. \$7.95 cloth, \$5.50 paper.)

Political and economic developments in Western Europe which have been subsumed under the rubric of regional integration are among the most thoroughly researched and intensively analyzed of postwar phenomena. Responding to this profusion, Lindberg and Scheingold offer "not simply a book about the European Community, but also a book about theories and theorists of European integration and of international integration processes more generally" (p. v). The authors also choose to shoot at a moving target—to delineate patterns of change upon which to base projections about the Community's future. This ambitious endeavor, which they hope will meet "a need . . . for a theoretical and conceptual framework capable of providing an overall perspective on the often conflicting theories, descriptions, predictions, and evaluations" (p. v) of the European system, has produced a mixed result.

To this reviewer, it does not offer a fully satisfactory overall perspective (assuming one is needed), nor does the model of systemic change seem to overcome its tautologies sufficiently to afford a useful conceptual tool for future prediction. The Preface recites that the authors will construct "some analytical models to help us understand how and why changes in this political system occur." When the key model of system change is later presented, the reader is told that it "says nothing about how or when the posited changes might occur (i.e., the conditions and circumstances that cause the individual variables to vary)" (p. 115). Well, one might ask, why not? Is an equation which omits how and when changes might occur a

model of change? The equation and its variables cannot be fully (or fairly) explored in a brief review, but a footnote explains that models are only steps along the road to theory, and simply consist of "abstract statements that posit relationships between variables that may or may not conform to reality" (p. 106).

A more significant dimension of *Europe's Would-Be Polity* is its contribution to an understanding of the integration process. In the postwar years, the problem of whether to rebuild or transcend the European nation-state was resolved by a compromise based upon the common commitment to economic stability and prosperity. The Community as it evolved in the 1950s and 1960s and remains today, the authors properly stress, has largely been the result of political and technical elites and is still largely economic and "consequently rather obscure" (p. 22). What has emerged, they state, is a would-be polity. To be sure, the nation-state is still predominant, but "its almost total monopoly has been clearly broken" (p. 75), and it has managed to cope with the evolution of the Community system in such fashion as to allow integration to proceed in economic matters while reserving to itself such domains as military security policy.

For Lindberg and Scheingold, political integration is defined by the scope and institutional capacity for decision making. Measured against its own record since it began, the European Community *has* achieved a significant level of political integration, and the authors look to the patterns in the past ups and downs of the Community to build their alternative models of system change for purposes of future projections. The decisions which produce change are the outcome of a process of coalition-formation, whose mechanisms include functional spillover, side-payments and logrolling, actor socialization, and feedback. Related elements include demand flow, systemic support, interest-group pressures, the style and power of political actors, and the amount and kind of leadership available. A number of "alternative process models" are offered for purposes of analyzing systemic change, and detailed case studies are then presented to illustrate how the European Community experience has included such models as forward linkage (agriculture), output failure (transport), equilibrium (the general customs union), spillback (the coal sector), and systems transformation (successful with the birth of the common market, unsuccessful with respect to British entry in the 1960s).

The tendency toward tautology seems to re-

appear in the case studies. For example (and in contrast to the case of agriculture), the inability to achieve a common transport policy is traced to poor Commission leadership and to a demand flow inadequate to generate the mechanisms of coalition formation, the latter because governments and relevant political actors did not so perceive their interests that a common policy was possible. In this case study, the authors emphasize the failure of the Commission to use the potential which (in their view) was available to build a coalition for a common policy, however modest. The Community's pattern of success and failure seems to be explained as follows: if there is sufficient demand flow and leadership, coalition formation and a common policy will result (agriculture); but if both are lacking, there will be failure (transport). Here again one might ask, Is this a meaningful insight or a not very theoretical runaround? And even if this explanation is not tautological, does it permit projections concerning *future* systemic change?

Lindberg and Scheingold conclude with an assessment, based upon their analytical framework, of prospects for continued Community growth. They admit that there is no practical way to use their model to predict numerous possible influences (such as international developments, unforeseen crises, or changes in government or ruling elites) which might introduce fundamental changes. Hence either forward linkage or spillback is equally possible, depending upon circumstances. We are given a European Community which will continue to grow in the short run, but which will gradually reach an equilibrium—based upon coordination and harmonization of *national* policies more than upon a common Community policy process. Future leadership, systemic support, the flow of demands, and institutional capacity are unlikely to change in such fashion as to trigger striking growth. The authors persuasively argue that neofunctionalism has just about reached its limits, rejecting the spillover theory and arguing that initial satisfaction with systemic growth leads not to new demands for broader decision making but to a conservative concern for protecting what has been achieved, i.e., equilibrium. "Ironically, then, the success of integration tends to retard growth" (p. 292).

Although in the end the authors admit how limited is their "ability to make exact projections about the Community's future" (p. 303), they are quite certain that it will stop growing "well short of the point where we can talk of federal or pseudo-federal structures" (p. 305). What, then, are we left with? Lindberg and

Scheingold conclude that we shall be faced with "an unprecedented, but curiously ambiguous 'pluralistic' system. . . . Indeed, there seem to be no satisfactory models or concepts in the social science vocabulary to adequately describe it" (p. 306).

Thus, with respect to the future, *Europe's Would-Be Polity* does not present more than a clouded crystal ball. Indeed, since the authors wrote we have witnessed what may be a remarkable movement toward an expanded Community, with a projected economic and monetary union by 1980, an agreement on the desirability of British entry and further expansion of membership, and even a rudimentary step toward political union with the inauguration of periodic meetings looking toward policy cooperation in foreign affairs. Whether strengthening and expanding the Community can simultaneously be achieved remains to be seen; what is genuinely impressive in terms of traditional international relations is not that European integration does not proceed quickly, but that it proceeds at all. Even if a United States of Europe should not even remotely evolve, the Community *is* (as a member of the Commission recently observed) "an entity in its own right, not merely in constitutional terms, but also with respect to its position in world politics." Lindberg and Scheingold do not satisfy the desire for predictive powers (could anyone?), but they do demonstrate that a system which has experienced as many patterns of change as has the European Community must be at least a would-be polity, if not a real one.

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India's China War. By Neville Maxwell. (London: Jonathan Cape, and New York: Pantheon Books, Random House, 1970. Pp. 475. \$10.00.)

India inherited her ill-defined northern borders with China from the British Raj, and attempted to deal with them in the traditional British manner. The policies failed because India lacked Britain's resources and was confronted with a different kind of China.

Duncan Sandys, who headed a British mission to India in November 1962, was reported by Galbraith (*Ambassador's Diary*, p. 495) to believe that "the whole argument with China [was] over a few acres of Ladakh which he would have the Indians surrender." Had India been willing to negotiate the conflicting border claims, would Peking have agreed to yield NEFA in return for the Aksai Chin ("desert of

white stones") area, and the trauma of the 1962 war been avoided? Whereas India's strategic interest in NEFA is indisputable, it is difficult to discern any tangible value in her possession of Aksai Chin.

In his comprehensive examination of these disputes, Neville Maxwell provides the basis for an informed answer to this and related questions. Posted to India in 1959 as the South Asia correspondent of *The Times*, he found that for the next three years the dispute was the "staple of my work," and a continuing interest until his departure in 1967, as well as subsequently at the London School of Oriental and African Affairs. He draws heavily upon secondary works for historical background. For the contemporary period he relies upon readily available documents and others that remain classified, as well as upon numerous interviews with both Indian civilian and military personnel who were intimately involved. Although the book is formidably "backnoted"—899 times to be exact—many of his sources for recent information are understandably held in confidence. Aside from the irritating necessity of turning to the back of the book for the references, and the virtual impossibility of discovering the referents of many of his numerous "*op. cit.*'s," the book is remarkably free of similar defects.

Originally intended as a rewrite of his dispatches to be included as a section of a book on India in the 1960s, the project, Maxwell soon realized, required "something much more full, fundamental and searching. . . ." *India's China War* is the product of his "subsequent reappraisal" (p. 12). After an "Historical Introduction: The Limits of Empire," (45 pages) the story unfolds in five generally chronological chapters: "Collision Course" (105 pages); "The Forward Policy" (86 pages); "The View From Peking" (33 pages); "The Border War" (120 pages); and "Ceasefire" (30 pages).

Had the author set out to vindicate the Chinese to the disadvantage and discomfiture of Indians he could not have done a better job. Yet one has the distinct feeling that this was not his intent, that his reappraisal led him, perhaps with some reluctance, to tell it as he believed it honestly was. The account is substantially as follows: Prior to 1947, none of the contested areas had been delimited by valid international agreements. Of some eleven different lines drawn by the British in the Western Sector, only one had even been proposed to China. The MacMahon Line in NEFA, never agreed to, and in fact specifically repudiated by all Chinese governments, had been approved by Tibet, but only on the understanding that it was

part of an arrangement, never fulfilled by the British, by which the latter would obtain Chinese consent to virtual Tibetan autonomy. Needless to say, the Tibetan right to negotiate an international agreement was at best dubious. Both areas remained practically unoccupied (except by tribes in NEFA) until this century, when the British, and subsequently the Indian government, began to move into the NEFA area and (after 1950) the Chinese into Aksai Chin. Presented with a number of alternative boundaries for their Western claim line, the Indian Government chose the most ambitious of the former British lines, but one that seems to have been regarded by London principally as a bargaining point for possible future negotiations. It was not until 1957 that the Indian Government, through an article in the Chinese press, became aware that the Chinese had constructed a road through Aksai Chin.

India maintained that there was no border dispute because her claim was incontestably valid, and hence there was nothing to negotiate about. The Chinese, on the other hand, exuding sweet reasonableness, recognized the existence of a dispute and were willing to settle it through negotiations. Moreover, throughout the conflict, Peking indicated a willingness to agree to the Indian claim to NEFA. It was logical for the Indian government to conclude that the Chinese would also yield elsewhere.

But, in fact, by ruling that the remaining stretch of the northern borders should be regarded as a "firm and definite" alignment, "not open to discussion," Nehru had taken the step which was to transmute a boundary problem into a dispute, and the dispute ultimately into a border war (p. 81).

Perhaps it is open to refutation, but Maxwell presents strong evidence that the war was unnecessary, that the responsibility was primarily India's, and that had the issue been negotiated before it became a matter of national pride and honor, India would have gained the NEFA area and lost Aksai Chin, the latter of no tangible value to her.

The account of the Indian domestic scene, of the roles of Nehru, of Menon, and of Major-General Kaul and his "Kaul boys," is fascinating to one interested in Indian personalities and politics. So is the treatment of the strategy and tactics of the war which, despite the sad performance of the Indian forces, offered instances of courage, skill, and wisdom. Perhaps the one benefit that India derived from the conflict was a more realistic assessment of her military forces and the improvement that took place prior to the 1965 conflict with Pakistan.

Those interested will find perhaps the best in-

formed of the Indian reviews of this book (unanimously unfavorable) in K. Subramanian, "Neville Maxwell's War," *Hindustan Times*, October 18 and 25, 1950; the review also appears in the *Journal of the Institute of Defense Studies and Analysis*, III (October 1970), 268-91. Those firmly convinced and unwilling to consider any other possibility than that India was the innocent victim of an unprovoked Chinese attack should avoid this book.

CHARLES P. SCHLEICHER

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The Art of the Possible: Diplomatic Alternatives in the Middle East. By Michael Reisman. (Princeton, N.J.: Princeton University Press, 1970. Pp. 161. \$6.00.)

This book is an exhortation and a proposal. It exhorts the United States and the Soviet Union to cooperate in preventing the Arab-Israeli conflict from setting off a world conflagration. The proposal is for arrangements establishing "minimum order" in Arab-Israeli relations. The assumption underlying the proposals is that at present "peace" between the Arab states and Israel is unattainable. Instead, diplomacy should aim at the more modest and more realistic goal of "minimum order."

The arrangements proposed by the author amount to a comprehensive territorial settlement, with built-in guarantees in the form of international participation in machineries and institutions designed to separate the parties. The scheme calls for the return of Sinai to Egyptian sovereignty, accompanied by neutralization through the establishment of an internationally administered Sinai Development Trust. The future of the West Bank is to be decided by a series of referenda, which would provide an opportunity for the Palestinians to exercise the right of self-determination. The Golan heights would be set up as a Druze trust territory, administered by Israel under UN auspices. Jerusalem is to remain under nominal Israeli sovereignty, with a system of effective international guarantees for the rights of different religious communities.

The objections likely to be raised against these proposals, stemming from the interests of the parties concerned, need not concern us here. The proposals ought, however, to be evaluated in the light of the goal and principal criterion which the author has set himself: the goal is "minimum order," and the main criterion to guide the design is realism.

This reviewer tends to agree with the author that "peace," in the sense of normal neighborly relations between the Arab states and Israel, is

at present unattainable, and that the more immediate diplomatic objective ought to be to diminish the likelihood of renewed conflict. The problem is how can this be achieved, and how can processes be set in motion which would tend to reinforce and stabilize a system of minimum order.

The system of minimum order outlined in this book is to be brought about by the cooperative efforts of the United States and the Soviet Union. In setting the background for this cooperation the author draws an oversimplified symmetric relationship between the Soviet Union and Egypt on the one hand and the United States and Israel on the other. He lists some important common interests shared by the United States and the Soviet Union, and suggests that these can be made to prevail over their competitive interests. The Arab states and Israel are assigned the role of reluctant clients, who will behave in accordance with the guidelines laid down by their respective patrons. The entire scheme depends not merely on a momentary Soviet-American agreement on the terms for a settlement, but on continued concert between them, and a long-term insulation of the Middle East from competitive power politics. I wonder whether this accords with the "realistic" and "achievable" goals that the author says are required.

Another weakness of the scheme is that in at least two out of the four major proposals, the argument seems to be inconsistent with the purpose of minimum order. One concerns the West Bank. After presenting a good argument for permitting the Palestinians on moral grounds to exercise self-determination, the author continues by discussing the security advantages of his proposal. Here his argument is that if commando activity by West Bank Arabs against Israel continues, "the existence of a Palestinian state from which attacks originated would provide a legitimate target and would render the threat of Israeli retaliations an effective deterrent . . ." (p. 52). One is left to wonder whether a "minimum order" advocated in these terms would constitute an improvement over the past and the present.

A more fundamental internal contradiction is raised by the author's proposal for a Druze trust territory in the Golan. The author implicitly acknowledges that for the time being the Druzes have not given rise to a movement claiming self-determination. The author says that "if our era is indeed characterized by the demand for political self-determination of discrete groups, then it is predictable that the Druze, at some point in the future, will begin to

make such a claim" (p. 65). This hardly is a good reason, however, for inducing such a movement among them—as this is the effect that the establishment of a Druze trust territory would undoubtedly produce. Nor is it clear how the arousal of Druze irredentism in Israel, Syria and Lebanon, where none exists for the time being, would promote the goal of regional order and stability which the proposal is designed to serve. Indeed, the author's argument on this proposal seems to go against his own views of the preconditions for peace, as expounded in Chapter VI of the book. In that concluding chapter of the book the author laments that "no leader in the Middle East has been able to rise above his ethnicism, tribalism, nationalism . . ." and states that "the real problem in the Middle East is neither territorial nor ideological; it is the continuous inculcation and reinforcement of parochial subjectivities that cannot tolerate coexistence with others" (pp. 86–87). The stimulation of Druze political demands is not likely to facilitate the transformation of values and disappearance of nationalistic attitudes which the author deems desirable.

There is merit in the author's argument that such highly intractable problems as the Arab-Israeli conflict indicate "the need for social and political invention" and for "an unfettered investigation of new political techniques and legal institutions, tooled to goals of minimum order and the particular exigencies of the context in which they must be realized." Had the author undertaken a systematic investigation such as he recommends, the book might have advanced this purpose.

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Nullity and Revision. The Review and Enforcement of International Judgments and Awards.

By W. Michael Reisman. (New Haven and London: Yale University Press, 1971. Pages xvi, 900. \$25.00.)

Employing the policy-oriented perspective and the analytical framework developed by Professors Myres McDougal and Harold Lasswell, Dr. Reisman has examined in exhaustive detail the problems of revising, nullifying, or giving effect to the awards of international arbitral tribunals and the orders and judgments of international courts. That part of his monograph concerned with the doctrinal debate on finality or reviewability of arbitral awards, and with claims of nullity made with respect to arbitral agreements and awards, was originally presented in 1965 as a doctoral dissertation at the Yale Law School. Two additional years

were then spent in expanding the study into its present comprehensive form. It now includes three major parts.

Part I, "The Anatomy of Authority," is a theoretical study of the relationships of authority (defined as shared group values which provide standards) and control (i.e., devices available to secure desired behavior in others) as they affect the reception of international arbitrations and adjudications. Reisman rejects the artificial model of the past which posited the finality of awards and judgments on the basis of prior acceptance by the parties of an obligation to be bound by them. He presents abundant evidence of a far more complex and equivocal set of relationships running through the processes of compromissary agreement, the arbitral or judicial process (including definition of relationships between state claimants and injured nationals for whom they stand as surrogates, characteristics of the several types of tribunals, here labeled "cogentive," "dispositive," and *ad hoc*, and their procedures), and the review of awards and judgments. Review is to be understood not merely in the sense of a judicial reopening or reconsideration of cases but of the broader political-social processes which determine how far the judicial decisions become effective (in truly Lasswellian diction, ". . . who, with what objectives, with what means at their disposal, how manipulated, where and with what effects?" [p. 15]). As he observes, international awards and judgments frequently only mark the beginning of renewed diplomatic and political effort to achieve a modified basis of settlement. There has been little enough realistic study in such terms of the extent of political-social acceptance of judgments of national courts. In the international arena the problem is even more complicated. The international community usually lacks institutions for direct review and enforcement, although it may be able to muster indirect controls reflecting a sense of the requirements of "minimum world public order." It cannot avoid reopening the tribunals' work to scrutiny by each national community affected, with all that implies of competitive diplomatic and political forces. Reisman develops, on the basis of much historical evidence from past cases, heuristic models of the sequence of procedures from *compromis* through arbitration or adjudication to the steps taken to give effect to or to modify or annul the award or judgment. These he hopes may assist the international decision makers to assess more realistically at each stage the probabilities of effectiveness and to manipulate controls available to them so as to contribute to it.

His analysis of these processes is the most thorough and useful with which I am familiar, although it owes a good deal to some earlier writers, notably Carlston (a debt Reisman acknowledges). Its perspectives rest strongly in sociological and realist jurisprudence. Its materials include a wide range of arbitral and judicial opinions, diplomatic instruments, and doctrinal writing, which he has subjected to careful analysis and sharp judgments. Unfortunately, especially as some of the presentation is quite abstract, Reisman lacks a feeling for succinct style and plain, unpretentious language; the section is therefore laborious although profitable reading. Fortunately the remainder of the book achieves a somewhat simpler, more direct style.

Part II, "Claims of Nullity," consists of two long chapters which examine the grounds of nullity asserted with respect to compromissary agreements and arbitral procedures, respectively. The first necessarily opens a good deal of general treaty law, as it applies to the validity of submission to jurisdiction in terms of competency to negotiate or invoke state responsibility, coercion of parties, national requirements for ratification, domestic jurisdiction limitations, state succession to obligations, unilateral abrogation or other termination, interpretation of agreements with respect to exhaustion of local remedies or pendency of litigation elsewhere, estoppel, and other matters. With respect to grounds of nullity in the arbitral process, Reisman rejects *a priori* limitations upon the scope of review such as exclusion from it of "errors of fact" or those "errors of law" conceived not to be "essential error." Such limitations he considers inconsistent with the fundamental purpose in review of contributing to the effectiveness of the decision and maximizing its authority. He is critical of doctrinal literature favoring such limitations or attempting definition of them. Accordingly, he examines a broad range of grounds of nullity: these include competency and propriety of conduct of arbiters and judges as evident in the procedure of appointment, participation in hearings, consultations, and decisions, disqualification to avoid possible prejudice, and corruption; geographical factors affecting the capacity of parties to present evidence; crises which affect the acceptability of decisions; considerations of equity; the propriety of *non liquet* judgments to avoid unacceptable consequences of judicial legislation; failure to accord right of hearing and representation and to compel discovery of evidence by adverse parties; coercion especially as it affects exhaustion of local remedies. Reisman

has ransacked the literature of international arbitration as a basis for discussion of these topics; his erudition is impressive. Without suggesting agreement with his critique of every case he discusses, I can testify to his incisive analytical powers and consistent effort to relate his analysis to the principles developed in Part I. This section is a substantial professional contribution of interest particularly to international lawyers.

Part III, "The Process of Enforcement," begins with a general discussion of principles which should govern enforcement of international awards and judgments. In the absence of an "international sheriff," Reisman looks to a varied "functional enforcement." This does not exclude such controls as use of force or mustering the moral suasion of the international community, but he makes little of these as practical resources for general use. He relies mainly upon greater ingenuity and flexibility in reaching substitute assets to satisfy awards. He reviews in detail enforcement theories and practices in international organizations: the League of Nations under Article 13(4) of the Covenant, with a useful review of the Optants Case; the United Nations, with consideration of possibilities under Article 94(2) of the Charter in the light of the problem of enforcing interim measures ordered by the International Court of Justice in the Anglo-Iranian Oil Case; the Specialized Agencies, with a suggestion that the International Banks and the Monetary Fund might control assets of depositor-states to satisfy judgments against them; and the regional organizations of Europe, Latin America, and Africa, which now differ greatly in the institutionalization of enforcement through national agencies. Reisman then moves to enforcement of international awards and judgments by national executive agencies and national courts. Here he examines the implications of the decision in the case of the *Monetary Gold Removed from Rome*, and reviews doctrinal literature and recent cases with respect to the impact of the doctrine of sovereign immunity upon enforcement by national courts of foreign awards. The section concludes with discussion of the circumstances in which national self-help may still be appropriate to vindicate international legal rights when other authority fails to secure them. Part III is certainly the most systematic, unified discussion of enforcement of international arbitral and judicial decisions available to us. While the relevant material is not novel, it is assembled and focused upon the problem in a manner not hitherto undertaken.

The discussion is valuable for students of both international law and international organization.

Dr. Reisman's research has been prodigious, his analysis is searching, and his conclusions throughout are persuasive and significant. His work merits widespread professional attention.

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The Law of Treaties, A Guide to the Legislative History of the Vienna Convention. By Shabtai Rosenne. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1970. Pp. 443. \$25.00.)

This is not a treatise on the law of treaties. It is a documentary guide through the complex process which, between 1962 and 1969, produced the Vienna Convention. For the work of the International Law Commission on this project between 1949 and 1962, when J. L. Brierly, Sir Hersch Lauterpacht and Sir Gerald Fitzmaurice were its successive rapporteurs, the reader is referred to the 1962, 1963 and 1964 reports submitted to the Commission by Sir Humphrey Waldock, the final rapporteur.

For all purposes of academic research and governmental or judicial inquiry into the long series of debates, compromises, and votes by which the eighty-five articles of the Convention were hammered out, the guide is priceless. It is another major contribution to the study and development of international law by an author who combines dedicated scholarship with exceptional practical experience and who has been since 1962 an active participant in the work of codification.

In a 66-page introduction, Rosenne emphasizes the supreme importance of this part of the United Nations program for development and codification of the law of nations. What is involved is the law of international contract, and the law of contract is, as he puts it, "the very heart of any coherent legal system." The decision insisted upon by Sir Humphrey Waldock when he accepted the role of rapporteur was that the goal should be not a mere model code but a convention binding upon states. The decision meant that the rules to be formulated must be adaptable to the multiple purposes that treaties serve in the relations of states and to radical changes in the nature and scope of the problems to be met. This called for abstract drafting, which increased the importance of adequate provision for dispute settlement; failing this provision, abstraction could mean excessive subjectivity, not to say license, in interpretation and application. Of the two most stubborn po-

litical battles in the entire process of negotiation, one was that between advocates and opponents of compulsory arbitration or adjudication. The other was fought over the question of universal participation in the convention. How a group of African states worked out the "package deal" that saved the conference from shipwreck on these reefs is a stirring part of Rosenne's introduction.

The author takes an optimistic view of the culmination at Vienna. He concludes that "a project of codification, which the International Law Commission had initiated twenty years previously, was completed." How much does this mean in view of the fact that eighteen states, including the entire Communist bloc, abstained in the final vote, that France voted No, and that by December 31, 1970, only Canada, Jamaica, Nigeria, Syria, and Yugoslavia had ratified or acceded to the Convention? Can the task really be considered complete until the text has been generally ratified? Perhaps the author would say that, ratified or unratified, the Vienna Convention will stand as the supremely authoritative statement of the law of treaties and will be relied upon as such by judicial and arbitral tribunals. It has already been cited in the Advisory Opinion on Namibia handed down by the International Court of Justice on June 21, 1971.

P. E. CORBETT

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Stilwell and the American Experience in China, 1911-45. By Barbara W. Tuchman. (New York: Macmillan, 1971. Pp. xv, 621. \$10.00.)

As commander of U.S. forces in China, Burma, and India and Allied Chief of Staff to Chiang Kai-shek from 1941 to 1944, Joseph Stilwell was the central American figure in the first phase of the tragic relations between the U.S. and China. This period has been treated by many authors in different ways. Theodore White's *The Stilwell Papers*, a best-seller, consists of selections from Stilwell's wartime diary. Subsequently, this period of Stilwell's career was covered with deep insight by Herbert Feis. It was examined authoritatively and with meticulous care by Charles Romanus and Riley Sunderland. An evaluation of Stilwell's programs and actions in the context of an interpretation of American policies and Chinese politics was attempted in the reviewer's *America's Failure in China*. In terms of methodology and historiography, this book represented a political scientist's self-conscious endeavor to give the central place to the explicit and systematic analysis

of the assumptions and structure of policy in the writing of diplomatic history while using the traditional form of historical narrative as a supplement. But Stilwell as a person and his early life are not the subject matter of these works.

Barbara Tuchman's full-length biography fills this important gap. It is written with the style of a master of popular history and character sketches. It evokes once again strong emotions of sadness, an intense sense of despair, and an oppressive feeling of doom in those who have lived through or studied this period of history. It enables us to see how Stilwell's personality, his early experiences, beliefs, and likes and dislikes shaped his outlook and activities during the Pacific War.

The theme of Tuchman's book is "the relations of America to China, in a larger sense to Asia" and "the vehicle of the theme is the career of General Stilwell" (p. xi). Thus an interpretation of this relation is a major thread running through the whole book. During the Pacific War, the American approach to China is characterized by Tuchman as the "can do" spirit, using the motto of the 15th Infantry Regiment (stationed in Tientsin in the 'twenties) in which Stilwell served. In the 'fifties and early 'sixties, the writings on both current problems and failures of American policies during and after the war were also characterized by a search for more effective policies, approaches, and methods to use American power to effect American purposes in a "can do" or "could have done" spirit.

The Vietnamese war since 1965 has challenged this spirit and its underlying assumptions. It has raised three interrelated questions: First, can the U.S. ever understand the problems of Asian nations? Second, can American goals in Asia ever be attained? Third, can American policies and actions ever have any effect on the political development in Asia? Tuchman's book reflects this drastic change in perspectives, but she has not formulated her conclusions and assumptions as carefully as many other scholars who share these new perspectives.

With regard to the first question. Tuchman correctly rejects the previous naive tendency to believe that China can be understood in American terms and judged by American standards—a tendency which many of us have examined and criticized at length. Unfortunately, she swings in the opposite direction of the nineteenth-century obscurantist position that the East and West cannot really understand each other. It is one thing to affirm the importance

of cultural differences. It is quite another thing to assume the validity of popular clichés about these differences, to use them to explain conflict, and to conclude that they prevented two nations from ever understanding each other.

Thus, Tuchman allows facile generalizations about cultural differences between the U.S. and China to mar her narrative on the conflict between Stilwell and Chiang Kai-shek at important junctures of her story. She refers to the "fundamental cultural clash between Chiang's and Stilwell's theories of war"; the different forms which conscience "stemming from Judaeo-Christian tradition" takes from that rooted in "the Confucian tradition"; the Chinese "cultural imperative" of hoarding resources; the Chinese tendency to manipulate foreigners; Chinese cynicism, etc. (pp. 250, 266, 276, 288, 303, 332, 412, 490).

Yet Tuchman herself and other scholars before her recognize that at stake in the irreconcilable conflicts of policies and interests between Chiang and Stilwell was nothing less than Chiang's dominant position and the future development of China (p. 502). On this crucial question at least, there was no mutual misunderstanding due to cultural differences. On the contrary, the intensity and irreconcilable nature of the conflict arose from fairly shrewd guesses about each other's attitudes and an almost perfect understanding of the consequences of the policies pursued by the other side.

It is also clear that the intense conflict between the U.S. and China arose not so much from conflicts of national interest in the abstract as from the actual and potential impact of American policies on China's internal politics. This is conclusively shown by the facts that Chiang's political rivals demonstrated great enthusiasm toward Stilwell's army reform program and that the Chinese Communists expressed a willingness to serve under Stilwell. In return, Stilwell entertained a high opinion of them, in contrast to his utter contempt for and hostility toward Chiang.

Stilwell's experience in China also suggests the decisive importance of internal politics in the relations between states in another way. If one side in the conflict succeeds in exploiting the internal divisions in the other side, winning over the most influential actors or helping build a triumphant coalition favorably inclined to its viewpoint, it will prevail. In his conflict with Chiang, Stilwell had the firm and consistent support of Marshall and Stimson. But Chiang won over many influential American officials to his side. Subject to conflicting advice and cross-pressures, Roosevelt never acted decisively ei-

ther by backing Stilwell to the hilt or by transferring him on many occasions of serious conflicts prior to September 1944.

When his hand was forced by the military crisis in China in the Summer of 1944, Roosevelt adopted *in toto* the Stilwell-Marshall program, including a demand to put Stilwell in command of all Chinese forces. But he was never fully convinced of the correctness of his course of action. When he was confronted with Chiang's threat of a rupture of the alliance, and when urged on by Hurley, he backed down.

The history of Sino-American relations and Tuchman's latest account of them once again remind political scientists of a lack of an adequate theoretical model for the conceptualization of these relations. Recently, Graham Allison has developed three models of foreign policy as rational choices, outputs of organizations, and outcomes of bureaucratic bargaining. But even this sophisticated formulation is not complex and comprehensive enough to serve as theoretical models for the analysis of China's foreign policies in this period, unless we interpret his third model of bureaucratic bargaining very broadly and loosely. In China, foreign policies pursued or advocated were functions of political struggle among various independent or semi-independent political groups which, in contrast to those in Allison's model, did not occupy definite positions in a hierarchical system and did not bargain merely along regularized channels. The pace, structure, and rules of the game, if these existed at all, emerged out of the process of struggle itself and changed constantly. The power of the political groups came ultimately from their control over armed forces. Not only did their internal power position in a fluid political situation determine their views on foreign relations; relations with foreign powers also had direct effects on their internal power position. This type of foreign-policy making which has occurred throughout the ages has always been well known to diplomatic historians, but it is incumbent on political scientists to formulate a conceptual model for it. After the decisive factor of internal politics has been taken into account in the construction of the model, we can then proceed to construct a model of greater complexity by including the cultural variable.

With regard to the second question of whether American goals in China during the Second World War could have been achieved, Tuchman's answer is perceptive and has serious implications for current American policies toward Asia. Obviously influenced by the lack of decisive results of the massive American efforts

in Vietnam, Tuchman has come to stress the limits of American power more heavily than many of us did at an earlier time. She believes that piecemeal reform was impossible and that even if the U.S. had adopted Stilwell's policy of making American aid conditional on Chinese performance, the result would still have been nil. "Stilwell exaggerated the power of the quid pro quo because he did not have it" (p. 308). But underneath, Stilwell knew that to reform the Chinese system, "it must be torn to pieces" (p. 381). Not surprisingly, Stilwell was one of the first to draw the correct conclusion when he wrote in August 19, 1945, five days after Japan's surrender, that "we ought to get out—now" (p. 522). But the whole history of American policy since 1941 shows that it has been easy to make commitments but extremely difficult to "get out." Today, "getting out" of the involvement in the Chinese Civil War would be even more difficult than at any juncture since 1945.

If Barbara Tuchman is correct in her conclusion that "China was a problem for which there was no American solution," her rhetoric has probably carried her too far when she writes that "in the end, China went her own way as if the American had never come" (p. 531). This answer to the third question posed above is less than adequate. It is easy to see that what the U.S. did or failed to do inevitably affected the external relations and internal development of China even if the results were not those intended by the U.S. When the U.S. failed either to force Chiang to undertake the necessary reforms or to discard him, when she failed either to intervene effectively or to withdraw completely from China, when she failed either to sustain the non-Communist forces in China or to reach a *modus vivendi* with Chinese Communists, and when she neutralized the Formosa Straits, crossed the 38th parallel, and rushed headlong to the Yalu River, she in effect guaranteed the emergence of a violently anti-American Communist regime in China, made it necessary for the Chinese to be dependent upon Moscow for a fairly long period of time, and contributed to the eventual destruction of the political influences of pro-Western, moderate, non-Communist Chinese. To affirm that the U.S. opted for the worst possible alternatives in the years between 1941 and 1950 does not imply that American policies did not limit China's options and China's own choices in both internal and external affairs.

Tuchman's conclusions and assumptions on America's relation to China and Asia reflect powerful intellectual currents released by

America's frustrations over the Vietnam War. It remains to be seen to what extent these intellectual currents will become the basis of official policies. Whether these assumptions and conclusions are correct or not, only another encounter with destiny will tell. But even then, the test will not be conclusive. If there are firm lessons which we can learn from the totally unexpected and tragic outcome of Sino-American relations in the past thirty years and the rapid and drastic shifts in our interpretations of that history, these are the uncertainty in human affairs, the unpredictability of historical development, and above all, the need for intellectual humility.

TANG TSOU

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Survey of International Affairs 1962. By D. C. Watt. Issued under the auspices of the Royal Institute of International Affairs. (London, New York, Toronto: Oxford University Press, 1970. Pp. 558. \$14.50.)

Political scientists and international historians will long be indebted to the Royal Institute of International Affairs for sponsoring the series of annual surveys of international affairs and to D. C. Watt for composing the present volume which encompasses world events from varying dates in 1961 to the end of January 1963. The arduous task of transforming raw data into patterned history or political analysis is of course best realized if the working scholar is allowed to transgress the bounds of a short period so as to see events in the light of previous years, decades or centuries. In few fields is the time factor more crucial than in international relations. For the essence here is the continuous interpenetration of multiple foreign policies that are disparate not only because they emanate from different power centers and cultural traditions, but also because they are projected onto time in greatly various ways. Thus, whereas the process of foreign policy making has been essentially short-term in twentieth-century American history (a view receiving considerable support from Mr. Watt's presentation), that of the Soviet Union is cast in long-range terms, allowing for the protracted realization of certain basic and enduring concerns. And this means, of course, that the former can be integrated more persuasively in an annual survey than the latter.

In the face of these difficulties it is no small tribute to the scholarship and talents of Professor Watt that he was able not only to chronicle a year dense with internationally significant events but also to shape the fluid re-

lations he records by isolating themes and locating major junctions between quite separate, often starkly conflicting policies. Furthermore, he has rendered his version of the interactions in eminently lucid, readable prose.

The core of the work is a thought-provoking, well-aimed, often scathing critique of the Kennedy Administration's "Grand Design" for the restructuring of the Atlantic Community. This part, together with the chapter on "The Financial Policies of the Western Powers" (written by Michael Donelan), deserves to be widely read especially in light of Britain's entry into the Common Market in 1971 and the continuing disarray of monetary policies. Conceived on the model of the Marshall Plan, which was "only imperfectly understood in the U.S." (p. 137), the Grand Design was doomed to fail according to Watt because it was built on certain purely hypothetical propositions, namely that Britain and other E.F.T.A. powers had in fact entered the Common Market, that the E.E.C. was about to transform itself into a political community, and that having done this, it would be content to be a junior non-nuclear partner, ready to leave the responsibility for its ultimate defence to the nuclear deterrent power of the U.S. (p. 112). Preoccupied with the military rather than the political uses of their armed forces, the Americans, notably the President himself, were in Watt's view so mesmerized by the fear of nuclear proliferation that they missed out on the vast task of transforming superficial contacts with their European allies into meaningful communications. Although the author notes that "negotiation was not President Kennedy's personal forte" (p. 13), he squarely identifies the major culprits in this sad story as the President's ideology-ridden advisors (with McNamara and Rostow singled out for special rebuke), who were ignorant of elementary diplomatic techniques, preoccupied with unfolding the "Design" in rhetoric rather than methodical planning, and allowed by their Chief to indulge in rival, mutually contradictory initiatives.

The major illustrations here cited are the failure to perceive the organic connection between policies toward the Alliance on the one hand and the crisis of confidence in the dollar on the other; the mishandling of the Skybolt missile project which revealed the Administration's "inability to reconcile the methods of its strategic and defence policy with the goals of its foreign policy" (p. 160), and the inept treatment of the arms control and disarmament proposals. A study of the latter, said to have included virtually every idea that had emerged

from discussions of disarmament since the interwar years, leads Watt to a poignant critique of the "American penchant for mixing the legalistic with the idealistic in their approach to international affairs" (p. 26)—reaffirmed in a subsequent chapter by references to this country's emotional, populist, quasi-parliamentary view of the UN—and to the general conclusion that public understanding of the nature of negotiation had not improved much since 1929 when Lord Robert Cecil complained that American delegates come with precise instructions, usually made public before their arrival, and that any variation from plan is then likely to be regarded as a diplomatic defeat for the U.S.

The bias in some of these discussions is decidedly British, as when Watt writes (p. 122) that Britain should have been the interpreter of the new Europe—a view that lays the author open to the same charge of unrealistic hypothesizing that he levels against the Americans. For after noting that Macmillan harbored bitter anti-American sentiments while priding himself on his ability to manage the Americans (p. 164), that Canada and Britain were the weakest among America's allies in NATO (p. 61), and that Britain and the U.S. pursued quite different, indeed rival schemes of solving the financial crisis, Watt himself deplores the "disastrous misjudgment" of the British in the Skybolt affair, as well as the "unfortunate penchant" of his countrymen for seeking their own peace of mind by suggesting concessions on the part of those whom their government is pledged by treaty to aid (p. 173). Likewise he is himself highly critical of Britain's uninterrupted industrial succor (in the form mainly of Black Sea oil deliveries) to Cuba during January-August 1962, all the more so as this did not net the U.K. any diplomatic advantage. Further, in regard to the Cuban situation he notes explicitly (p. 106) that Britain no less than France was preoccupied with tripping up her allies rather than her enemies.

Having followed these altogether convincing strictures on the subject of Anglo-American relations, one finds it difficult to acquiesce in the author's uncompromising indictment of France and Germany as sole perpetrators of all that went wrong in the year. For the major thesis of the book has it that 1962 ushered in *the* great Soviet offensive, targeted in particular upon Cuba and Berlin, and that it ended as a triumph for de Gaulle and a resounding defeat not only of Kennedy but also of Krushchev. It is here in the treatment of Communist strategy and continental Europe's reactions to Anglo-

Saxon and Russian policies that the Survey's own Grand Design disintegrates. For one thing, Soviet policy does not lend itself to the kind of easy personalization possible in respect of the U.S. or France. After all, Krushchev himself had proclaimed long before 1962 that all Soviet leaders are Stalinists when it comes to foreign affairs. His verbal behavior vis-à-vis the Atlantic nations or Communist China may deserve such epithets as "lurid" and "wild" (it is even described as "Faustian") by comparison with the measured cadences of Heath or McNamara, but in the traditional terms of Leninist polemics it is more properly viewed as an exercise in purposeful statecraft. Next, in the context of the Soviet operational code which enjoins tactical withdrawals and shifts of offensive from one scene to the other it is doubtful, especially in light of what has happened in both Cuba and the Germanies in the last nine years, that Krushchev's Russia was really defeated. Watt himself points out that Krushchev was at that time planning a step-by-step exacerbation of the conflict centering on Germany, Berlin and "The Wall" so as eventually to crack the American president's nerve (p. 34), and also that Cuba may have been a means to force the U.S. to yield on Berlin. It is thus disconcerting to find him say: "As to the reasons for the Soviet obsession with Berlin they can only be the purest speculation" (p. 58), and to castigate Adenauer for provoking a well-meaning Krushchev who "hoped for a rapprochement with West Germany and resented Dr. Adenauer's reaction to the Cuban crisis" (p. 74).

Parts II and III of the work, consisting of separate essays by Richard Gott and John Major on what is summarily called "The Third World," are of uneven quality. The piece "China defeats India" (Gott) is noteworthy for its excellent use of contemporary news items and policy statements even though it was surely doubtful even then that "the losses to China's prestige were immeasurable" (p. 426). Another major chapter on "The Sub-Systems of the Third World" has only two entries, the break-up of what then existed formally as the "United Arab Republic" and Africa south of Sahara where the discussion deals almost exclusively with the Union of South Africa, Portugal and Southern Rhodesia. A very useful index concludes this richly informative volume.

ADDA B. BOZEMAN

Sarah Lawrence College

International Claims: Postwar French Practice.

By Burns H. Weston. (New York: Syracuse University Press, 1971. Pp. 224. \$10.75.)

It would be a distinct loss to the political science profession if Professor Weston's book were studied only by specialists in international law or comparative government and politics.

Issued as the ninth volume in the *Procedural Aspects of International Law Series*, it undertakes a comprehensive and systematic probe of eight claims commissions established by France since World War II to develop and administer policy for the distribution of lump sums received as compensation from Poland, Czechoslovakia, Hungary, Yugoslavia, Bulgaria, Romania, Egypt, and Cuba. The author's translation, examination, and comparison with U.S. and British practice of processes and actions of the claims commissions, together with interviews with government officials and private practitioners, provide the reader with rich and reliable data and insights. Professor Weston, who teaches at the University of Iowa Law School and serves on the Executive Council of the American Society of International Law, is neither neophyte nor dilettante as he strives to clarify our understanding of contemporary mechanisms and institutions for coping peacefully with nationalization and other takings of foreign business and property.

He is as concerned with process as with substance. For example, he is troubled by the aura of secrecy that surrounds the work of the French claims commissions, even though the details he discusses and criticizes offer something of a testament to the capacity of a resourceful scholar to pierce successfully the bureaucracy's penchant for secrecy. With regard to substantive policy, he is especially critical of overemphasis by the commissions on recovery of allegedly lost profits. He finds scant support in historic practice for such dedication to protection of evanescent profit interests. He regards initial expectations of profit as a relevant item for inclusion in a list of claims but rejects payment by the commissions of estimated lost profits "as a matter of right" (pp. 113-114).

Professor Weston is equally critical of the circumscription of redress for claimants wrought by embodiment in the claims system of the simplistic adage that "without legal capacity to play the benefactor, no decision maker can be expected to give satisfaction" (p. 152). He finds that claimants have often been denied commission benefits simply because the controlling texts governing the commissions' powers have made distribution impossible. Sometimes settlement agreements failed to authorize indemnification for claims raised, and at other times, as with debt claims brought to the Bulgarian and Romanian commissions, the

French Government chose to exercise its "diplomatically prescribed prerogative to distribute the lump sums according to its own dictates" (p. 153). Given the reasonable assumption that many claimants would not have undertaken to petition the commissions at all had they been aware of such limits, "one must wonder about the adequacy of notice on the part of the French government" (p. 154).

Professor Weston's volume is as rewarding for the general political scientist as it is for the international law or comparative politics specialist; for his analysis of claims practice eschews narrow or purely legalistic components in favor of a sophisticated examination of the multiple interrelated contexts in which claims are asserted. In probing commission processes and practices, he traces the successes and tribulations of a modern instrument of conflict resolution. The international claims commissions of yore having become museum pieces, the period since World War II has had as its hallmark for the peaceful resolution of claims resort to the lump-sum settlement/national claims commission device.

The international legal order today, as in the past, has a primarily horizontal structure. It has minimal hierarchical command and enforcement structures. Accordingly, Professor Weston believes that

however much traditional notions about sovereignty and nationalism may be on the wane, the nation-state remains the primary repository of international legal authority and control, with most effective decisions being taken on a government-to-government plane among formally separate and equal bodies-politic (p. 1).

This recognition of reality should not, however, generate a self-fulfilling cynicism about international law. The author remains convinced that international legal significance can be distilled from practices of domestic decision-making institutions. Horizontal decision making can and does have "very real legitimacy... for the maintenance and growth of world public order" (p. 2). Although he relies primarily on international lawyers to implement his call for "energizing the horizontal possibilities for assuring and expanding world order" (p. 190), Professor Weston's articulate faith and meticulous scholarship deserve a wide audience among political scientists.

VICTOR G. ROSENBLUM

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The Politics of Trade Negotiations between Africa and the European Economic Community—The Weak Confront the Strong.

I. William Zartman. (Princeton, N.J.: Princeton University Press, 1971. Pp. xi, 243. \$8.50.)

This pioneering book will interest students of African and European diplomacy as well as those primarily concerned with theories about the process of bargaining and negotiations. The author has undertaken a difficult comparative study of several sets of trade and aid negotiations among the six members of the European Economic Community on the one hand, and four groups of African states on the other: the eighteen African states associated with the EEC; the three East African states of Kenya, Uganda and Tanzania; the three Maghreb states of Morocco, Algeria and Tunisia; and Nigeria.

Although the book deals with much economic material it is essentially a political analysis of how "the weak confront the strong." The author emphasizes the process of negotiations rather than its results. He contends that African states have a kind of power that enables them to win benefits despite their "weakness," but he tries to avoid overstating the case. He also endeavors to make a contribution to the development of a "comprehensive theory of negotiations" but recognizes that his contributions have not led very far toward this end.

After a background chapter on the Treaty of Rome, four chapters tell the story of the disorderly and complex offers and counteroffers the European and African states made to each other in negotiating the Yaoundé Convention between the Six and the Eighteen, and the related treaties between the Six and seven other African states. Each of these four narratives concludes with a helpful "analysis" in which the author sets forth the conclusions he draws from the relevant negotiations. He suggests that in negotiating the Yaoundé Convention, "the African states greatest strength was their weakness" (p. 64). Their underdevelopment was the basis for their appeals as well as their excuse for not offering greater concessions. By putting their fate in the hands of the Six, where it was anyway, they increased their chances of achieving more than if they had tried hard bargaining when they had "nothing to bargain with." As this chapter also shows, the Six were so severely divided among themselves that when they did reach agreement in their own separate negotiations, they presented a tight package to the Africans, who had only the options of accepting the package or rejecting everything. France's hard line on maintaining its position in its former territories caused most of the friction.

The Africans, unfortunately, were also often divided, thereby weakening their position further. The Eighteen tried to focus on principles rather than details, but the Six soon "turned to working out the details among themselves, thus letting the details determine the principles rather than the reverse" (p. 62). The Eighteen could only warn of the dire results if they were left impoverished, and attempted to enforce Europe's obligations by repeatedly citing the voluntary commitments of the Six to continue the Association.

For both economic and political reasons, Nigeria had more success than other African states in negotiating with the Six. It was a potentially large market for both trade and investment, and it would be a valuable symbolic Associate since it was not only Africa's largest state but was politically moderate. In addition, Nigerian negotiators were skillful, and they had the advantage of acting alone rather than as part of a divided group of states. The East African states, on the other hand, had a much smaller appeal as a market and in the end could only take what was given them.

The Maghreb negotiations took six years (1963-1969) before even limited agreement was reached. The situation of these Maghreb states—Morocco, Algeria, and Tunisia—was more complex in one sense because they were on the Mediterranean, the southern flank of the Community itself, and their products competed directly with the produce of some of the Six. Italy, for example, insisted on excluding fruit, vegetables, and olive oil from the negotiations until the Common Agricultural Policy of the Six had been decided; Italy also required special treatment for cork. The position of Morocco, Algeria, and Tunisia was also weakened by their failure to negotiate jointly, a point the author elaborates at some length.

The fourth of the author's case studies of negotiations deals with the operations of the "Eurafrican" institutions set up by the Yaoundé Convention to deal with such matters as the problem of "origins," which, incidentally, the author does not explain as clearly as he might have. The chapter also covers the negotiations leading to the renewal of the Yaoundé Convention, signed on July 29, 1969. Though generally satisfied with the aid aspects of Association, Africans expressed many and repeated grievances over its trade aspects. The new Convention only "smoothed out the details" of the old one and left Africans dissatisfied. It produced "neither a vertical integration, nor a Commonwealth-Yaoundé African Association, nor a continental or subcontinental free trade zone, nor

an effective price and market guarantee, nor a working marketing organization, nor even a successful preferential area" (p. 197).

The author's final chapter is a contribution to theory. Distinguishing between the study of diplomacy and the analysis of negotiations, he notes that the great writers on diplomacy—Thucydides, Machiavelli, de Félice, de Galilières, Pecquet, Nicholson—simply suggested how diplomats should behave, while the diplomatic historians only described this behavior in specific cases. In Zartman's view, even contemporary students of diplomacy (Morgenthau, Aron, Lall, Ikle, Fisher) have also sought to establish, or confirm by new methods, prescriptive principles for successful diplomacy. This "principles approach," whether it be power principles or behavioral principles, "provides useful lessons for diplomats, but little in the way of analysis of the negotiating process (how outcomes are attained)" (p. 202).

The author also believes that recent efforts to analyze bargaining through game theory and matrix methodology are inadequate for the analysis of negotiations because, *inter alia*, many positions in most negotiations cannot be expressed as quantifiable items. Game theory can help to determine whether to negotiate or

not, but it is not applicable to the analysis of the process.

Zartman suggests that the main question in analyzing negotiations is, "How are divergent viewpoints combined to produce a common agreement?" (p. 222). His answer is what he terms the "alternative/convergence approach to the study of negotiations." Divergent positions may be combined (1) by limiting alternatives in some of four different ways, and (2) through the convergence of positions by five possible means. The details of this are too complex for elaboration here.

The author concludes with a brief application of his theory to these Eurafrican negotiations. In summary, he tells us that weak states can make the most of their weakness by effectively provoking encounters, by putting forth their needs, and by the very act of accepting what is offered them. The results achieved by the weak are thus quite limited, which may help to explain why the author focuses primarily on the analysis of the negotiations process rather than on the evaluation of its results.

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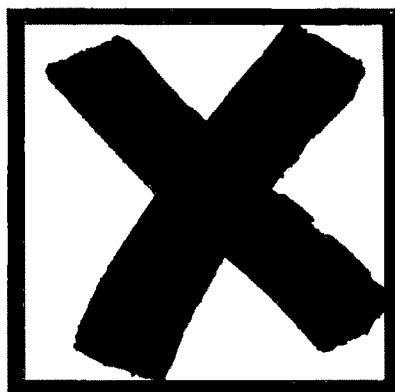
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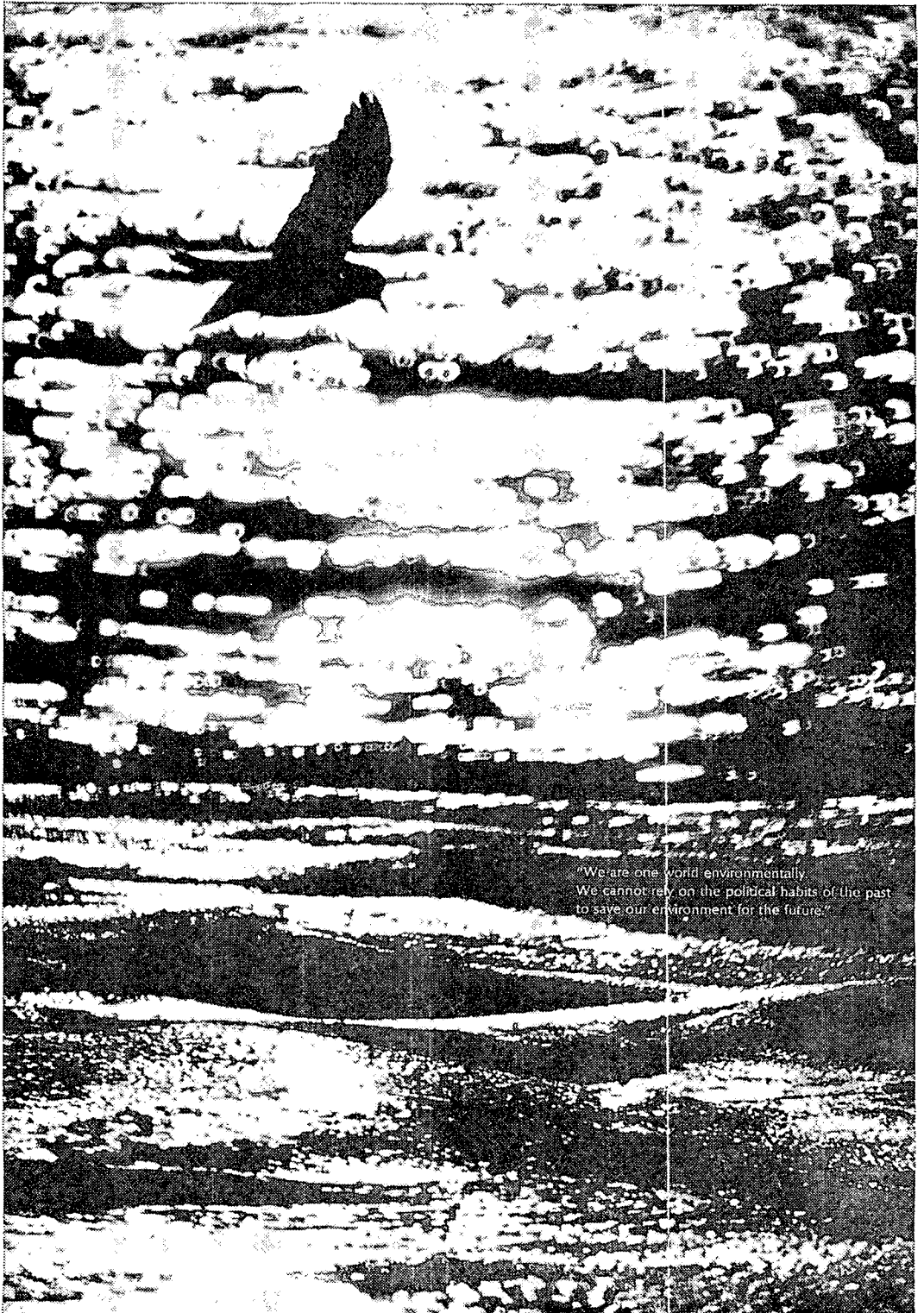
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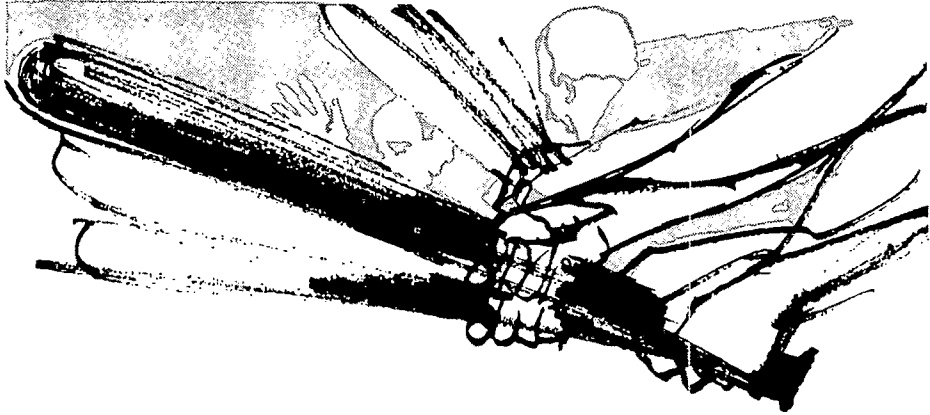
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